

American Political Science Review



MARCH 1994 VOLUME 88 NUMBER 1

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The American Political Science Review (ISSN-0003-0554) appears quarterly. It is published by the American Political Science Association, 1527 New Hampshire Ave., N.W., Washington, D.C. 20036, and sent to all members. Dues: Regular Members with gross calendar income: under \$30,000, \$55; \$30,000-\$40,000, \$75; \$41,000-\$50,000, \$85; \$51,000-\$60,000, \$90; over \$60,000, \$95; Student Members (limited to 5 years), \$25; Retired Members (who have been members 25 years) with gross calendar income; under \$25,000, \$20; \$25,000 and over, \$40; Life Members, \$2,000; Institutional Members: Domestic, \$141; Foreign, \$157. Dues allocated for a subscription to the APSR: \$17 for individual members; \$110 for institutional members. Changes of address sent to Membership Secretary of the APSA. Postmaster: Send address changes to: APSR, 1527 New Hampshire Ave., N.W., Washington, D.C. 20036. Second-class postage paid at Washington, D.C. and at additional mailing offices. Copyright © 1994 by the American Political Science Association.





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ARTICLES

LIMITS OF POLITICAL STRATEGY: A SYSTEMIC VIEW OF THE AFRICAN AMERICAN EXPERIENCE

Presidential Address, American Political Science Association, 1993

LUCIUS J. BARKER

iewing the African American experience through a systemic perspective tells much about the nature and operation of the political system. I suggest that a systemic perspective incorporates a broad range of factors that must be considered in a constitutional context that guides not only the choice of goals and strategies, but also signals the relative chances of success in the outcome of interest conflict. I focus on the capacity and limits of various strategies, and show how the political system generally militates against fundamental policy change. However, on rare occasions such policy change does occur, but it is difficult to implement and maintain, especially regarding the matter of race. Under the circumstances, I suggest that overcoming America's continuing dilemma of race requires extraordinary leadership and people of good will who are determined to see the constitutional—democratic system work.

I ow to deal with what Tocqueville and Myrdal long ago referred to as the "Negro problem" remains a dilemma for our American polity. Indeed, the overall problem of race and color continues to plague the political system and appears intractable to solution (Bell 1987). It is also apparent that the problem will not fade away; nor can it be long forgotten or ignored. Thus, to address the problem of race and color is to address the nature and problems of American politics.

The overall effort here is to show how these matters can be most profitably addressed through a systemic perspective. Specifically, I shall suggest that a systemic perspective incorporates a broad range of factors (economic, social, cultural, institutional, political, and legal) and that in the development and implementation of public policy, the complex of relationships among these various factors must be considered and balanced in a constitutional context that not only guides the choice of goals and strategies but also signals the relative chances of success in the outcome of interest conflict.

Viewing the African American experience through a systemic perspective might tell us a great deal about how the nature, structure, and operation of the political system facilitates, impedes, or otherwise affects the realization of the democratic promise. That promise, as seen by African Americans and as reflected in the formal contours of our Constitution and laws, is the fair and equal opportunity for each person to develop to full potential, consistent with the clear recognition of the identity, worth, and respect that is due to every person by virtue of their status as human beings.

The focus in this essay is on the African American experience since 1950.² Over the ensuing 15 years, we see a convergence of developments (including pathbreaking court decisions, a broad-based Civil Rights movement, and the enactment of major civil rights

legislation), whose interactive and combined effect spurred the kind of recognition and protection of basic rights that had long been denied to African Americans. Developments such as these dramatically exemplified the strong determination of African Americans to use (like others) a range of strategies (e.g., litigation, direct action, and electoral politics) to promote and protect their policy interests. Of more enduring importance, policies emanating from the 1950s and 1960s now provided a more viable legal framework within which to utilize such strategies.

To be sure, there is a rather rich scholarly literature on the overall nature and functioning of our politics and political system and on how blacks and their allies have attempted to overcome the deleterious effects of racial segregation and discrimination. The scholarly record, however, does not sufficiently delineate (except rarely or by casual discussion) what the African American experience suggests about the relative limits and capacity of various strategies (when viewed in context of the overall political system) to achieve fundamental policy change. In brief, though measurable progress has been made, the African American experience since the 1950s indicates clearly that no matter the individual or collective thrusts of the strategies employed, glaring incongruities remain between democratic theory as expounded in the law and the empirical reality as experienced in everyday life and practice.

This essay projects a continuing hope tempered by empirical reality. It is informed by my research and teaching over some 30 years and enriched—but I hope not blinded—by a lifetime of personal involvement and experience. From a theoretical perspective, we are interested in the differential consequences of various forms and structures of our politics and governing system that serve to maximize or impede the practice and potential for democratic government. The practical importance of our discussion is

clearly evident. Developments at home and from around the world demonstrate concretely the urgent need to show how individuals and groups from diverse racial, ethnic, religious, and cultural backgrounds can be fairly and peacefully united into effective democratic governments and societies.

That we must face such problems is not a matter of choice but one of necessity. Sheer demographics alone illuminate the imperatives for doing so.⁴ Indeed, one does not have to be a soothsayer or doomsayer to suggest that dealing with such problems is likely to prove a cutting issue of politics and governance at home and abroad for some time to come. Clearly then, this is an issue to which political scientists should give increased attention. Should we not do so, then we will, in fact, be as irrelevant—or as "merely academic"—as some of our critics so often claim we are.

Let us, then, consider what the African American experience might tell us about the nature and functioning of our politics and political system.

FROM LEGAL PRINCIPLES TO POLITICAL REALITY

Since 1950, policy conflict with respect to the African American experience may well be described as moving along a continuum from the principles and dynamics of moral-legal combat to the realities and vagaries of everyday practical politics. Indeed, during the past four decades, African Americans have used an expanding range of strategies to promote their interests. Different strategies of resource or influence mobilization are substantially dictated by the rules and norms of particular institutions. If the votes are available to offer a hope of winning office, for example, then electoral politics makes sense. If not and appeal to constitutional-legal principles seems more promising (as it did in the 1950s), then the decision of Thurgood Marshall and the NAACP to resort to litigation, rather than electoral politics, appears altogether the sensible course to take.

Litigation as political strategy is inextricably linked to the limits and capacity of courts as governing institutions in the political process. The chief benefits of using courts and litigation for political strategy rest on two major pillars: (1) the responsibility of the federal judiciary to expound ultimately the meaning and limits of the Constitution as the supreme law of the land and (2) the widely held view, no matter what the empirical reality, that we are a "government of laws, not of [persons]." The authority of the judiciary in constitutional interpretation, bolstered by its authority in statutory interpretation, accords the judiciary (especially the U.S. Supreme Court) a highly visible role in our politics and policymaking.

In addition, the resources needed to negotiate and prevail through litigation are far less massive than those needed to prevail in electoral politics. Law and reasoned argument, not popular support and political influence, are the crucial determinants ostensibly needed to prevail in the judicial arena (Barker 1967). This was reflected well by Justice Brennan's widely quoted comment in *NAACP* v. *Button*, where he stated forthrightly: "Groups which find themselves unable to achieve their objectives through the ballot frequently turn to the courts. . . . Under the conditions of modern government, litigation may well be the sole practicable avenue open to a minority to petition for redress of grievances" (*NAACP* v. *Button* 1963, 429–30).

Even so, the capacity of courts remains limited (Horowitz 1977, 17–56). Indeed, some of the benefits that some attribute to courts might likewise serve to limit the use and viability of litigation as political strategy. For example, the nonelective character of the federal judiciary might serve to limit not only the relative legitimacy and acceptance of judicial policy but its nature and scope, as well.

The judiciary is also limited in that, for example, it it not a self-starter and cannot initiate action but, rather, must wait for such action to be brought. Similarly, the nature and scope of judicial policymaking is said to be limited by the particular and limited confines of the case or controversy requirement. Just as with its benefits, however, these limits on litigation are not static or fixed but may be modified or circumvented in accord with the force of dynamics existent under the circumstances involved.

Thus, when viewed in overall context, the use of litigation as political strategy was not chosen by accident by Marshall and his colleagues. They were keenly aware of the dire lack of interest representation that African Americans commanded prior to the 1954 Brown decision in both the judicial and the elective—political forums. Indeed, part of Marshall's genius as a litigator was his grasp of the federal judiciary in systemic perspective. He viewed the courts as "part of a system in which various political and legal actors act interdependently, responding to one another within the [opportunities] and constraints of their various constitutional roles" (Barker 1992, 1240).

The legal principle sought and articulated by the Court in the 1954 school desegregation cases effected a revolutionary change in our constitutional law and provided important symbolic and substantive supports with potentially important consequences. This was clearly reflected, for example, by the position of the United States in its amicus brief supportive of the NAACP position in the 1954 *Brown* litigation. The government said: "The subordinate position occupied by Negroes in this country as a result of governmental discriminations presents an unresolved problem for American democracy, an inescapable challenge to the sincerity of our espousal of the democratic faith. . . . We know the way, we need only the will" (*Brown* v. *Board of Education* 1954, 166).⁵

Nevertheless the very success of pro-civil rights interests in the 1954 cases demonstrates both the strength and limits of litigation strategy. Though grandiose in rhetoric and certainly important, a sys-

temic perspective tempers one's assessment of the victory achieved in *Brown*. In 1955, the Court outlined the obviously ambiguous terms under which its new legal policy was to be implemented with all deliberate speed. "It is clear," as a former general NAACP counsel put it some years later, "that what the formula required was movement toward compliance on terms that the white South could accept" (Carter 1968, 243).

Other systemic factors contributed to the tenuousness of the Court's policy announced in *Brown*. This was clearly reflected by the equivocal support that the Court received from important political elites, including President Eisenhower himself, and the outright hostility and opposition openly voiced by a host of congressional leaders and state officials.

Indeed, though Marshall's litigation strategy had wrought fundamental changes in the law, there were no corresponding changes in the everyday life and practice of the masses of black Americans. Among other things, this situation illumined clearly the limited nature of judicial efficacy in terms of effecting major social change and served to increase the rising tide of black discontent.

Direct Action Politics: The Civil Rights Movement

The torch of black leadership thus passed from the legal lawyer-leadership of a Thurgood Marshall to the political movement-protest leadership of a Martin Luther King. This leadership transformation reflected the urgent need and desire of African Americans to bring about more immediate and far-reaching policy change unrestricted by the legalities, structures, and formalities of courts and litigation. The term movement itself is "a language of action" that stresses "the imminence of the crisis," "the magnitude of the stakes involved," and conveys "a sense of dynamic growth and prospective impact" (Salisbury 1989, 29). Indeed, "the political logic that underlies movements is that only large-scale action and demands can get beyond incrementalism to achieve fundamental changes in the structure of social relations, political power, or economic reward" (p. 22). Further, the strength of masses and numbers themselves lends a democratic character to the movement and its cause.

Movement-protest politics provides an attractive option to those who lack the resources needed to make effective use of electoral politics, litigation, or other strategies to achieve their objectives. Direct action movement politics lacks many of the socioeconomic biases that work against involvement of such groups or persons in more "mainstream" forms of political participation, such as electoral politics and interest group and party activity (Schattschneider 1975). In other words, movement politics draws support from the "underaffiliated segments of society" (Salisbury 1989, 25). Movements espouse large-scale collective action and encourage people to press for their appeals not only upon legislators and other policymaking elites but also on public opinion generally. Movement politics, with its marches, demonstrations, and protests, attracts attention of communication media, raises the consciousness of both actual and potential supporters, and (when it succeeds) expands the scope of conflict (Baumgartner and Jones 1993; Schattschneider 1975).

But such apparent benefits may also hold some costs. Expansion of the scope of conflict, for example, might well invite sympathizers to become active supporters and apathetic bystanders to become sympathizers. But this very expansion may arouse opposition and engender backlash. Media coverage similarly cuts both ways: it provides exposure and free publicity to movements, but it can also galvanize opposition and may misrepresent and distort the image or goal(s) of the movement and stir leadership tensions within the movement itself (Gitlin 1980, 146–79).

Moreover, political movements constitute attempts to change some basic political and social practices and bring them into line with what some writers have labeled the "American creed" (Huntington 1981; Myrdal 1962, chap. 1). But some of these changes, such as those involving fundamental economic power relationships, fail to attract much support. Not only do these changes call for a redistribution of wealth and power, but they would require Americans to question some components of the "American creed," such as self-reliance and the superiority of free enterprise. This was why, initially at least, the Civil Rights movement did not push the economic issues to the fore, although they were clearly on Martin Luther King's agenda. He had stated that for years the Negro "had lived on a lonely island of economic insecurity in the midst of a vast ocean of material prosperity" (King 1963, 11).

Movement politics undoubtedly captured the attention of many African American communities, indicating growing unease and discord with the NAACP and a legal approach whose ultimate policy implications were not fully spelled out.

Given the circumstances, resources, and stakes involved, the resort to direct action-movement politics was likewise an altogether plausible strategy. This is reflected well by Martin Luther King, who indicated disappointment with not only the ambivalence of judicial policy but also the response and reaction to the Court's decisions. "The failure of the nation, over a decade, to implement the majestic implications of these decisions, said King, led "to the slow ebb of the Negro's faith in litigation as the dominant method to achieve his freedom. In [the Negro's] eyes," King continued, "the doctrine of legal change had become the doctrine of slow token change and, as a sole weapon of struggle, [has] now proved its unsuitability" (1963, 23). King also stated that "the outburst [of African American discontent]"-his characterization of the 1963 massive march on Washington-was also "rooted in the disappointment with both political parties."

Overall, the congruence of its basic goals with constitutional-democratic norms, plus the practical realities of the existent situation, gave a strength and unity to the Civil Rights movement far beyond what traditional indices of political clout would suggest. And as it evolved, the movement engendered a crisis situation and attracted the kind of mass and elite support that would eventually stir a reluctant America and two presidents to action.

Not unlike litigation, the successes attributed to the Civil Rights movement also revealed its limits and capacity as political strategy. The 1963 march on Washington and the subsequent passage of the civil rights acts of 1964 and 1965 represented the high point of the Civil Rights movement. But once those broad policy goals were achieved, the capacity and limits of movement-protest as political strategy came into sharper focus. Attention began to focus on new directions and on the question, Where do we go from here? Factionalism and tensions once again emerged. Differences over strategy and tactics between and among civil rights organizations provided constant tensions and threatened to jeopardize the strength and objectives of the overall movement, but until the legislation of 1964 and 1965 had been enacted, the appeal for unity prevailed. A movement must continue to advocate stirring goals of broad appeal, and it was not at all clear after 1965 what these might be. In the same spirit, one scholar has recently suggested that once the Nineteenth Amendment was enacted, the organized pressure for maternity and other welfare benefits abated, and those programs quietly faded away (Skocpol 1992). After the vote was fully secured, the "women's movement" lost much of its momentum in respect to other objectives.

Indeed, the wide visibility and media coverage that can help mass movements through activating more sympathizers and supporters might similarly publicize that the movement's objectives have been achieved, thus lessening the imperative for the continuation of the movement. This is particularly a salient point in that many African Americans viewed the right to vote and participate freely in elections as the watershed accomplishment.

From Protests to Politics: Electoral Politics as Political Strategy

Whatever its potential, the Voting Rights Act sparked the decided shift of African Americans from "protests to politics." Perhaps the most salient advantage of using electoral politics as political strategy is the widely shared belief in the legitimacy of elections as expressions of the popular will, and thus as the core of democratic government (Grofman and Davidson 1992, 261–317). This belief has been strengthened over time by Court decisions reaffirming elections and the right to vote as fundamental and essential to democratic government (e.g., Baker v. Carr 1962; South Carolina v. Katzenbach 1966).

Further, election campaigns tend to engender widespread media coverage, providing visibility to particular candidates and the interests they represent. Even more basic (as already intimated), electoral

politics, in contrast to other strategies, provides a built-in legitimizing mechanism (elections) from which elected officials and their policy interests benefit. A system of regular free and open elections gives meaning and vitality to the theory of representative government. But the extent to which these concepts are realized in practice points toward some of the limits of electoral politics.

One such limitation is the prevalence and continued use of electoral structures that are not responsive to desires of large numbers of voters. These structures determine in large measure how votes translate into legislative seats. The legitimacy attached to elections, however, makes it difficult to change established electoral structures, no matter what built-in biases are involved. This was essentially the issue that spurred President Clinton to withdraw his nomination of Lani Guinier as assistant attorney general for civil rights. These situations do indeed raise important questions concerning what can be accomplished through electoral politics (Fraga and Anhalt 1993; Guinier 1991a and b). Consider, for example, questions relating to vote dilution, the continued great underrepresentation of African Americans in our governing institutions, and the impact of racialbloc voting and racial sterotyping (Williams 1990).

An additional downside to the use of electoral politics as political strategy is that it is indeed difficult to translate votes and elections into policy outcomes. And even should policies result, the monies and taxes needed to implement such policies depend on a confluence of political and economic forces difficult for elected officials, acting alone, to control.

These reservations aside, however, the right to vote is seen by many African Americans and others as both a basic cherished democratic right and a powerful political weapon. Thus, with the vote secured, black voter registration skyrocketed, as did the number of black elected officials. Blacks were elected to political office throughout the entire system, at national, state, and local levels. This movement from "protests to politics" was especially highlighted by the election of a host of big-city black mayors. These developments perhaps reached their apex in the late 1970s and early 1980s, leading many to think that at long last, African Americans were well ensconced in the political system.

But as time passed, the success of blacks in gaining elective office and their experiences there began to illuminate anew and quite vividly the limits of electoral politics as political strategy. This is particularly salient at local levels, where blacks have made their greatest gains in winning elective political office. Holding local office in Detroit, New Orleans, and Los Angeles, however, does not bring with it the resources required to address the needs and heal the wounds of the core city population. But at the state and national levels, where resources might be more readily mobilized for such purposes, African Americans are not nearly as strongly represented, thus making their local political power seem a "hollow prize" indeed (Friesma 1969; Preston 1990).

The fluidity of electoral politics as political strategy has also been demonstrated at the highest level. Eight years under Nixon–Ford led many African Americans to fear continued Republican rule. In particular, President Nixon's "southern strategy" resulted in appointments to the Supreme Court that portended the erosion of recently won judicial and legislative policies supporting pro–civil rights interests. Not unexpectedly, then, in the 1976 elections blacks gave their strong support to Democratic candidate Jimmy Carter, clearly hopeful that he would reverse particular policy trends of the Nixon–Ford era.

But such expectations soon turned to disappointment. Whatever the reason, the Carter administration undertook few policy initiatives regarding minority concerns. (e.g., housing, jobs, education). To be sure, Carter did appoint a number of blacks to important positions, including the appointment of a record number of blacks and minorities to lower federal courts. However, this did not blunt the overall frustration and disappointment felt by many blacks toward the Carter administration.

This general disenchantment was further exacerbated by messages emanating from the Supreme Court that began more fully to reflect the impact of the changing political climate on courts and law. To be sure, certain decisions of the Burger Court relating to privacy and the rights of women served generally to advance rights and liberties. But the record of the Burger Court in other areas, such as school desegregation, affirmative action, and voting rights was decidedly mixed, with uncertainty being reflected between-and even within-decisions. In short, though the Burger Court did not bring about the counterrevolution that some had expected, its overall decisional thrust was to make more uncertain the kind of support and representation that African Americans had come to expect from the Warren Court (Barker and Jones 1994, chap. 5). The 1980 Mobile voting rights case especially roused opposition among civil rights interests: the decision's "intent" standard rather than "results" standard clearly threatened the effectiveness of the Voting Rights Act of 1965—the central foundation upon which blacks based an "electoral politics" strategy. Subsequently, however, pro-civil rights interests did succeed in persuading Congress to pass remedial legislation designed to overcome the adverse effects of the Mobile decision.

The Resurgence of "the system": The Central Force of Electoral Politics

The 1980 presidential election demonstrated concretely the increasing popular disquiet over the real or perceived consequences and trends flowing from major policy thrusts of the 1950s and 1960s. Indeed, the 1980 election (and the 1984 and 1988 elections as well) illuminate once again that electoral politics lies at the center of our politics and political system.

As expected, and given his constituency base,

President Reagan gave little support to African Americans and pro-civil rights interests. His legislative agenda and budget priorities brought about "a total redefinition of racial equality": "Throughout government and in the management of many major institutions, the view that further governmental action for racial equality was necessary simply disappeared" (Orfield and Ashkinaze 1991, 205–7).

In general, the national consensus and commitment to civil rights forged during the 1960s was clearly coming apart, to be replaced by policies and officials who assumed that enough had been done, and, as they saw it, had now resulted in policies and practices that were unfair to whites (e.g., affirmative action). During the Reagan administration, key civil rights agencies (e.g., the Civil Rights Commission and the Civil Rights Division in the Department of Justice) were headed by officials who shared these views and acted on them.

Ironically, it was through courts and litigation that the Reagan administration brought about changes that are likely to endure for some time. In addition to making an unusually large number of judicial appointments (Goldman 1989; Lyles 1991, chap. 5), Reagan's legacy also reflects his administration's aggressive use of litigation as political strategy. Indeed, as suggested by his former solicitor general Charles Fried (1991), the Reagan administration directed and strongly supported litigation specifically designed to overturn judicial policies favorable to pro-civil rights interests.

Although the Reagan era saw an increase in the number of black elected officials, it was at a slowed rate. Even more, drastic budget cuts reduced the value of many of these offices. This was particularly salient among African Americans whose mayoralty elections in some of the nation's largest cities did not have as much impact on people's lives or conditions as many persons, including blacks, had thought or expected (Preston 1990).

This was the overall political climate that gave rise to the candidacy of Jesse Jackson for the Democratic presidential nomination in 1984. The Jackson candidacy, evolving as it did, dramatized well the strategic shift of African Americans from movement-protest to an electoral politics strategy (Barker 1988; L. Morris 1990). And Jackson's candidacy achieved benefits far beyond what traditions and resources would suggest. But it—and his 1988 campaign as well—illuminates vividly some of the limits of electoral politics for African Americans. These limits were clearly illustrated by Jackson's relative lack of resources (e.g., money, experienced staff, policy analysts) needed to run a competitive campaign for the presidential nomination. This situation reflects clearly the underrepresentation of African Americans not only in politics and government but in other major socioeconomic sectors from which money, staff, and other resources needed to negotiate the rigors of electoral politics (and interest group politics generally) would be expected to come. But perhaps of greater importance, the limits of electoral politics were also illustrated in Jackson's campaigns by the large number of white Americans (about 20%) who indicated that they would not vote for any African American candidate for president, no matter how qualified (L. Williams 1990, 51).

As a candidate, Bush vowed to follow the lead and policies of the Reagan administration, and as president, he made good on this promise. During his campaign, for example (and reflective of his constituency) Bush took strong positions seemingly designed especially to attract and increase the white vote (e.g., the need to revamp the welfare system and to strengthen laws against crime).

Once in office, President Bush continued to take positions and actions that were not supportive of African American interests. This was perhaps most visibly (and importantly) demonstrated when the president nominated Judge Clarence Thomas to replace the retiring Justice Thurgood Marshall. Although both African Americans, Marshall and Thomas represented almost polar opposites in their concept of the judicial role and their posture with respect to civil rights and civil liberties. At bottom, Marshall and Thomas differed in their very definition of the African American predicament and the responsibility of government regarding it. After raucous and suspenseful hearings involving serious charges of sexual harassment leveled against him by black law professor Anita Hill, Judge Thomas eventually won confirmation despite major concern about his judicial competence and strong, almost unanimous opposi-tion from established black political leadership, though (at least during the hearings) not from African American public opinion at large (Mansbridge and Tate 1992). This initial support from the black community may well reflect that the temporary strength of the symbolic ties of race and color was able to overcome, at least initially, the more enduring effects of policy and substance now being reflected in certain court decisions. Indeed, what makes Justice Thomas's appointment to the Court especially crucial even at this relatively early point in his career is that the initial thrusts of certain of his decisions (e.g., with respect to voting rights) could pose threats to basic

The 1992 elections afford still another vantage point from which to view the African American experience with electoral politics. Since the mid-1980s, Governor Clinton and his Democratic Leadership Conference were committed to the assumption that to recapture the White House, the Democratic party had to recapture Reagan Democrats. This had to be done by developing a more moderate—centrist party image that would appeal to the middle class, rather than to the poor and lower classes, where blacks and minorities are found in disproportionate numbers. This middle-class focus fits well with the general "mainstream" tendencies of American politics, where compromise and consensus, not conflict and division, are considered the hallmark of good politics.

legal gains fashioned in the 1950s and 1960s.

Clinton's developing candidacy was clearly enhanced by the overriding concern expressed by Dem-

ocrats generally, (including many black leaders) that a united party was essential to win the election and recapture the White House. This concern affected the campaign and election in significant ways. It probably contributed, for example, to the eventual decision of Jesse Jackson not to enter the contest for the Democratic presidential nomination. And Jackson's decision was important to Clinton. Jackson's appeal had been shown in the 1984 and 1988 campaigns, and it was clear that Jackson posed a real threat to Clinton. Very directly, Jackson's decision helped to subordinate, if not suppress, race as an issue, making it easier for Clinton to hold on to the black vote without jeopardizing the moderate-centrist image believed necessary to recapture Reagan Democrats and appeal to whites generally.

Of course, another significant development favoring Clinton was the rapidly worsening economic condition that caused the high, Gulf War-based popularity ratings of President Bush to plummet, allowing the politics of the economy to further obscure and engulf the politics of race. Thus, Clinton could more readily address many matters in "deracialized" terms, focusing on issues (e.g., unemployment, education, health care) that have disproportionately affected blacks and minorities but that could now be addressed toward the increasingly dire plight of the predominantly white middle class. Clinton also distanced himself from certain leaders (e.g., Jesse Jackson and Senator Edward Kennedy) and took more conservative positions on issues (welfare, crime) so as to avoid too close an identification with "racial" or "liberal" concerns.

In general, Clinton was able to forge a winning coalition relatively free of commitments to specific interests or groups, such as labor and minorities. African Americans, for example, overwhelmingly supported Clinton without exacting any obvious or apparent commitments in exchange for their vote. As with Democrats generally, black electoral strategy and politics in the 1992 presidential election were influenced by the overriding objective of winning the election. They were also influenced by the fact that blacks lacked a meaningful alternative (as has been the case traditionally) and simply had nowhere else to go.

This is all there was to black electoral strategy in 1992, and it remains too early to determine its efficacy. On the one hand, some of Clinton's major appointments demonstrate his repeated commitment to a government that reflects the "diversity" that exists in America. On the other hand, his widely publicized actions in other matters (e.g., the withdrawal of the Lani Guinier nomination and his positions on Haitian refugee policy and such issues as welfare, crime, and gays in the military) reflect the determination of Clinton as president to hold on to his centrist image.

When seen in total perspective, an overview of the African American experience since 1950 suggests that we might need to revisit the theory of American pluralist politics (Hero 1992; Pinderhughes 1987).

Pluralist politics illuminates clearly, for example, that there are always winners and losers in elections and that some interests are furthered and others retarded depending on who wins. But the interdependent and incremental nature of our system usually mitigates the loss of the losers. These matters are considered by Madison in the Federalist 10 and 51. Moreover, the degree to which losers are able to protect previous gains is usually related to their size and electoral involvement. But this does not appear to be so with racial minorities, at least not with African Americans. The relationship to the system seems to be the reverse. After the threshold surge in the 1960s, as black electoral involvement became stronger, the policies that would have favored them (at least at the national level) became weaker, having been quite steadily—even systematically—eroded, with the prospects for improvement becoming dimmer (A. Morris 1991).

SOME FINDINGS AND OBSERVATIONS

The phenomena just described may not be unique to African Americans, but it is not the typical pattern in American politics, either. And the consequences seem to be more profound and rather permanent. Let us take a closer look at how the African American experience since 1950 may shed some light on this and other aspects of our politics and the political system.

First, the African American experience suggests convincingly that the overall nature and functioning of our politics continue to advantage certain interests and disadvantage others. For example, those interests strongly represented in our original constitution making (i.e., propertied white males) have been clearly protected and advantaged by the nature and structure of the political system they developed. Conversely, those interests not initially represented (e.g., African Americans and women) have beenand remain—clearly disadvantaged by the nature and operation of that system. But the African American experience also demonstrates that the character of certain principles as embodied in the Constitution, particularly as strengthened by subsequent amendments (e.g., the Fourteenth), are important on their own terms and can be used effectively by excluded or overlooked interests to overcome some of their continuing disadvantages.

The African American experience strongly reinforces the long-held observation that the limits and capacity of particular political strategies and institutions are determined essentially by their interaction with, and interdependence upon, one another. Especially for minorities, however, the choice of strategy cannot be taken as an end in itself. It must be taken in larger context, toward the view of making more enduring changes in the "rules of the game," the process, and the system. Electoral strategies resulting

in electing mayors and controlling city councils, for example, mean little unless they work toward bringing about such enduring changes.

Within this context, however, there remains the necessity for groups to forge coalitions and coordinate strategies in order to safeguard and promote their interests. In many ways, this is reflected by the African American experience in the 1950s and 1960s, when a combination of strategies (litigation, direct action, and electoral politics) stimulated and steered major change in our law and public policy. This very example, however, reflects the basic difficulty of bringing about major change in behalf of aspiring interests. Seldom can such forces be brought and held together sufficiently long to bring about fundamental policy change. This could prove even more difficult given the changing nature of the issues involved and the resultant problems that make coalition formation in today's political climate quite different from that during the 1950s and 1960s. Though African Americans, like many others, are amenable to coalition politics, "the problem has been that for the most part they have had few important takers, especially when the agenda concerned economic issues, as it does today (Hamilton 1991). When viewed in this light, Madison's prescriptions, which work so well in many instances to prevent factional mischief, seem not to work so well when it comes to protecting racial minorities, at least in the case of African Amer-

The difficulty of effecting major policy change is exacerbated by the fact that strong supports are needed not only to enact such changes but to implement them effectively, especially when they are designed to overcome the effects of deeply ingrained practices and traditions that continue to advantage the very interests that traditionally and currently remain dominant in our politics and society.

The two broad coalitions that give life and vitality to the governing system—the major political parties-have been unable or unwilling, for the most part, to deal forthrightly with matters of race. Race continues to be a volatile issue in American politics, as evidenced by both public opinion polls and racially inspired outbreaks of violence (Levin and McDevitt 1993). This has resulted in a situation that one political scientist described as follows: "We have become an extremely multi-racial society during a period of political domination by [a] virtually all-white national party, the Republicans, and in a time when political defeat and fear of defeat has rendered the [other] national party (the Democrats) that receives the great bulk of the minority vote, incapable of discussing and acting on problems of racial and ethnic discrimination and inequality" (Orfield 1993, 6-7).

Moreover, we must also recognize that in this day of "privatization," African Americans (and other historically underrepresented groups, e.g., Latinos and women) continue to be grossly underrepresented not only in the public sector but in key areas of the private sector, as well. And it remains the case that outcomes in both sectors favor those whose interests

are sustained and advanced by strong and assertive representation. This is especially critical given the intimate relation of our politics and political system to the private sector. This public–private nexus allows us to discern more clearly the nature and influence of private decision making (especially the role of business) in fashioning public policy (Lindblom and Woodhouse, 1993, chap. 8; Nelson 1990, 192–94).

But representation may not be enough and at times might even thwart the possibility of basic change for those who need it most: "In taking up electoral politics, blacks and their leaders have implicitly embraced strategies that preclude the possibility of radical goals" (Tate 1993, 170). At least, this author continues, the elections of black city mayors and city councils "have not produced the radical and profound changes envisioned by the civil rights activists of the 1960s" (ibid.). It must be remembered that these changes, however, were envisioned in large measure on moral-jurisprudential and constitutional grounds, not on compromise-practical grounds of electoral politics. To be sure, the shift of blacks from protests to politics and the nature of electoral politics itself minimize attention to these constitutional and moral-jurisprudential considerations and, rather, enhance decisions being reached in terms of "politics as usual," that is, on practical political grounds.

Another observation that flows from the African American experience is the meaning and importance of symbols and symbolism and their relation to the reality and substance of politics (Edelman 1964). Important public acts have both substantive and symbolic dimensions. The symbolism of political practice may be used, however, to promote diverse political interests ranging from incentives for additional accomplishments all the way to what is needed to maintain system stability and satisfaction.

The symbolic elements of the civil rights act of 1964 and 1965 could well be used either to highlight or to obscure the substantive benefits that might reasonably be expected to flow from such important developments. For example, the increase in black voter registration and in the number of black elected officials clearly had substantive importance in certain areas. On the other hand, repeated focus on such numbers might well exaggerate the degree of actual substantive change. Moreover, even with such dramatic increases, black elected officials still constitute less than 2% of all elected officials in the country (Williams 1990, 45).

Even more basic, the focus on numbers encourages us to avoid questions relating to the efficacy of the vote and electoral politics as agents of social change: "The daunting task facing black political actors," as put in a recent volume [is that of] "trying to consolidate their newly achieved positions in the system while at the same time trying to change the system itself." (Barker and Jones 1994, 321; see also Keech 1968, chap. 6).

Martin Luther King's comments with respect to "tokenism" are relevant here and help to put our

discussion of symbolism, especially its inhibiting effects, in sharper perspective. Said King:

Those who argue in favor of tokenism point out that we must begin somewhere; that it is unwise to spurn any breakthough, no matter how limited. This position has a certain validity, and the Negro freedom movement has more often than not attained broad victories which had small beginnings. There is a critical distinction, however, between a modest start and tokenism. The tokenism Negroes condemn is recognizable because it is an end in itself. Its purpose is not to begin a process, but instead to end the process of protest and pressure. It is a hypocritical gesture, not a constructive first step. (King 1963, 20–21)

Two somewhat interrelated strategies—crossover appeal and deracialization—have been offered as ways to overcome some of the continuing dilemmas of electoral politics as political strategy. Crossover appeal focuses primarily on the ability of black candidates to attract white voters. It also, of course, relates to the ability of white candidates to attract black voters. But research suggests that crossover appeal poses severe obstacles, especially for black candidates. Black voters harbor far less racial prejudice and sterotypes and are far more likely to vote for white officials than white voters are likely to vote for black candidates (Williams 1990, 51). Moreover, the higher the office, the more likely it is that whites will not vote for a black candidate.

Deracialization, however, cuts both ways. Black leaders, for example, were willing to follow then governor Clinton in a deracialized campaign because it appeared that taking care of issues affecting the white middle class (health care, jobs, and education) would also cover major issues that have for so long disproportionately hurt blacks. Indeed, given the circumstances, by putting issues in race-neutral terms and appealing to universal values (health care for everybody), a candidate can couch his programs in race-neutral rather than race-specific terms and thus enhance the chances of victory.

But this central strategic purpose of deracialization to circumvent or otherwise minimize controversy over the emotionally divisive issue of race points to one of its greatest problems. To continue to treat lightly or brush aside such problems, especially in the face of serious and continuing opportunities to focus on them, defies reality as well as democracy. The 1992 Los Angeles riots offer a prime example of such a missed opportunity. Another prime example is how ostensibly race-neutral policies (urban budget cuts) can and do have devastating race-specific effects (unemployment). We cannot overlook how less-dramatic-even daily-occurrences reflect the extent to which race continues to inhibit African American progress. To continue to overlook and squander such opportunities tends to raise questions about our professed commitment as a nation to democracy. Indeed, it is difficult to fathom how the broader public interest is served by policies that limit the opportunities of such a substantial segment of the American population (Fraga and Anhalt 1993). Our sense of historical reality should remind us, however, that neither the state nor force of arms can quiet or circumvent such matters forever.

In addition, the African American experience underscores once again the pervasive influence and impact of electoral politics throughout the governmental system, including the courts. A systemic perspective allows us to see how Court decisions on major policy issues (even those articulating the meaning and scope of basic constitutional-democratic values) encounter difficulty—even reversal—when they venture too far from the views of electoral majorities.

The African American experience also shows that when combined with judicial support, movement politics can sometimes go a long way toward influencing elective institutions to take important decisive action. But that experience also shows how electoral politics can, over a relatively short time span, radically shift the balance of forces in regard to such issues. Since 1968, electoral politics, influenced by a number of factors, has brought about a resurgence of "normal" politics, replacing the essentially constitutional—legal framework in which race issues had been fought since about 1950. This shift in contextual emphasis has culminated in clear erosion—even outright reversal-of major policy thrusts emanating largely from nonelective institutions, namely, the courts. These policy thrusts initially supported minority interests, but over time and in the course of their implementation, they are now being interpreted as adverse to majority interests. And that changed interpretation has been spurred mainly through electoral politics. This suggests strongly that notwithstanding its limits as political strategy, electoral politics must attract the fullest possible participation from African Americans if only to prevent the erosion of policy gains achieved elsewhere.

When viewed in systemic perspective, the African American experience points up the importance to minority groups of pursuing multiple strategies to achieve their objectives. The nature of courts, for example, gives credence and weight to the use of litigation as political strategy. Litigation permits raising and deciding fundamental issues in forums generally free from vagaries of majoritarian politics and popular opinion. The strategic use of litigation calls on leadership elites in both the public and private sectors, at least to consider the basic moral and jurisprudential dimensions of the issues involved. It lays bare the gaps between professed constitutionaldemocratic values and principles and everyday experience and practice. Notwithstanding their limits, the African American experience indicates that favorable court decisions can prove important supports in interest conflict and policy combat. That experience suggests, for example, that Court decisions may prove formidable barriers against popular assaults on minority rights. Thus, though the Supreme Court is not a panacea, neither is it a "hollow hope" (Barker 1967; Rosenberg 1991).

Similarly, the African American experience suggests the importance of direct-action politics, allow-

ing mass publics to articulate and petition for redress of grievances relatively free from the risks of misrepresentation by others. But as I have suggested, movement strategies encounter difficulties, their potency and influence being swiftly reduced once the original broad goals have been achieved. Still further, interest-group bargaining to form coalitions of support in Congress and to bring pressure to bear in executive branch actions is as necessary to minority group success as it is to any other interest in American politics. In this regard, the efforts of the NAACP in Washington and of the Congressional Black Caucus exemplify the continuing importance of these "normal" political strategies in interest conflict. Overall, though varying in their efficacy, not one of these strategies is sufficient or can be ignored. To do so is to overlook the breadth and complexity of the systemic context in which they all operate.

CONTINUING HOPE AND EMPIRICAL REALITY

In my introductory remarks, I suggested that this essay would project a continuing hope tempered by empirical reality. The African American experience does suggest some rays of hope and empirical reality implores us to find them. In my judgment, the constitutional structures and limits outlined by the framers were designed to enhance, not impede, the realization of democratic politics and government. Our discussion of the African American experience offers evidence illustrative of this thesis. That experience, since 1950, demonstrates that fundamental policy change in pursuit of basic democratic goals and values can be achieved within such structures and within what otherwise appear to be intractable limits, depending largely on the nature of the dynamics and circumstances involved (Baumgartner and Jones 1993).

That fundamental policy change was characterized by a confluence of factors including: (1) the perseverance of the aggrieved interests engineered by an extraordinary and resourceful leadership; (2) judicial articulation and support of the fundamental constitutional, moral, and jurisprudential nature of the minority rights at issue; (3) the massive scope of movement-protest that dramatized the serious gaps remaining between professed constitutional-democratic ideals and the realities of everyday life of African Americans; and (4) the unusually strong (though relatively brief) commitment of presidential leadership in support of the cause involved.

When viewed in combination, these factors suggest that the president and the congress took meaningful actions in civil rights "only in response to serious pressure or outright crisis," for "only crisis can normally greatly speed the incremental process" of major policy change (Rodgers and Bullock 1972, 211–13).

This is the major reason why I think that the leadership dimension illuminated in the African American experience should be underscored. No

matter what crisis or stimulus is involved, that experience demonstrates clearly the need not only for courage and commitment but for skilled leadership in both public and private sectors. That leadership becomes even more crucial in that today issues of race and color are much more complex than they were in the 1950s and 1960s and have become fused with issues such as inadequate education, housing, and health care, which may now affect many more white Americans than what had or seems to have been the case. For some, this complexity leads to inaction and indifference. For others, it suggests that the problems are too intractable for real solution. To yet others, it may suggest that there is no racial problem. Still others may be convinced that our country has made more progress in this regard than other nations and that we should thus take heart from such accomplishments and strive to do more.

Whatever position one takes, however, and despite the complexities involved, glaring inequities and disparities remain between persons along racial and ethnic lines. In the face of such stark empirical reality, it becomes increasingly difficult to overlook continuing institutional and structural barriers that perpetuate the vestiges of racism. As a result, it is also becoming increasingly clear, for example, that more than litigation, movements, and electoral—politics are needed to achieve equality and justice for all persons.

At bottom, the African American experience suggests that achieving equality and justice for all persons takes extraordinary leadership and people of good will in both the public and private sectors, who are committed to see the constitutional-democratic system work. In discussing the work of the constitutional convention, one student of American politics put the matter in sharp perspective: "Historic opportunity always exists. The difference between then and now is that the founders appreciated their situation, and today's political leaders rarely are able or willing to do so" (Lowi 1976, 78).

But some are. Though himself initially reluctant on political grounds to support civil rights legislation, in the end, it was President John F. Kennedy who perhaps best captured the essence of the attitude that is still needed if we are to deal with the vestiges of race and racism today. On 11 June 1963, in the immediate aftermath of wanton acts of violence perpetrated against blacks and their allies, Kennedy bluntly told the nation:

Law alone cannot make [persons] see right. We are confronted primarily with a moral issue. It is as old as the Scriptures and is as clear as the American Constitution. The heart of the question is whether all Americans are to be afforded equal rights and opportunities, whether we are going to treat our fellow Americans as we want to be treated.... We face, therefore, a moral crisis as a country and as a people. It cannot be met by repressive police action. It cannot be left to increased demonstrations in the streets. It cannot be quieted by token moves or talk. It is time to act in the Congress, in your State and local legislative body and, above all, in all of our daily lives.... Those who do nothing are inviting shame as well as

violence. Those who act boldly are recognizing right as well as reality. (1964, 468-71)

These words remain relevant today. Their continued relevance is evidenced by the dire need to blunt the proliferation of hate groups and hate crimes spurred by racial, ethnic, and religious bias and prejudice. (Levin and McDevitt 1993; Southern Poverty Law Center 1992). Their continued relevance is evidenced by the dire need to overcome such attitudes as those reflected by our relative indifference to the outbreak of racial violence in Los Angeles in 1992. The words remain especially relevant to overcome a political climate whereby both major parties shy away from close identification or ties with civil rights and minority interests, fostering a political mainstream that provides just enough substantive gains to give symbolic accord and effect to democratic norms and values but denies giving reality to major substantive policy change.

The currency of the challenge remains especially salient in view of the imperatives of our rapidly changing demographics and by the obvious continuing quest of peoples around the world for democratic government and society. The challenge remains current to each of us in our respective areas of competence and authority. Whether in government or the private sector or ensconced within the hallowed halls of the academy, the message embodied in Kennedy's words indicates the continuous need for extraordinary leadership to make an extraordinary form of government—democracy—work.

Finally, I have decided that my remarks might come into sharper focus by sharing some personal reflections on what it has been like for me as a black person in our profession and society generally.⁷ This is a subject about which I have been asked repeatedly and about which I think the time has come to share my views.

First of all, I fully recognize that many of us might wish to forget about the matter of race or might wish to believe that our current public policy now makes for a truly free and open society where any individual, irrespective of such factors as race, ethnicity, or gender, has a fair and equal opportunity to reach full potential, dependent solely on individual talents and initiative. But so to believe is simply to ignore the objective reality that African Americans, for example, continue to be disadvantaged throughout our socioeconomic system. I am afraid that this dire reality has become so commonplace—so much a part of the everyday landscape—that it has dulled our sense of justice and outrage, leading us to become too accepting of the unacceptable and too tolerant of the intolerable. Unfortunately, such attitudes cut across the entire socioeconomic spectrum, and my many years of participant observation in both faculty and administrative roles suggest that these attitudes are also present in the academy.

My overall basic concern is how some of us, as professors, seem quite willing, for example, smugly

to criticize the failures of politicians in civil rights and related matters without recognizing (or perhaps conveniently ignoring) the fact that we, too, are part of the system and can and do perform important roles in this regard. Given the freedom and independence accorded through tenure, we as scholars have a weighty responsibility to pursue the truth in our teaching and research wherever that might lead, irrespective of our own personal or ideological inclinations. Above all, the scholar must be willing critically to analyze even the most volatile problems (such as those involving race) that others might shun or approach reluctantly. Indeed, professors and scholars, particularly those in so-called elite institutions, speak from vantage points that inform and influence, as well as lend authority and legitimacy to attitudes and actions of both elites and masses (Steinfels 1979, 6). To disagree about the problems of race is one thing but to ignore them is unconscionable.

We must begin by looking at ourselves. I trust that we are able and willing to see, for example, both the scholarly and societal value in the good faith recruitment, training, and retention of minority graduate and undergraduate students. Similarly, I am concerned about the dire need for more blacks and minorities to become university and college professors—and top administrators as well. The strains on the few of us who are presently in the academy are uncommonly heavy and are in many ways obscured from the view of many of our colleagues. Sometimes, for example, I think I should become an employment agency and charge a fee for the many inquiries I receive asking if I know where they can find "one," or "might you be interested yourself." But at the same time, I am pleased that at least someone is asking, no matter why. And numerous are the requests to serve on committees, to grant media interviews, and to speak on campus and to community and outside groups.

It is not so easy for me to decline such invitations, as some of us—and I include myself—are wont to do. Indeed, I have come to realize that despite my personal feelings, the stakes involved are much too high for me, as an African American, routinely to decline such invitations. I have seen and experienced first-hand, for example, that the presence or absence of extraordinary leadership, both black and white, in teaching, research, and administrative situations can at times make for material differences in outcomes, as for example, in the recruitment and retention of minority students and minority faculty.

Thus, a central concern is that we as a profession, may be overlooking potential and well-qualified faculty because they are not exactly "our kind" in terms of the nature of their research and methodological approaches or in terms of the journals in which they have published or are likely to publish. To make judgments on such surface grounds is to perpetuate the kind of stereotypical behavior that has historically disadvantaged significant segments of our population. Even more directly, continued adherence to such attitudes and actions tend to blunt and jeopar-

dize the kind of free, open, and diverse environment that first-class universities and colleges need to survive and prosper.

Despite such concerns and pressures, however, I have been able to achieve the kind of balance that has allowed me to keep afloat. I have done so by becoming highly selective in what I take on and learning how to harness my energy and resources, with special attention to undertaking projects that might make a difference on things that matter most to me.

That I personally have been able to survive and reach some level of achievement, however, should not obscure the continuing demeaning and inhuman indignities that many African Americans are still forced to endure solely because of race and color. It is sad but true that what the late Justice Thurgood Marshall wrote in the 1978 Bakke case remains as relevant today as it did them. "It is unnecessary in 20th century America," said Marshall, "to have individual Negroes demonstrate that they have been victims of racial discrimination; the racism of our society has been so pervasive that none, regardless of wealth or position, has managed to escape its impact."8 Marshall's observation remains on the mark: statistics and conceptual models may provide information and explanation, but they cannot begin to give life to the hurt that flows from personal experiences that I and many other African Americans encounter, no matter what our achievements or socioeconomic status.

Let me be very direct and personal. I continue to encounter directly—not obliquely or by implication revolting indignities solely because of my race or color. And these incidents have occurred as late as in 1992–93 in California, not only in the 1950s or 1960s in the deep South, though I encountered them there also. (Barker 1988, 138-40; McClain and Perry 1992, 752-56). Clearly, the demeaning and insulting words used in such encounters and the messages they convey call for a remarkable level of restraint and self-control such as no human being should have to endure. More importantly though, these incidents reflect deep vestiges of a social structure that relegates African Americans to glass ceilings or to certain positions. That ceiling might have been broken a second time by my presidency of this association, example, but neither that nor anything else had relevance for those who, in the past year or so, referred to me as a "bus driver" or those who asked my wife and me upon answering our doorbell whether we were the "caretakers" of our own home.

Despite such encounters and experiences, however, I continue to hold to the goal of a society where, as Martin Luther King eloquently put it, persons are judged by the content of their character, not the color of their skin. Thus, I have found it rewarding to operate in both predominantly black and white institutions and related professional contexts. But I can assure you that while these can prove challenging, they also hold risks. Performing this sort of bridge function can prove difficult; persons who try to serve as bridges risk the chance of falling off both ends or

being stranded and frustrated in the middle. Fortunately and with the help of many persons, I have been able thus far to weather such pitfalls, allowing me an opportunity to carry on such crucial bridge functions as I trust more of us will resolve to carry.

Overall, I continue to be fascinated and excited in my work as a professional political scientist: the last few years have been among the most productive and rewarding of my career. Those rewards and satisfaction become most meaningful, however, when they can help to transform our country and the world into a more fair and just society for all persons. Such a transformation will clearly require extraordinary leadership from our universities and colleges and from our educational system generally. This is also the sort of leadership that is needed to overcome the limits of political strategy and manifest whether the overall political-social system has the capacity and the will to eliminate racism and related problems so as to live up to its democratic potential.

Notes

This essay has profited greatly from comments of a number of colleagues who graciously read all or parts of the manuscript: Mack Jones, Robert Salisbury, Michael Preston, Theodore Lowi, Twiley Barker, H. W. Perry, Kevin Lyles, Luis Fraga, Dianne Pinderhughes, David Brady, Susan Welch, and Michael Combs. I also appreciate the tireless help of my graduate assistant at Stanford, Jeremy Buchman.

1. See Tocqueville 1969, esp. 340–63; Myrdal 1962. See also Jaynes and Williams 1989; Willie, Garibaldi, and Reed 1991; and related volumes produced under the auspices of Trotter Institute of the University of Massachusetts, Boston.

Of particular interest is Rogers Smith's recent essay suggesting that "[a] study of the period 1870-1920 illustrates that American political culture is better understood as the often conflictual and contradictory product of multiple political traditions, than as the expression of hegemonic liberal or democratic political traditions" (1993, 549).

2. For a succinct overall history of the African American experience before and after 1950, see Franklin and Moss 1988.

3. Taking account of much of the available literature, Frank Baumgartner and Bryan D. Jones (1993) provide a perceptive analysis and commentary on the nature and dynamics of fundamental policy change in context of the overall policy process and the political system.

4. The overall census projections, for example, show that by the year 2050, there will be 81 million Hispanics, 62 million African Americans, and 41 million Asian Americans. The overall result is that the total white population will steadily

- 8. See Regents of the University of California v. Bakke 1978, esp. 395-402. Justice Marshall concurs in the judgment at p. 387.
- 9. The late Ralph Bunche was the first African American elected president of the American Political Science Associa-

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DEMOCRACY AND THE PEACEFUL SETTLEMENT OF INTERNATIONAL CONFLICT

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he research reported here develops an explanation for the often-noted absence of international war between democratic states. This explanation is derived from a theoretical rationale centered on universal democratic norms for reconciling competing values and interests. I argue that democratic states locked in disputes are better equipped than others with the means for diffusing conflict situations at an early stage before they have an opportunity to escalate to military violence. Not only is this explanatory logic consistent with the published findings on democracy and war, but it also entails the novel empirical proposition that disputes between democracies are more amenable than are other disputes to peaceful settlements, the hypothesis I examine here. Analyses of contemporary interstate disputes reveal that even when potentially confounding factors are controlled, democratic opponents are significantly more likely to reach peaceful settlements than other types of disputants.

rearly two hundred years ago Immanuel Kant envisioned a liberal democratic zone of peace resulting from what he thought to be an inexorable evolutionary trend toward republican government. There is indeed a global trend toward democratization, though its inevitability remains open to doubt. And Kant was clearly wrong in his presumption that democracies are inherently peaceful. Nevertheless, the basic outline of Kant's vision is remarkably consistent with the weight of current scientific evidence. We now know, as Kant could only speculate, that over the last two centuries democratic states have rarely engaged one another in violent military conflict and, by some criteria, virtually never in a full-scale war. Yet we also know that democracies are no less prone to foreign conflict and war than states organized under alternative arrangements. Unfortunately, there is considerably less consensus about why democracies behave this way.

The research reported here develops and tests an explanatory account based on shared norms for regulating competition and conflict that are integral to the democratic process. In brief, I argue that democratic states locked in disputes are better equipped than others with the means for diffusing conflict situations at an early stage before they have an opportunity to escalate to military violence. This explanatory logic is consistent with the published findings on democracy and war and also entails an empirical proposition, namely, that disputes between democracies, more than other types of disputes, lend themselves to a form of accommodation I term peaceful settlements. Such settlements are but one of several means by which disputing states might manage to avoid escalation to violent confrontation.1 The hypothesis is thus not merely an affirmative restatement of previous results: it identifies a novel contingent claim that represents a significant departure from previous studies.

I shall briefly review previous empirical work relat-

ing democratic governance to foreign conflict, then develop the main theoretical argument motivating the study and discuss the research design and analytic procedures. The empirical domain is a worldwide sample of approximately 250 postwar interstate security disputes compiled by Alker and Sherman (1986) from a list originally developed by Butterworth and Scranton (1976) and others. I present empirical results evaluating the peaceful settlement hypothesis and conclude by providing a summary and discussing the significance of the study.

DEMOCRACY AND INTERNATIONAL CONFLICT

Extensive research on the relationship between democracy and international conflict has revealed two empirical patterns that, taken together, pose a most interesting puzzle. First, democracies are no less likely to become involved in foreign wars, crises, or potentially violent disputes than states organized under alternative governing arrangements (Chan 1984; Domke 1988; Maoz and Abdolali 1989; Small and Singer 1976; Weede 1984; Wright 1942). The weight of current evidence thus suggests that democracy per se does not entail any discernable pacifying effect on foreign policy.2 Yet a second and equally compelling pattern of results shows that democratic states rarely, if ever, fight wars against one another (Chan 1984; Doyle 1986; Maoz and Abdolali 1989; Ray 1993; Rummel 1979, 1983; Small and Singer 1976; Weede 1984, 1992). In fact, democracies seldom even engage one another with threats of military violence, and when disputes do arise, they hardly ever result in military hostilities (Bremer 1992, 1993; Maoz and Abdolali 1989; Maoz and Russett 1993). Taken together, these two empirical patterns have come to be known as the "democratic peace" (Russett 1993).

There is little doubt about the existence of the democratic peace; on this, the historical record has been manifestly clear. What is not yet settled is why democratic states behave in this curious way. Efforts to explain this phenomenon have identified a variety of causal mechanisms broadly based either on common democratic norms, values, and practices (e.g., conceptions of legitimacy or conciliatory means of dispute resolution) or on the institutionalized impediments to war and conflict imposed by democratic decision-making structures (Bueno de Mesquita and Lalman 1992; Doyle 1986; Ember, Ember, and Russett 1992; Merritt and Zinnes 1991; Russett 1990).3 Much recent work on the democratic peace has attempted to articulate competing claims of each approach and to adjudicate them on empirical grounds. Here, however, the historical record has proved much less accommodating, yielding ambiguous and generally inconclusive results (Maoz and Russett 1993; Morgan and Campbell 1991; Morgan and Schwebach 1992; Wang, Chang, and Ray 1992).

It may well be that the empirical evidence is ambiguous because the theoretical arguments are themselves ambiguous. As Morgan and Schwebach aptly note, "A nation imbued with a democratic culture [i.e., norms] will likely establish a correspondent political system and a state structured to constrain will likely foster a democratic culture" (1992, 318). In other words, norms and institutions go hand in hand and any effort to untangle their causal implications faces serious obstacles. The problem is exacerbated by the fact that most of these studies employ but a single testing ground where questions focus on what democracies do not do (i.e., fight), rather than on what they might do differently.⁴ But if this phenomenon is already "overdetermined" by overlapping theories, as Ember, Ember, and Russett suggest (1992, 577), then the search for empirically grounded explanation may be more productively conducted elsewhere.

DEMOCRACY, NORMS, AND PEACEFUL SETTLEMENT

My search for a satisfactory explanation of the democratic peace takes the following form. First, I develop a conceptualization of democracy that highlights the role of norms for regulating the competition over material interests and values. I then show how democratic norms can serve a similar function in the international arena when they are shared by both parties to a dispute. This norm-based explanatory logic not only accounts for established democratic peace results but also encompasses the novel proposition that democracy enhances prospects for some form of peaceful settlement. Following the theoretical development I turn to an empirical evaluation of this critical hypothesis.

Democratic Norms of Bounded Competition

To understand what is distinctive about the international behavior of democratic states, it is necessary to begin with a clear conception of democracy. 6 Definitions abound, but most converge on the central role of certain procedural characteristics such as fairly contested elections, virtually universal suffrage, restrictions on government power, accountability of political leaders, and public expression of preferences (Dahl 1971; Lipset 1959; Powell 1982). Yet identification of common governing procedures implies neither identical institutions nor a broad array of shared values. Certainly there are some values common to virtually all democracies, but there are deep differences as well. (Witness, for example, the divergence between social-democratic and classical liberal conceptions of democracy.) This diversity of democratic norms, values, and institutions, highlights the need to focus as narrowly as possible on those features of democratic systems that are both universally shared and causally relevant to foreign conflict behavior. It will not do to explain the democratic peace in terms of a broadly conceived liberal ideology or a democratic political (or civic) culture. Concepts such as these are too strong to encompass all modern democratic forms and at the same time too weak to carry the explanatory load required of them.

What specific features of democracy meet these criteria? Without minimizing the significance of other defining characteristics or essential procedures, I shall argue that special emphasis should be given to the universal trait of bounded competition. All modern democracies are openly competitive systems of governance where conflicting material interests and basic political values routinely clash over the proper course of public action. Sometimes the competition is fierce and the dissention deep, sometimes the differences are less than profound, but always there are competing interests and values. Elections are one important arena of democratic competition dealing with what is perhaps the most fundamental question of government, Who will rule? But it would be a mistake to conclude that the full measure of democratic competition is embodied in intermittent elections (Schmitter and Karl 1991). Competitive processes may take a wide variety of forms and involve many types of actors, including the political elites who legislate and execute policy at all levels and the citizens who organize and express their interests through a bewildering array of associative arrangements.

Just as competition is a constant of democratic governance, so too is the presence of rules, procedures, or guidelines for setting its boundaries. It may be a commonplace to characterize democracy as less a battleground than a playing field, but this juxtaposition of images clearly conveys the distinctively circumscribed nature of democratic competition. Democracy is feasible only so long as politicians and citizens implicitly agree on the delineation of these boundaries and constrain their actions accordingly. Some boundaries and rules may be universal while

others vary from society to society but all rest on a form of exchange that Schmitter and Karl (1991) call "contingent consent." To take the most ubiquitous example, losers of elections are expected to acknowledge the winners' right to govern and abide by their policy decisions, but only so long as the election was fairly conducted and the winners' fulfill their obligation not to ban losers from future participation. While contingent consent does not deny to democracy a political culture based on deeply held values such as tolerance, compromise, and fair play, neither is democratic culture a necessary condition for contingent consent. Rather, the principle of contingent consent highlights the emergence of "operative guidelines" or "rules of prudence" as the glue holding democracy together (ibid.).

Democratic competition for political office is illustrative of a more general pattern of bounded competition common to all democracies, namely, that political actors, whether inside or outside of government, agree not to employ physically coercive or violent means to secure a winning position on contentious public issues. This is not to say that modern democracies never experience politically motivated violence. My claim is only that the prevailing mode of democratic competition, particularly among politicians and other political elites, is nonviolent. Nor is it the case that democratic states never resort to coercive means within their borders. All states retain exclusive rights to coercion, but democratic states do not ordinarily use coercion for day-to-day management of political competition (though they may do so under extraordinary circumstances). Furthermore, the contingent consent to eschew violence is by no means immutable. Under some conditions, it may even be very fragile and tenuous. Democracy is known to break down. Finally, the claim that democratic actors compete nonviolently in no way implies that they always compete fairly, or nobly, or even

When factions and interests collide in the public realm, as they necessarily must, the resulting conflicts are regulated by contingent consent to follow familiar procedures and practices. The particular form of regulation varies widely from country to country and issue to issue (it may be political or juridical, formal or informal, routinized or ad hoc, authoritative or negotiated), but it is always nonviolent and noncoercive. Although the immediate outcomes of such procedures are by no means certain, unregulated competition and potentially violent conflict risk even greater uncertainty. Contingent consent implies that within democratic societies political actors will prefer to follow nonviolent regulatory procedures and will expect competing actors to do likewise. When actors' preferences and expectations for regularized behavior are widely generalized and reinforced, they frequently become redefined as not merely preferred and expected but also right and proper; that is, they become norms (Axelrod 1986; Opp 1982).

Admittedly, this discussion paints a thin picture of

democracy, but it is sufficient for highlighting an essential point. Modern democratic societies foster the internalization of norms for regulating and reconciling competing interests and values in public affairs in ways that are neither violent nor coercive. For convenience, I shall designate this the democratic norm of bounded competition. I follow conventional usage by stipulating norms to be commonly held expectations of acceptable behavior within society. Norms thus have an evaluative component delineating what is regarded as acceptable and a behavioral component encompassing rules or procedures that are regularly followed. My conception of this universal democratic norm is both proscriptive and prescriptive. First, it narrowly proscribes as inadmissible any use of violent, coercive, or forceful means to prevail in contentious public issues or to gain advantage over political opponents. Second, it promotes use of institutionalized procedures for regulating political competition and resolving contested issues, though I necessarily leave vague the precise structures or mechanisms prescribed in order to accommodate the full range of democratic forms.

Although the norm of bounded competition pervades all segments of democratic society, it is likely to be more firmly rooted among politicians and political elites than elsewhere. The competition of interests and values inherent in democratic systems is actually manifested as a competition among the political elites who are responsible for articulating and advocating representative policy positions and political preferences. Political elites thus have more opportunities to act in accordance with the norm and to benefit from its operation, which together serve to strengthen its internalization (Opp 1982). Strongly internalized norms are powerful influences on behavior, but they are not iron-clad laws. Deviations may be rare (else we would not speak of norms); but they surely do occur, which is one reason why democracies so often institutionalize at least some areas of bounded competition by establishing constitutional or other legal mechanisms, along with measures for enforcement.

Democratic Norms and Peaceful Settlement

The preceding conceptualization of democracy acquires explanatory leverage with help from three auxiliary assumptions that bridge the gap between domestic governing arrangements and foreign conflict. First, I assume that all governmental decisions, including those relating to war and peace, are ultimately made by individuals empowered by institutional roles to act in the name of the state. Recruitment to these role positions is highly variable from state to state, though the competitive institutional structures of democratic states usually entail a recruitment path limiting such positions to seasoned politicians, career government officials, or experienced elites accustomed to operating in the public realm within the normative standards of bounded competition. Second, I assume that interstate conflicts and wars arise for much the same reason as domestic

political disputes—over competing or incompatible material interests and basic values. This is not to imply that foreign policy is nothing more than an extension of domestic politics; for the former is especially characterized by concerns for national sovereignty, security, identity, and the like. All I mean to suggest is that competition of interests and values in foreign affairs is not wholly unlike the competition inherent in the democratic process.

The third assumption is less conventional than the others, though I do not claim original credit. Here I draw from Bloomfield and Leiss (1969) by assuming that conflicts are dynamic situations that evolve over time (see also Alker and Sherman 1982; Sherman 1993). This evolutionary process should not be thought of as continuous movement along some single or fixed trajectory. Most conflict situations consist of relatively static periods of contention punctuated by sporadic transitions marking discernible shifts in levels of antagonism and severity. In the international arena, we usually characterize the severity of conflicts with reference to disputant parties' reliance on military force (Bloomfield and Leiss 1969; Gochman and Maoz 1984). This process need not follow a preordained series of steps or threshold levels. To be sure, some interstate grievances emerge with such severity that military hostilities are initiated almost immediately, but this is relatively rare.7 Thinking of conflict as a dynamic process unfolding through a series of stages directs attention to transitions between stages and in particular to the question why only some interstate disputes escalate to the point where resolution is sought by military means.

Although a full accounting of this escalatory process is far beyond my present purposes, it is possible to offer some provisional and rather stylized observations. Disputes arise initially because two or more parties clash over competing interests or values. Escalation from initial dispute to crisis typically occurs when at least one of the parties perceives little prospect of a satisfactory resolution and decides to communicate the strength of its resolve by making explicit or implicit reference to the availability of military options. Once threats are public and forces positioned, it seems to be a regrettably small step past the next threshold to the actual use of military violence. Of course, not all interstate disputes necessarily traverse this escalatory path. Sometimes adversaries poised on the brink of military hostilities manage to deescalate the dispute (perhaps with the help of intermediaries) while still leaving the underlying issues unresolved. Sometimes the issues at stake are not sufficiently vital to either party to warrant military threats in the first place. Sometimes these issues are overtaken by other events such as a change in political leadership or even by acts of nature that alter the terms of contention. And sometimes the parties are able to achieve mutually agreeable arrangements that remove from contention some or all of the issues precipitating the dispute. Such arrangements, or peaceful settlements, merit special attention because they are directly related to the democratic status of disputing parties.

I earlier asserted that international disputes of democratic states are in the hands of individuals who have experienced the politics of competing values and interests and who have consistently responded within the normative guidelines of bounded competition. In situations where both parties to a dispute are democracies, not only do both sides subscribe to these norms, but the leaders of both are also fully cognizant that bounded competition is the norm, both for themselves and their opponents.8 Democracy, after all, cannot be conducted in secret. When democracies do confront other democracies, these shared norms of bounded competition will provide a mutual basis for contingent consent, suggesting that disputes between democracies should evolve somewhat differently than do disputes between states not sharing these norms (i.e., between democratic and nondemocratic states or between two nondemocratic states).

Two such differences comprise the core propositions of my argument. First, each party to a purely democratic dispute is secure in the knowledge that its opponent is also normatively proscribed from violent and coercive means for reconciling divergent values and interests, at least in the domestic political arena. These norms are externalized to foreign affairs only if the practices underlying them are sufficiently robust to foster mutual expectations of congruent behavior beyond borders. Accordingly, we would expect highly democratic opponents to be constrained from resorting to violent and coercive means at the earliest stage of their disputes no matter how severe or contentious the underlying grievances. I have already pointed out that relatively few disputes originate with hostile military confrontations whether the parties are democratic or not. Democracies may be somewhat more restrained than others, but the absence of immediate violence is certainly not unique to democratic disputants. Nevertheless, this is an important proposition because it renders purely democratic disputes even less likely than others to commence with military action.

Of course the absence of hostilities at the initial stage of a dispute provides no security against hostilities arising from the sort of escalatory process I have described. Even firmly entrenched normative proscriptions can wear thin when severely contested issues fester unresolved for long stretches of time. But bounded competition also entails a prescriptive element that promotes reconciliation of competing interests and values. Because democratic leaders will be normatively inclined to strive for some form of peaceful settlement so long as they can trust their opponent to abide by its provisions, they will be most likely to succeed when confronting other states whose leaders operate under similar normative guidelines. While disputing democracies can never be assured of quick, easy, or successful resolution of their disputes, they should at least be able to establish the diplomatic communication necessary for a fulland measured consideration of possible solutions. Even when democratic states are separated by extremely divergent interests, their common adherence to bounded competition is likely to increase chances of at least some procedural accommodation. In short, democratic disputants are able to diffuse escalatory pressure to military hostilities largely because they are more amenable than others to some form of peaceful settlement.

The preceding rationale offers an unambiguous explanation for the democratic peace based on the role of shared democratic norms in averting military hostilities. This has nothing to do with peaceful predispositions or institutionalized constraints of democratic decision-making structures. It is, rather, that only in the special case of disputing democracies is there a basis for contingent consent. Democratic states confronted by the actual or threatened use of military force from foreign powers will show little reticence to respond in a similar fashion. In fact, such instances are probably governed by their own norms of reciprocity that demand a military response (Axelrod 1986). Nor are democracies unconditionally constrained from initiating military hostilities against others. Thus, the norm of bounded competition accounts for the absence of war between democracies but does not imply that democracies are, on the whole, any more peaceful or any less war-prone than nondemocratic states.

Moreover, this explanatory logic entails a novel and crucial empirical proposition—that shared democratic norms directly enhance the prospects of peaceful settlement. It is crucial in the sense that if the proposition is unable to withstand systematic scrutiny, then my explanatory account fails its most critical test. It is novel in that my emphasis on peaceful settlements represents a significant departure from previous work focusing almost exclusively on the absence of war or militarized disputes. While peaceful settlements do imply a contemporaneous absence of hostilities (at least temporarily), the reverse is not true. Hostilities can be averted by numerous nonsettlement means as well. This is an important point because it establishes the connection between democracy and settlement as a contingent relationship, rather than merely a tautological restatement of previous findings.

Contrary to some restrictive definitions, I do not require that peaceful settlements occur prior to any outbreak of hostilities. I stipulate peaceful settlements to be any written or unwritten mutually agreeable arrangements between disputant parties that at least temporarily resolve or remove from contention one or more—but not necessarily all—of the issues underlying the dispute. This definition has several advantages. First, it does not carry any implication of finality and thereby avoids the problem of specifying a necessarily arbitrary time horizon for distinguishing final settlements from temporary ones. This uncertainty in definition, observes Miall "reflects the real uncertainty in the conflict" (1992, 44). Second, I explicitly recognize partial settlements as diplomatic

achievements in which some issues—often the most contentious ones—may be deliberately and skillfully omitted from consideration in order to secure agreements. Finally, this definition also effectively disentangles the act of successfully reaching agreement from the conceptually distinct task of implementing its terms.

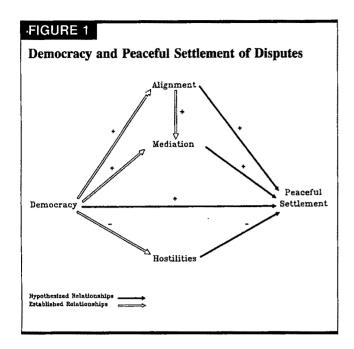
I began with a conceptualization of democracy that gave special emphasis to norms of bounded competition. This was joined with an interpretation of conflict as a dynamic process, along with some other assumptions to construct an explanatory logic able to account for previous results on the democratic peace. This logic also implied a novel empirical proposition linking the democratic status of disputant parties to peaceful settlements. The main task ahead is to develop suitable measures and design procedures to systematically test this hypothesis.

Before I can do this, however, one other matter requires attention. By designating the democracy-settlement hypothesis as a crucial test, I have left implicit the notion that confirmation of the hypothesis also entails support for the underlying theoretical logic. This is not necessarily so. In fact, there are at least three equally plausible alternative explanations that could account for an observed empirical relationship between democracy and peaceful settlements. Fortunately each of them establishes this linkage through an intervening factor that can be brought into the analysis design and tested directly. I shall briefly elaborate each of these alternative linkages.

ALTERNATIVE EXPLANATIONS OF PEACEFUL SETTLEMENTS

My central hypothesis is challenged by three alternative explanations of peaceful settlements that confer causal importance to (1) political or military alignment, (2) efforts of mediators, and (3) an absence of prior military hostilities. Because previous research has established each of these factors as directly linked to the democratic status of disputing parties, an empirical relationship between democracy and settlements is left open to multiple interpretations. While I regard each of these interpretations as plausible alternatives, they are not mutually exclusive, either with one another or with the norm-based account I have given. I provide a schematic overview of these alternative causal paths in Figure 1, using filled arrows to represent hypothesized relationships and unfilled arrows for relationships supported by previous research. Thus my principal hypothesis is depicted by the filled arrow running from democracy to settlements in the central part of the figure. Unfilled arrows connect democracy to alignment, mediation, and hostilities to signify established empirical relationships. The remaining filled arrows on the right portray the three alternative causal linkages. Let us now examine each of these intervening effects.

To begin, I assume that prospects for peaceful



settlement will depend on alignment, which I conceptualize broadly to encompass the general atmosphere of political relations between disputant parties. Certainly states with historically friendly interactions or those that maintain some areas of amicable relations even while engaged in disputes will find it easier to reach settlements than actors with longstanding or broadly defined animosities (Bercovitch, Anagnoson, and Wille 1991; Miall 1992). While I am unable to undertake a full assessment of political relations among potential disputants, I can gain some leverage over this hypothesized relationship by narrowing consideration to common memberships in formal alliances. Admittedly this is a rough approximation of the more general notion, though it is one that has been adopted by others to signify the presence of a cooperative, interdependent relationship in the critically important area of national security (e.g., Siverson and Emmons 1991). Moreover, alliance bonds between democratic states have been shown to occur more often than chance alone would predict (ibid.). To summarize, we know from previous research that democratic states are more likely than others to join in an alliance, and we hypothesize that states choosing to ally with one another and to maintain that alliance even during periods of conflict are more likely to reach peaceful settlements than are nonallied states.9

An alternative justification of the democracy-settlement linkage is based on the possible presence of third-party mediation efforts. Intermediaries may consist of other states or coalitions of states, international organizations, transnational or subnational organizations, ac hoc commissions, or even individuals with international standing. Although mediation is formally defined in legal practice, actual usage varies widely (Bercovitch 1992). Following Skjelsbæk, I designate as mediation any third-party involvement "that goes beyond facilitation of communication and contacts and includes drafting and promotion of plans for conflict resolution" (1986, 144). Mediation efforts hinge on the willingness of both disputant parties to entrust in third parties at least some measure of responsibility for shaping valued outcomes. While democratic leaders may not always acquiesce to the intercession of third parties, they are probably inclined to do so under a broader range of circumstances than leaders lacking norms of bounded competition. This speculation is supported by a recent study of postwar disputes showing that conflict management-defined broadly but including mediation—is more likely between democratic opponents (Dixon 1993). In addition, formal alliance ties also appear to enhance the likelihood of mediation, thus accounting for the alignment-mediation arrow in Figure 1. Further empirical support for the democracy-mediation relationship emerges from Raymond's (1992) investigation of third-party resolution efforts.

Although the hypothesized relationship between mediation and peaceful settlement has intuitive appeal, the backing of empirical evidence is surprisingly absent despite an enormous literature assessing the efficacy of management and mediation efforts. Even so, mediation may be especially important in the case of democratic disputants. Several studies do agree that mediation efforts in the postwar era have the greatest probability of success when conflicts are neither ideologically divisive nor intensely violent (Bercovitch, Anagnoson, and Wille 1991; Butterworth 1978; Haas 1983, 1986; Miall 1992). Disputes between democracies meet both criteria: they are obviously not ideologically motivated and the empirical record demonstrates that they are seldom violent. By this account, then, democratic norms have only an indirect effect on peaceful settlement that operates through the intervening presence of third-party mediators.

Finally, I turn to an alternative rationale arising even more explicitly from the empirical findings on the democratic peace. The key finding documented earlier-that democratic states rarely engage one another with military force—is portrayed in Figure 1 by the negatively signed arrow connecting democracy and hostilities. Furthermore, I concur with Miall (1992) that the obstacles to peaceful settlement are likely to become much more formidable once a conflict situation has passed the threshold of major violence. Taken together, the two links in this chain suggest that democracies may achieve peaceful settlements with one another largely because they have managed, for whatever reason, to avoid military hostilities. Inclusion of a control for prior hostilities not only eliminates this particular rationale but also has the benefit of subsuming other rival explanations. Consider, as a prime example, the argument that certain decisional structures of democratic states serve as impediments to war involvement. The structuralist logic does not entail the prediction of a direct democracy effect on peaceful settlements, although it could explain such an effect by appealing to the fact that democracies seldom resort to military hostilities

and it is this absence of any prior hostilities that favors settlements. By controlling for prior hostilities, I also eliminate this structuralist interpretation.

OBSERVATION AND MEASUREMENT

This examination of the democracy-settlement hypotheses requires a sample of international disputes over some reasonably long time span along with information about the governing arrangements of the parties to these disputes. For this initial look at the hypotheses, it is necessary only to determine whether or not some form of settlement was achieved during the evolution of the conflict situation. It is also necessary to operationalize the three intervening factors listed in Figure 1. I shall discuss each of these components of the research design.

Conflicts and Conflict Phases

Conflicts are drawn from an evolving list of post-World War II interstate political and security disputes that for two decades has served as a principal source for systematic studies of international conflict management. Conflict situations represented in the data base were originally identified by Haas (1968) and expanded by Haas, Butterworth, and Nye (1972) for their pioneering studies of international organization management practices. Coded attributes of the disputes and a collection of synopses for a modified set of 247 conflicts updated through 1974 were published by Butterworth and Scranton (1976). A subsequent update through 1979 by Butterworth added another 60 conflicts (Butterworth 1980). Further updates and revisions have been undertaken by Haas (1983, 1986) and Sherman (1993), though I here examine only the 1945-79 period from the files compiled by Alker and Sherman (1986).10

The Alker-Sherman set has several specific advantages for the present study. First, as Alker and Midlarsky (1985) note, the empirical domain is defined so as to correspond to international diplomatic and legal practice. In following Butterworth (and Scranton 1976, 3), the encoding procedures "included only postwar conflicts that centrally involved specific power-political aims and demands having direct impacts on national behavior, and that were perceived internationally as being focused on political and security affairs. Excluded are domestic issues that did not directly lead to interstate conflict, together with more diffuse antagonisms or instances of general interstate competition." Second, actors on the contesting sides of each dispute are itemized individually at various stages during the conflict situation. This procedure ensures that all primary actors, including members of coalitions, are identified regardless of changes in participation over time. Third and most important, by disaggregating conflict situations into distinct phases, this version of the data incorporates the dynamic element required by the theoretical assumptions stipulated. These latter two features are

so distinctive that the Alker–Sherman data are sometimes designated the "phase/actor disaggregated" conflict set. They are also of critical importance to the present effort.

Conflict situations are disaggregated into phases to reflect changing degrees of intensity and antagonism by keying on the potential or actual use of direct military hostilities. The Alker–Sherman set relies on Bloomfield and Leiss's (1969) classification scheme marking six distinct phases through which a conflict may proceed until it is settled or disappears. In recognition that conflicts do not evolve along a single trajectory, the scheme is explicitly nonlinear and iterative by permitting phases to be skipped or repeated as necessary. Phase types include

- A. initial disputes, or disagreements
- B. *conflicts*, in which at least one party exhibits some commitment to military force
- C. hostilities, during which military force is actually used
- D. posthostility conflicts, where fighting has ceased but threats of force remain
- E. posthostility disputes, in which military resolution is no longer a serious possibility
- F. termination, in which the dispute is resolved or no longer of concern to the contesting parties.¹¹

The outbreak of military hostilities thus serves as a kind of watershed phase. Prior to hostilities, phases may cycle between types A and B; after hostilities, cycles may involve types C, D, and E. Type F, termination, may occur at any time. Specific phase transition rules are given by Alker and Sherman (1986, 91).

Phase disaggregation has several important conseguences for empirical analysis. In the first place, transitions from one conflict phase to another may also entail changes in certain other attributes of the conflict situation. For example, specific actions taken by the parties to the dispute may vary dramatically from phase to phase, and even the parties themselves may change (though this is relatively rare and usually involves an additional actor's joining one side or the other). Mediation efforts—another factor of central importance to this study—are rarely pursued in every phase of a conflict. Of the 307 postwar conflicts in the Alker-Sherman set, only about 8% exhibit either the presence or absence of mediation in every observed phase, and even this is inflated somewhat, for when we consider only those 265 conflicts with multiple phases the proportion declines to only 5%. Disaggregation by phases thus affords a unique and valuable perspective for examining the shifting conditions that determine the course of international conflict situations.

In addition, my definition of peaceful settlement as at least temporary accommodation on one or more contentious issues is operationally well suited to this disaggregated phase structure. Settlements represent mileposts of stability and reduced tensions along the unfolding course of a conflict situation. Any subsequent change in the situation, whether toward still further reductions in tension or toward renewed hostilities, would trigger the start of a new conflict phase. Of course, some settlements do succeed in bringing about a full and final resolution that terminates the conflict. Others merely mark the boundaries of phase transitions. For this reason, the conflict phase serves as an especially useful observational device for the study of peaceful settlements.

Finally, use of the conflict phase as the basic unit of observation suggests that many conflict situations will appear more than once in the guise of different phases. Although most disputes (70%) transcend no more than three phases, two do cycle through nine phases and three through eight. Multiple accounting of the same conflict situations entails at least the possibility of nonindependent observations, a condition that could have deleterious implications for subsequent estimation and inference. Because this issue is best considered in the context of specific estimation models, I defer further discussion until it will be possible to assess nonindependence through alternative estimation strategies. At present, I simply note that the potential nonindependence of observations does not appear to have any substantial bearing on the main results I shall present.

Peaceful settlement-my dependent variable-is recorded for each conflict phase with a binary indicator variable that assumes a value of 1 if some agreement was concluded during a given phase and 0 otherwise. 12 As noted, I define peaceful settlement with no distinction between comprehensive and partial agreements. The latter category may include cases of agreements that are deliberately crafted around some unresolved issues as well as those in which transpiring events make certain issues obsolete or subordinate them to new issues. Because I stipulate peaceful settlements to be mutually agreeable arrangements, I do not count those instances in which settlements were imposed with the defeat or collapse of one disputant party. In a few very unusual cases, partial settlements occur even as the conflict escalates or spreads. These, too, are omitted. By this definition, peaceful settlements are neither exceptional achievements nor politics as usual. They occur in about 31% of the 927 conflict phases contained in the Alker-Sherman disaggregated set.

Two of the hypothesized intervening variables hostilities and mediation—are also operationalized in terms of dispute phases. I am able to take account of prior hostilities with a dummy variable signifying whether or not the present phase was preceded by an earlier phase in the conflict situation that had passed the threshold of military violence. Note that by this operational definition, the initial phase in any conflict cannot possibly be designated as having experienced prior hostilities and is therefore automatically scored 0 on this indicator. Although this is a minor point, it will have consequences for later analyses. About 30% of interstate dispute phases were preceded by some form of military hostilities. The presence of mediation is also recorded with a dummy variable. In addition to what is designated as mediation in formal legal usage, I also count the presence of conciliation and arbitration activities. Mediation is somewhat less prevalent than peaceful settlement, with one or more mediation activities present in 22% of the Alker–Sherman conflict phases.

Democracy

A second key feature of the Alker–Sherman disaggregated conflict set is its identification of specific actors comprising the primary parties during each dispute phase. The two sides in the dispute are arbitrarily designated parties A and B as an organizing device. With the identification of disputant actors, it becomes possible to take explicit account of alliance memberships and governing arrangements. For the former, I employ conventional sources and procedures from the Correlates of War Project (Bremer 1992; Small and Singer 1969). Alliance ties are recorded as a dummy variable, with 1 indicating mutual membership in any type of alliance by any state on one side of the dispute with any state on the other. The latter—governing arrangements—requires more extended comment.

While I cannot directly measure democratic norms of bounded competition, I can gauge the competitiveness of national political institutions using the 11point democracy scale predefined in the Polity II data collection (Gurr, Jaggers, and Moore 1989). 13 This instrument is constructed as a weighted summative index combining judgmental codings of four national-level authority traits recorded annually for 134 nations through 1986. Although space does not permit a full presentation of this scale, I can briefly note its four components and their weights. Two of these relate explicitly to the competitiveness of the political system. These include competitiveness of political participation (competitive = 3, transitional = 2, factional = 1) and competitiveness of executive recruitment (election = 2, transitional = 1). While the other factors also relate to competitiveness, they are more directly concerned with constraints. These refer to the openness of executive recruitment (open or partial election = 1) and constraints on the chief executive (parity or subordinate = 4, substantial limitations = 2, slight or no limitations = 0, with two intermediate categories = 3 and 1). Because the notion of democracy underlying this scale emphasizes political institutions, I follow Gurr, Jaggers, and Moore (1990) in referring to it as institutionalized democracy. 14

The appropriate institutionalized democracy score is recorded for the primary parties in each dispute phase for the year in which the phase begins. Several specific points require elaboration. First, because conflict phases may span several years, there is a possibility that governing arrangements may change within a given phase period. Visual inspection of the data suggests that this is not likely to be a serious problem, though it does occur on occasion. Second, because my interest is in assessing the effect of institutionalized democracy on peaceful settlements, I confine attention to cases with formally recognized

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national political institutions. These are cases of strictly interstate disputes, that is, phases in which each party includes at least one nation–state represented in the Polity II collection. This filter eliminates exclusively nonstate actors such as colonial-era independence movements, revolutionary groups, political factions, and the like, as well as a few states failing to meet the Polity II requirement of at least one million population by the mid-1980s. The resulting sample encompasses 264 postwar disputes disaggregated into 718 phases, of which 33% exhibit peaceful settlement.

But strictly interstate disputes are not necessarily bilateral, and this fact raises a third point for elaboration. Nearly 40% of the remaining dispute phases encompass at least one party comprised of multiplestate actors. Others have solved this problem by dismantling coalitions into sets of discrete dyads (e.g., Bremer 1992; Maoz and Russet 1993). While this approach may be adequate for some purposes, I reject it here because it would entail a highly distorted representation of the settlement process. Alternatively, I could rely on some summary value such as the mean of coalition members. The theoretical implications of this procedure are not very appealing either, given that a coalition comprised of equal numbers of highly democratic and nondemocratic states and one of all moderately democratic states might well receive identical mean scores, implying that their behavior would be governed by equally democratic norms. A more theoretically satisfying approach follows from the assumption that the democratic values of the most powerful coalition actor will prevail. Accordingly, I recorded the power of each coalition actor, using the continuous composite indicator of national capabilities compiled by the Correlates of War Project (Singer 1988), and then assigned the entire coalition the value of democracy observed for the most powerful member.15

The empirical elements for evaluation of the democracy-settlement hypothesis are now in place. Procedures outlined thus far yield a reasonably comprehensive collection of strictly interstate dispute phases classified according to the presence or absence of peaceful settlements, along with two institutionalized democracy scores, one for each of the two opposing sides. In cases where a party to a dispute is a coalition of states, I assign the democracy value of the most powerful member. I have also established sufficient controls to expose the effects of democratic norms by accounting for the potential intervening effects of alliance bonds, mediation, and prior hostilities.

ANALYSIS AND RESULTS

The central hypothesis claims that democratic disputants are more likely than others to achieve peaceful settlements. Although a multivariate analysis is required to sort out potential intervening effects, much

TABLE 1

Peaceful Settlements and the Democratic Status of Participants (%)

	PAR'		
PARTY A	NON- DEMOCRACY	DEMOCRACY	TOTAL
Nondemocracy	30.9	33.5	31.9
	(278)	(167)	(445)
Democracy	31.6	46.3	35.2
	(206)	(67)	(273)
Total	31.2	37.2	33.1
	(484)	(234)	(718)

Note: Central cell entries display the percentage of dispute phases ending in peaceful settlement under each combination of democratic and nondemocratic disputant parties. Entries in parenthesis indicate the N size of each cell serving as the percentage base.

can be revealed by a provisional examination based on simple percentages.

Provisional Evidence

Table 1 crosses the democratic status of disputant A by the democratic status of B, with interior cells representing the percentage of dispute phases reaching peaceful settlement. Because the 11-point institutionalized democracy measure is much too unwieldy for this purpose, I temporarily resort to a dichotomized version designating democratic governance at level 6 and above. The resulting cell entries reveal that settlements are achieved in 30.9% of phases involving two nondemocratic disputants, a figure slightly below the overall rate of 33.1%. Phases in which only one of the parties is democratic fare little better at 31.6% and 33.5%. These values stand in sharp contrast to the 46.3% of dispute phases reaching settlement when both parties are democracies.

This initial evaluation is illuminating on two separate counts. First, these results do provide encouraging provisional support for the democracy-settlement hypothesis, though they must be interpreted with caution in the absence of any controls for intervening effects that could also account for settlements between democracies. Probit regression will be used to establish these necessary controls. Second, the pattern of entries clearly demonstrates that further investigation must faithfully represent the joint effects present only when both parties are democratic. For example, if it were presumed that party A's level of democracy affects the probability of settlement independent of the governing arrangements of B, and vice versa, then it would be appropriate to estimate these effects in a model including democracy scores for each party while controlling for the other. Any presumption of independent effects is quite implausible, however, given the pattern of results in Table 1, a pattern consistent with my earlier characterization of peaceful settlements as requiring both disputants'

acquiescence. Thus it is essential for any specification of democracy effects to allow both parties' level of democracy to come into play jointly and simultaneously.

Several forms are capable of representing the joint democracy effect seen in Table 1, but perhaps the most parsimonious procedure is to assume that the norms and expectations most likely to promote or discourage peaceful settlement are those of the less democratic party to the conflict. I began with the assumption that democratic norms would induce states to seek out-or at least acquiesce to-peaceful settlements. This implies that more democratic actors are more favorable to settlement than less democratic actors, other things being equal. In addition, it is already stipulated that settlements are mutually agreeable arrangements and thus require at least tacit approval of the active disputants. Taken together, these assumptions imply that the norms most likely to promote or discourage the potential for peaceful settlement are those of the less democratic disputant. This is surely a simplification, though it does not seem unreasonable as an initial approximation. In addition, other things are seldom equal-a matter I shall take up.

Probit Estimation

It is convenient to assume that conflict phases vary in their latent propensity for peaceful settlement and that this propensity can be modeled as a linear function of democracy along with the three intervening factors. Letting S^* symbolize this unobserved propensity for settlement in phase i, it becomes possible to represent the democracy-settlement hypothesis as estimable parameters in the following specification. Democracy is symbolized by D, with the notation

$$S_i^* = \beta_0 + \beta_1 \min(D_{Ai}D_{Bi}) + \beta_2 A_i + \beta_3 M_{i} - \beta_4 H_i + u_i,$$
(1)

where $\min(D_{Ai}, D_{Bi})$ explicitly denotes the minimum value for parties A and B on the full 11-point scale. The estimated sign and value for parameter β_1 is of special interest because it carries the effect associated with minimum democracy. Three additional terms in the specification, A_i , M_i , and H_i signify controls for alliance ties, mediation activity, and prior hostility, respectively. Finally, all parameters (except the intercept) are signed to be consistent with hypothesized effects.

Now let *S* stand for the observed realization of the latent propensity with values signifying the presence or absence of peaceful settlements defined by some threshold on *S**. As is the usual practice, this threshold is set at 0, establishing the relationship

$$S_i = \begin{cases} 1 & \text{if} \quad S_i^* > 0 \\ 0 & \text{if} \quad S_i^* \le 0. \end{cases}$$

This leads directly to a binomial probability model such that the probability of settlement (i.e., $Prob[S_i =$

1]) can be expressed as some function of the righthand side of equation 1 with the exact functional form determined by the expected distribution of the error term, u. For the present analysis, I assume u_i to be distributed normally, which defines the usual probit model.

I begin the probit estimation by examining minimum institutionalized democracy alone before introducing additional terms to accommodate potential intervening effects. Maximum likelihood estimates are displayed in Table 2, column 1. I report one-tailed significance tests as a guide to the robustness of coefficients because there are clear expectations of the direction of effects. With a parameter estimate of .044 a magnitude more than twice its standard error of .019-it is immediately evident that minimum democracy may well impart a systematic positive influence on the probability of peaceful settlement. Even so, the summary chi-squared value indicates that the fit of this initial model is not particularly strong, suggesting that democracy is probably not the principal determinant of settlements.

In order to infer a role for democratic norms, it is necessary to control for the intervening factors given primacy by alternative explanations. If the democracy estimate represents a causal effect operating entirely through the more proximate influence of alignment, mediation, or the absence of prior hostilities, then the observed model 1 effect should disappear with the introduction of these control variables. Results implementing these controls appear in column 2. Clearly the democracy effect remains undiminished. In fact, it is moderately strengthened in this specification. In addition, the fit is improved substantially and both alignment and mediation are properly signed. The one unexpected result in model 2 is the positive (and therefore nonsignificant) estimate associated with prior military hostility. Although it would be possible to develop a post hoc rationale for the proposition that hostilities actually increase the likelihood of settlements, I believe this result is better explained as a statistical reflection of my coding conventions.

A positive relationship occurs because prior hostilities are definitionally absent (hence coded as 0) in the initial phase of every conflict situation. Indeed, close to one-half of all observed phases without prior hostilities are initial phases. In order for hostilities to carry the hypothesized negative effect, settlements would have to be a fairly common occurrence at this initial stage. Yet close to 90% of settlements are reached after the first phase, a marginal distribution that masks any underlying negative relationship. The solution is to adjust the prior hostilities estimate by allowing it to vary across the ordinal sequence of phases in the conflict situation. I do this by introducing two additional controls: a sequential phase counter and the product of this counter and the prior-hostilities dummy variable. To simplify subsequent interpretation, I begin the phase counter at 0. Thus the initial phase of each conflict is recorded as 0, the second phase as 1, the third as 2, and so on through a maximum value of 8, indicating nine

TABLE 2 The Effects of Minimum Democracy on Peaceful Settlement

VARIABLE	MODEL 1	MODEL 2	MODEL 3ª	MODEL 3A
Minimum democracy	.044** (.019)	.058** (.020)	.066** (.021)	.057* (.026)
Alliance between parties		.339** (.100)	.391** (.104)	.443** (.119)
Mediation		.362** (.118)	.306** (.121)	.171 (.135)
Previous military hostility		.517 (.107)	.693 (.221)	.309 (.291)
Sequential phase			.643 (.084)	.397 (.148)
Previous military hostility × sequential phase			527** (.103)	280* (.159)
Constant	510** (.059)	951** (.091)	-1.493** (.122)	-1.084** (.231)
Chi-squared(df) Log-likelihood	5.2(1)* 453.5	53.3(4)** -429.4	121.2(6)** -395.5	32.6(6)** -310.7

Note: Main entries are probit regression estimates; entries in parenthesis are standard errors. All estimations are performed over 718 conflict phases except

for model 3A, which excludes the initial phase of each conflict situation and reflects 477 conflict phases.
"Estimates and significance tests for previous hostilities and sequential phase must be interpreted conditionally to account for statistical interaction.

* $p \le .05$, one-tailed test. ** $p \le .01$, one-tailed test.

phases. 16 This revised specification models prior hostilities as a conditional effect that is no longer constrained to the positive by the marginal distributions of prior hostilities and settlements. However, this interpretation holds only if the product term produces a negatively signed estimate.

Results for this revised specification are listed in column 3. First note that while there is some modification of the estimates from model 2, the differences are not substantial. Perhaps most interesting is the democracy estimate, which once again exhibits a moderate increase in magnitude. The key result of model 3, however, is the negative and highly robust estimate for the product term along with a doubling of the chi-squared goodness-of-fit measure. The prior hostilities estimate remains positive, although this is no longer a problem because the reported coefficient applies only to the initial phase, in which prior hostilities are definitionally absent and settlements are rare. This interpretation follows from the presence of the interaction term. The prior-hostilities effect must now be assessed at each phase using the expression .693 - .527P, where P takes on values assigned to the sequential phase counter. Herein lies the advantage of beginning the phase counter at zero: at the initial phase (P = 0), the second term drops away, leaving only the reported estimate of .693. At the second phase (P = 1), the hostilities effect is still positive (.166) but no longer significant (t = 1.0). However, by phase 3 (P = 2), the estimate switches sign (-.361, $\dot{t} = -2.3$) and remains negative and statistically significant at all subsequent phases.

Estimation of prior hostilities as an effect condi-

tioned on the phase sequence thus accommodates both the theoretically expected negative path portrayed in Figure 1 and the marginally constrained positive relationship at the initial phase. Of course prior hostilities cannot possibly have any impact during the first phase of a conflict, so this positive effect is better viewed as a statistical control than a result of any substantive importance. This is not to say that the phase 1 positive relationship is in any sense illusory or unreal. After all, phase 1 observations do legitimately qualify as having no prior hostilities, and this coding convention, coupled with the marginal distribution of settlements, is accurately reflected by a positive relationship at phase 1. The model 3 specification explicitly incorporates this relationship as a statistical control to minimize its impact on other variables of interest. An alternative means of control is simply to omit all phase 1 observations from the estimation. Although this procedure sacrifices degrees of freedom and truncates the variance on important variables, it does nevertheless provide a useful supplement to the statistical controls implemented in model 3. Estimates from this alternative procedure listed in the last column (model 3A) no longer show any trace of the significantly positive hostilities effect found in earlier estimations, a finding quite consistent with the account just given. Moreover, with the exception of mediation, the other estimates are not severely affected by the loss of observations. The democracy effect in particular is only slightly diminished from that found for the full set of observed phases.

On the whole, the various estimates in Table 2 are

consistent with all four causal paths depicted in Figure 1. There is certainly no evidence to support the view that democracy can only influence the probability of settlements through one or more intervening factors. In fact, the changing magnitudes from models 1–3 suggest that the direct impact of democracy is actually suppressed by these factors, albeit only marginally.¹⁷

Phase Disaggregation and Estimation

Before relying too heavily on the estimate of Table 2, it should be recalled that the repeated appearance of conflicts in the Alker-Sherman phase-disaggregated set raises the possibility of cross-phase dependence of observations, a condition that can have detrimental consequences for estimation. I have elsewhere argued that cross-phase dependence is analogous to cross-time dependence in least squares time-series regression (Dixon 1993). Cross-phase autocorrelations can be constructed from probit residuals (defined as the difference between the observed binary dependent variable and its predicted probability value) by first lagging across phases within conflict episodes and then correlating these residuals at different lags. Unfortunately, phase-lagged residuals for the model 3 specification of Table 2 confirm cross-phase dependence by exhibiting a high degree of autocorrelation.

The earlier study demonstrated that significant cross-phase autocorrelation could be substantially reduced by inclusion of a lagged dependent variable in the probit specification. Maoz and Russett (1993) rely on a similar strategy to contain the nonindependence of continuing conflicts in a pooled time-series analysis over annual observations. Because peaceful settlements do not necessarily terminate a conflict situation, it is feasible to include settlements lagged by one phase on the righthand side of the estimation. This modification produces a strong positive estimate for the lagged term and improves the overall fit with no material effect on the original estimates or conclusions drawn from them. Surprisingly, it also has little effect in attenuating the phase-lagged autocorrelated residuals.

I offer two alternative estimation strategies to assess the impact of this nonindependence on the estimates of model 3. The first assumes that crossphase dependence is likely to increase in direct proportion to the number of phases parsed from a conflict episode. I performed a weighted probit estimation of the model 3 specification using a weight factor of 1/np, where np represents the number of phases in each conflict situation. This procedure gives full weight to nonproblematic observations representing single-phase conflicts and reduces the impact of potentially dependent observations drawn from multiple-phase conflicts. Although a total of 718 phases are used in the estimation, the weighting scheme redefines the N size to be 264, equivalent to the number of conflict episodes. Results from this phase-weighted estimation in Table 3, column 1,

remain very close to the earlier estimates. While this exercise has not removed cross-phase dependence from these observations, it has at least diminished its influence and provides a preliminary indication that the estimates and hypothesis tests in Table 2 were not unduly affected by phase disaggregation.

Unlike phase-weighted estimation, the second strategy is able to purge all traces of cross-phase dependence among observations, though it extracts a price for doing so. Here I engage in a series of estimations performed over individual phases based on their temporal sequence in the conflict episode. Results for the first three phases (nothing useful is gained beyond this number) are presented in the columns 2-4. Before turning to these estimates, three implications of this approach merit attention. First, each successive estimation loses degrees of freedom by reducing the number of observed phases. All 264 conflict situations have an initial phase, but only 214 pass on to a phase 2. The decline becomes more rapid at phase 3 and beyond, dropping by about half (129, 61, 28, 14, etc.) at each successive phase. Second, single-phase estimation requires modest respecification. Most obviously, the sequential phase indicator ceases to vary and therefore must be omitted from the right-hand side, along with the phase-hostility product term. In addition, previous military hostility must be removed from the phase 1 estimation, since there could have been no prior hostilities during a conflict's initial phase.

Third and most important, limiting attention to single phases is likely to affect the estimates by truncating variances of the right-hand-side terms. This is especially true of the main variable of substantive interest, minimum democracy. According to the theoretical rationale advanced earlier, highly democratic states will be more likely than others to reach peaceful settlements at an early stage in a conflict and such settlements will frequently even terminate a conflict situation altogether. Thus the very effectiveness of mutual democracy in promoting settlements implies that conflicts between highly democratic parties will seldom transcend more than two or three phases. In fact, nearly 80% of observations having a minimum democracy score of at least 6 occur prior to phase 3, and in only one instance does this level of minimum democracy appear in a conflict extending beyond a fourth phase.

This truncation effect is clearly seen in the single-phase estimates in Table 3, where minimum democracy retains a strong positive estimate at phases 1 and 2 that declines virtually to zero at phase 3 (and beyond). This is an important result in two respects: (1) taken in conjunction with the overall estimates in Table 2, it adds further empirical weight to my theoretical account of democratic conflict; and (2) (more germane to the issue at hand) since cross-phase dependence cannot possibly affect these single-phase estimates, we can be confident that minimum democracy does increase the probability of settlement, at least at these early phases. Furthermore, given the larger magnitudes of these democracy estimates compared

TABLE 3 Peaceful Settlements and Phase Disaggregation

	PHASE-WEIGHTED	SINGLE-PHASE ESTIMATES ^b			
VARIABLE	ESTIMATES ^a	PHASE 1	PHASE 2	PHASE 3	
Minimum democracy	.076**	.088**	.109**	088	
	(.035)	(.034)	(.035)	(.052)	
Alliance between parties	.354*	.245	.433**	.422*	
	(.173)	(.202)	(.182)	(.226)	
Mediation	.375*	.818**	.508**	023	
	(.198)	(.234)	(.215)	(.252)	
Previous military hostility	.701 (.369)	-	.097 (.245)	344 (.226)	
Sequential phase	.664 (.138)	produce.		*********	
Previous military hostility × sequential phase	583** (.177)	- Marine	- MARIAN		
Constant	-1.413**	-1.561**	−.862**	125	
	(.187)	(.173)	(.160)	(.217)	
Chi squared(df)	43.0(6)**	20.8(3)**	21.7(4)**	6.1(4)	
Number of observations	264	264	214	129	

Note: Main entries are probit regression estimates; entries in parenthesis are standard errors.

Phases are weighted by 1/np, where np represents the number of phases in each conflict episode. The 718 observed phases thus reduce to 264 weighted observations for this estimation.

bEstimations are over individual phases based on temporal sequence. Thus, the observations in the first column represent initial phases in each of 264 conflict episodes irrespective of phase type.

 $p \le .05$, one-tailed test. $p \le .05$, one-tailed test.

to those in Table 2, it appears that the estimation strategy followed in Table 2 using all phases is actually a conservative one and that if cross-phase dependence has any effect on the estimates it is to diminish them.

Further Tests

The previous phase-weighted and single-phase estimations have shown that the evidence of a direct democracy effect on peaceful settlement is not unduly influenced by cross-phase dependence. Here I continue this critical scrutiny by entertaining a series of substantive hypotheses further challenging the results and interpretation of Table 2. These highlight the potential confounding of exogenous influences from economic development, the military balance, and geographic contiguity. I also consider the possibility that underlying relationships are obscured by my treatment of coalitions of actors as disputant parties. In each case, the challenge is posed by the introduction of additional terms that could attenuate the democracy estimate or alter its interpretation.

First, I examine one of the chief rival hypotheses to democracy as a factor discouraging state involvement in serious conflict or war (Bremer 1993; Maoz and Russett 1992, 1993). The argument arises from the conventional proposition that economic development is a prerequisite for (or at least a correlate of) democracy (e.g., Lipset 1959). Coupled with the further assumption that prospects of settlement are enhanced when both disputants are highly developed (perhaps due to bonds of commerce, costs of disrupted financial flows, or other ties of interdependence; see Dixon 1984), we are confronted with the possibility that the observed democracy effect is actually spurious, with economic development as the true causal agent. I gauge economic development using logged energy production per capita (for coalitions, it is the maximum level of any single member) drawn from the Correlates of War capabilities data base (Singer 1988). Then, following the same logic as with democracy, I introduce the minimum of the two development measures into the estimation. The results reported in Table 4, column 1, show that economic development fails to reach significance and has little bearing on the previously observed democracy estimate.

The relative power capabilities of disputants is commonly featured in studies of conflict and conflict resolution, though there is little consensus on its most likely effects. Some analysts have found settlements to be more likely between adversaries of roughly equivalent power (Bercovitch, Anagnoson, and Wille 1991). Alternatively, from a realist standpoint the chances of a state prevailing in a dispute would be proportional to its power, other things being equal. Thus, the greater the power disparity,

TABLE 4

The Effects of Minimum Democracy on Peaceful Settlement Controlling for Economic Development,
Capability Ratio, Geographic Contiguity, and Dyadic Disputes

VARIABLE	MODEL 4	MODEL 5	MODEL 6	MODEL 7
Minimum democracy	.59*	.055*	.062*	.108*
	(.022)	(.022)	(.021)	(.039)
Alliance between parties	.382*	.372*	.409*	.375*
	(.107)	(.105)	(.106)	(.105)
Mediation	.306*	.316*	.308*	.318*
	(.124)	(.122)	(.121)	(.122)
Previous military hostility	.706	.694	.694	.681
	(.225)	(.221)	(.221)	(.222)
Sequential phase	.664	.648	.644	.646
	(.086)	(.084)	(.084)	(.084)
Previous military hostility × sequential phase	545*	−.529*	−.528*	525*
	(.105)	(.103)	(.103)	(.103)
Economic development	.11 (.024)	-	· —	_
Capability ratio		.018 (.012)	-	-
Geographic contiguity	· · · · · · · · · · · · · · · · · · ·		105 (.120)	· <u> </u>
Dyadic phase		,	-	009 (.130)
Dyadic phase × minimum democracy		AMPER	. .	056 (.046)
Constant	-1.770*	1.488*	-1.419*	-1.490*
	(.212)	(.122)	(.147)	(.147)
Chi-squared(df)	125.2(7)*	123.7(7)*	122.0(7)*	123.5(8)*
Log-likelihodd	-384.5	-394.3	-395.1	-394.3

Note: Main entries are probit regression estimates; entries in parenthesis are standard errors. All estimations are performed over 718 conflict phases except for model 5, where there are 701 phases.

* $p \le .05$, two-tailed test.

the less likely it is that the stronger actor will acquiesce to a negotiated settlement. Finally, Miall reports "no significant association" between power disparity and settlements from a comparative study of postwar disputes (1992, 120). It is equally unclear how power might be related to democracy. It is true that many powerful states are also democratic, but so are many middle and small powers. Nevertheless, confounding effects could arise if we assume that powerful democracies are more likely to engage in disputes than less powerful ones, based on the extent of their global reach and interests. Power is measured (as before) with the Correlates of War composite capabilities index, with coalitions assigned the highest value of any member (Singer 1988). I arrange individual power scores so as to differentiate the stronger and weaker parties and then take the ratio of the stronger to the weaker as my summary indicator of power disparity. This variable is entered in Table 4, column 2. It too fails to achieve significance and does little to alter the democracy estimate.

Next, I examine geographic proximity, another factor that has traditionally received considerable attention in studies of interstate conflict (Bremer 1992, 1993; Maoz and Russett 1992). Because most wars and militarized disputes are known to occur between contiguous states (Bremer 1992), we might expect proximity to contribute to the seriousness of the dispute and thereby lessen the chances of reaching a settlement. Although this seems the most likely effect based on previous research, it is possible that once states become embroiled in disputes, contiguity might actually have a pacifying influence. This might arise from shared cultural norms and a degree of familiarity that serve to increase the prospects of peaceful settlement. Proximity may confound the democracy-settlement effect because it could also be associated with democracy, not in a direct causal sense, but rather through diffusion processes in which democratic forms are most readily conveyed across the common frontiers of contiguous states. For this analysis, I follow Bremer (1993) in counting states

as contiguous if they share direct land and river borders or if they are separated by less than 150 miles of water. This is accomplished with a dummy variable assuming a value of 1 if any state on one side is contiguous with any opposing state. The results listed in Table 4, column 3, reveal no evidence that contiguity either enhances or hinders prospects of peaceful settlement.

Finally, I consider a potential problem of specification. Throughout, I have treated coalitions of states as disputant parties in the belief that this procedure better represents actual decision processes than the common alternative of disaggregating coalitions into constituent dyads. The question addressed here is whether or not the observed democracy-settlement effect differs between purely dyadic disputes and those involving coalitions on one or both sides. I examine this question with a dummy variable coded 1 for phases in which both parties are comprised of single states, along with the product of this variable and democracy. The product term estimate is of particular interest because if found to be significant it would imply that underlying the previously observed democracy estimate are two different effects that obtain, depending on the composition of disputant parties. This procedure is equivalent to performing two separate estimations, one for purely dyadic phases and one for all others. However, it has the advantage of incorporating a formal test of the different-effects hypothesis, based on the product term. The dyadic phase dummy and its product with democracy are included in the model 7 results in Table 4, column 4. Although the minimum democracy estimate is augmented slightly in this specification, the product term fails to reach even a minimal level of statistical significance. I conclude from these estimates that the minimum democracy effect on peaceful settlement is unaffected by the existence of coalitions on either side of the dispute.

Democracy and the Probability of Settlement

In all of these tests, the effect of minimum democracy on peaceful settlement has remained remarkably stable. But how should we interpret this effect? The probit estimate of .066 in Table 2, column 2, is not directly interpretable in the same way as a least squares estimate, though it does carry comparable information. For example, the probit estimate can be easily transformed into a slope measuring the effect of changes in democracy on predicted probabilities. In the probit case, however, these probabilities follow the familiar S-shaped curve of the cumulative normal distribution. A probit slope, which is calculated as a partial derivative along this curve, must be evaluated at particular values of the variables comprising the probit function. Evaluated at the means, this corresponds to a slope value for democracy of .02 (t-ratio = 3.2). In other words, the instantaneous effect of democracy on the probability of settlement is .02 at

Although the slope may be a useful interpretative

device, it is not always intuitive. From a substantive standpoint, what we really want to know is how institutionalized democracy affects the probability of peaceful settlements net of other factors in the model. The qualifying phrase "net of other factors" means to hold those factors constant at some value while allowing levels of democracy to vary. This is exactly what is done in Table 5. The range of democracy scores is listed at the left, and all other columns contain predicted probabilities of settlement at each level of democracy.

Table 5, column 1 holds all variables in the model at their mean values. This range of probabilities begins at .36 for cases in which neither party is judged to have even minimally democratic institutions and rises steadily to just over .6 for cases in which both parties are rated highly democratic. One might argue that there is little to cheer in the fact that even dispute phases involving the most democratic states stand, on average, only slightly better than a 50-50 chance of reaching peaceful settlements. Furthermore, if even the strongest democratic norms can do only marginally better than a coin flip, what does this imply about the explanatory power of a norm-based approach? These are valid concerns, but before they lead us to a final judgment regarding the efficacy of democracy or the explanatory value of norms, two additional points deserve attention.

First, it is important to keep in mind that the overall probability of settlements occurring in any given phase is only .33. The odds of peaceful settlements are thus nearly doubled in disputes between highly democratic states. Second, while means may provide a good overall summary of probabilities, they are not necessarily the most informative values and many times may not be even a realistic representation. Because alliance ties, prior hostilities, and mediation are recorded only as binary indicator variables, their means are equivalent to the proportion of dispute phases in which each factor is present. But by my definitions, these factors must be either present or absent during a phase-information not carried by the mean. The remaining columns of Table 5 rectify this by showing variations in predicted probabilities of settlement under conditions defined by the combined presence or absence of alliance bonds between parties, a prior history of military hostilities, and mediation activity.

Although these eight columns of probabilities contain a great deal of information, the main implications can be quickly summarized. While the range and increments of probabilities remain roughly the same in each column, the marked shifts upward and downward demonstrate that democracy is by no means the only factor influencing settlements. We know from the results of model 3 that the most favorable conditions for settlement occur when disputants are joined in a common alliance, when the conflict has not exhibited prior hostilities, and when some form of mediation activity is taking place. The combined effect of all three conditions is represented in the first column to the right of the mean values. Under

TABLE 5

Predicted Probabilities of Peaceful Settlement at Varying Levels of Institutionalized Democracy

		ALLIANCE			NO ALLIANCE				
•		NO HO	STILITIES ^b	HOSTILITIES		NO HOSTILITIES ^b		HOSTILITIES	
DEMOCRACY MEAN		MEDIATION		MEDIATION		MEDIATION		MEDIATION	
SCORE	VALUE"	MED.	NO MED.	MED.	NO MED.	PRESENT	ABSENT	PRESENT	ABSENT
0	.36	.54	.42	.22	.14	.38	.27	.12	.07
Ĩ	.38	.56	.44	.24	.15	.41	.29	.13	.08
2	.41	.59	.47	.26	.17	43	.32	.15	.09
3	.43	.61	.49	.28	.19	.46	.34	.16	∙.10
4	.46	.64	.52	.30	.20	.49	.37	.18	.11
5	.49	.66	55	.32	.22	.51	.39	.20	.12
` 6	.51	.69	.57	.35	.24	.54	.42	.22	.14
7	.54	.71	.60	.37	.26	.56	.44	.24	.15
8	.57	.73	.62	.40	.29	.59	.47	.26	.17
9	.59	.75	.65	.42	.31	.62	.50	.28	.19
10	.62	.77	.67	.45	.33	.64	.52	.30	.21

Note: Probabilities are calculated from the estimates in model 3 of Table 2 while holding all variables but democracy constant at the specified value. Med = Mediation.

this favorable combination of circumstances, highly democratic states become quite likely to achieve settlements while even nondemocracies can expect a better-than-even chance. Now compare these probabilities with those under exactly the opposite conditions (no alliance, prior hostilities, and no mediation), listed in the far righthand column. Faced with these conditions, even the most democratic states would be hard pressed to conclude settlements. Moreover, the last two columns on the right are distinctive in that it is only here that the highest levels of democracy fail to achieve at least the overall baseline probability of .33. At the same time, we should note that democratic disputants will seldom find themselves in the circumstances represented by these two columns, because they will seldom, if ever, have resorted to military hostilities.

Another interesting feature of the probability values in Table 5 is the near-perfect linearity in each of the columns, with probability increments varying between .02 and .03 occurring at each step along the democracy scale. Although probabilities calculated from probit estimates often follow an S-shaped curve, such a pattern is not strongly in evidence here. 19 Consider the implications of a curved probability distribution: relatively small probability increments to either side of especially large increments would suggest the presence of a sort of threshold effect. But no clear threshold appears to underlie the values in any of the columns. On the contrary, it is evident that the probability of peaceful settlements is expected to improve by about the same amount with every increase in institutional democracy, an expectation that applies with equal force to the least democratic and most democratic states.

CONCLUSION

I began with the recognition of two documented empirical facts: (1) that democratic states engage in wars and violent conflict about as often as other types of states, and (2) that democracies virtually never fight one another. To explain the behavior underlying these observations, I developed a theoretical rationale centered on the universal democratic norm of bounded competition. This rationale not only accommodates both empirical facts concerning war and militarized conflict but also entails the novel prediction that democratic states embroiled in disputes will be more likely than others to achieve peaceful settlements. I evaluated the hypothesized relationship between democracy and peaceful settlement against the historical record of postwar international disputes as a test of this norm-based explanatory logic.

The analysis required a compilation of interstate disputes disaggregated by conflict phases in order to accommodate the theoretical process underlying the norm-based rationale and to capture empirical variation in peaceful settlements. I acquired disputes meeting these requirements from the Alker-Sherman conflict set covering the 1945-79 period. Judging peaceful settlements to be most affected by the less democratic party to a dispute, I assessed democracy as the minimum value on the composite index from the Polity II data collection. I then used probit regression to estimate the effects of this minimum democracy measure on the probability of settlement, while partialing out a series of controls to eliminate alternative explanations based on the potential intervening role of common alliance memberships, thirdparty mediation, and the absence of prior military

[&]quot;All variables are set at means except for the sequential phase-hostility product term, which is the product of the means, rather than the mean of the product.

The phase-hostility product term is set at 0 to reflect the zero value on the hostility indicator variable. The phase-hostility product term is set at the mean for phases coded 1 on the hostility indicator variable.

hostilities. The empirical results revealed that democracy does carry the direct positive effect on settlement anticipated by the norm-based logic.

The theoretical significance of this study can be stated directly. The democracy–settlement hypothesis examined here rests on an explanatory logic emphasizing norms of dispute resolution held by democratic leaders. Alternative explanations of democratic war involvement (particularly those focusing on decisional constraints) cannot easily account for the democracy–settlement results and would likely be stretched beyond recognition by any attempt to do so. This study thus represents a modest step in the direction of a norm-based theory of democratic dispute resolution. It is also encouraging that others are beginning to report results consistent with the norm-based approach (Maoz and Russett 1993).

Naturally this study alone cannot settle the matter. Certainly improvements are possible in my crude measurement procedures, and we should inquire as to whether these results hold up under a more extensive temporal domain. While the single-phase results reported afford an initial glimpse of how mutual democratic norms impede the escalatory process at an early stage of an evolving conflict situation. the theoretical and empirical implications of this effect surely deserve fuller attention. Perhaps most importantly, additional work is required to flesh out the internal structure and content of a true theory of democratic norms. Such a theory might even help account for other forms of interaction between democratic states, such as trade (Dixon and Moon 1993). Obviously, much remains undone. In the meantime, however, this study has given us further reason to hope for an expanding zone of democratic peace not altogether unlike the one Kant envisioned some two centuries ago.

Notes

An earlier and much different version of this paper was presented at the 1992 meeting of the American Political Science Association in Chicago. I am grateful to Alison Bailin, Thomas Cusack, David Gibbs, John Oneal, James Lee Ray, Bruce Russett, Michael Sullivan, and Thomas Volgy for comments on previous versions and to Shu-ling Liu and Katrin Gaertner for research assistance. This study was completed with partial support from the Wissenschaftszentrum Berlin für Sozialforschung and the National Science Foundation (SES 9211364).

1. Miall makes the point clearly when he observes: "Many conflicts are never settled at all, but simply lapse or become merged into other disputes. Since states are often unwilling to accept outcomes they consider unsatisfactory, but may not wish to actively prosecute conflict, it is common for conflicts to be allowed to lapse" (1992, 44-45). Thus one alternative to my claim that democracies avoid violence by reaching settlements is that democracies avoid violence because their disputes simply lapse.

2. Rummel (1979, 1983) is the most prominent exception, though most now regard his findings of democratic pacifism to be idiosyncratic to the time period studied. More recently, Morgan and Campbell (1991) have shown that decisional constraints common to democratic systems may inhibit war, but only for a relatively small subset of great powers. For

states more generally—and particularly for the vast majority of lesser powers—constraints had no effect.

- 3. These are fairly broad categories of explanation and my characterization is not meant to minimize the diversity within them. For example, while both Doyle's (1986) Kantian liberalism and Rummel's (1979, 1983) libertarian theory emphasize norms and values, they differ dramatically in their interpretation of these key elements. Likewise, rational choice assumptions lead both Lake (1992) and Bueno de Mesquita and Lalman (1992) to focus on institutional constraints but the former employs a rent-seeking formulation, whereas the latter authors rely on a logic of expected utility and strategic choice.
- 4. Exceptions are beginning to appear, such as Lake's (1992) examination of success in war rather than war involvement, Leng's (1993) study of influence strategies, and Mintz and Geva's (1993) innovative experimental work.
- 5. This strategy bears some similarity to Lakatos' (1970) widely quoted criteria for falsifying theories. I do not specifically invoke a Lakatosian justification, for the obvious reason that research on the democratic peace is too theoretically diverse to comprise a coherent research program (see n. 2). For a similar emphasis on expanding the scope of empirical applications, see Lauden 1977.
- 6. My conception of democracy draws heavily on Schmitter and Karl 1991 but is contoured to the purpose at hand and may well be too narrow for other uses.
- 7. A survey of 307 postwar disputes compiled by Alker and Sherman (1986) reveals that only about 11% (34) actually began with the outbreak of military hostilities. Another 49% (151) began with less severity but did eventually escalate to military hostilities.
- 8. This parallels the common-knowledge assumption made by Bueno de Mesquita and Lalman (1992) and Starr (1992). My emphasis, however, is on this knowledge as a basis for contingent consent rather than as a signal of democratic "dovishness."
- 9. This reasoning may appear to contradict reports that alliance ties actually increase the probability of war (Bueno de Mesquita and Lalman 1992). More recent evidence challenges this claim (Bremer 1992), but even if true it is not inconsistent with my proposition that once allies enter disputes they are more likely to reach settlements than are nonallied opponents.
- 10. This collection is sometimes informally designated the FACS data, meaning Farris–Alker–Carley–Sherman (individuals associated with the project). I refer to the data as the Alker–Sherman set, though this is not intended to diminish the contributions of others, including those noted in the text. The data were acquired from the Inter-University Consortium for Political and Social Research.
- 11. I use letters rather than Bloomfield and Leiss's (1969) roman-numeral phase designation to avoid any confusion with the ordinal sequence in which phases occur within a conflict situation. I also use termination, rather than the original settlement, for phase type F to avoid terminological ambiguity with the mutual agreements that serve as my dependent variable. Many conflict situations do end with such agreements, but others do not. Similarly, many agreements address only some of the contending issues and therefore do not terminate the conflict situation. Some further ambiguity may surround use of the terms conflict and dispute. Throughout, I employ both terms interchangeably when referring to the entire episode inclusive of its phases; the more specific usage will be made clear with appropriate modifiers.
- 12. This is based on a reorganization of categories from Alker and Sherman 1986, 34.
- 13. These data were generously supplied by Will Moore. For further details on the component measures and their weights, see Gurr, Jaggers, and Moore 1989. It is also worth noting that Polity II and its predecessor (Gurr 1974), are routinely used in democracy—war studies (e.g., Maoz and Abdolali 1989; Maoz and Russett 1993; Morgan and Campbell 1991; Morgan and Schwebach 1992; Siverson and Emmons 1991).

14. I generally rely on the full 11-point democracy scale, though I shall be making limited use of a dichotomized version. Maoz and Russett (1993) propose an elaboration of this measure by combining it in an index with similarly constructed Polity II indicators of autocracy and power concentration. Their index has the advantage of discriminating among the many types of nondemocracies clustered at scale points 0 and 1. For my purposes, however, this is a needless complication since my theoretical argument has focused exclusively on the democratic dimension. Empirically, it probably matters little one way or another, giver the high correlation (.90) between the two measures.

15. The Correlates of War capabilities index uses a multiple indicator approach to represent military, economic, and demographic dimensions of national power by recording each actor's share of total system resources in each area and then

taking the average of all three.

16. The sequential phase registered by the phase counter should not be confused with the phase type, though the two are not completely independent of one another due to the posthostilities requirement of phase types D and E. Thus an initial phase in a conflict could be any one of types A, B, or C.

17. Because I concur with Bollen (1991) that continuous measures of democracy are preferable to simple dichotomies, the Table 2 analyses are based on the full 11-point scale. However, I also performed all analyses in Table 2 with the dichotomous measure used in Table 1. I do not report them in a separate table since for all variables except minimum democracy the results are virtually identical to those in Table 2. The dichotomous democracy indicator parallels its continuous counterpart, yielding statistically significant estimates of .38, .45, .51, and .41 for models 1, 2, 3, and 3A, respectively.

18. I relied heavily on data from Gochman 1991 supplied by Stuart Bremer and on information from Siverson and Starr 1991 supplied by Randolph Siverson. I am most grateful to

both.

19. A graph of these values reveals a generally linear shape with only a very slight curvature.

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ELECTORAL COMPETITION WITH INFORMED AND UNINFORMED VOTERS

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present a model of electoral competition in which candidates raise campaign contributions by choosing policies that benefit interest groups and then expend those contributions to influence voters who are uninformed about the policies. Informed voters, however, vote based on those policies, so candidates face a trade-off between choosing a policy to generate funds to attract the uninformed vote and choosing a policy to attract the informed vote. Electoral equilibria are characterized for two categories of policies: particularistic and collective. In the case of particularistic policies, the equilibrium policies of the candidates are separated if the proportion of uninformed voters is sufficiently high, and the degree of separation is an increasing function of that proportion. The model is extended to include the public financing of elections and incumbency advantages. For the case of collective policies, the candidates locate at the median of the ideal points of the informed voters, and contributions are zero.

wo characteristics of U.S. elections are the substantial campaign expenditures by candidates and the considerable voter ignorance of both issues and candidate positions on those issues. Some campaign funds are used to cover the costs of staff, transportation, supplies, and additional fundraising, but a major portion of campaign expenditures goes for advertising and other informational activities that may influence or persuade some voters. Electoral competition thus involves candidates who raise campaign contributions from interest groups and individuals through their efforts as well as the positions they take. Those positions also affect the voting of those who are informed about candidate positions.

To investigate the interactions among interest groups and their campaign contributions, candidate positions, and the electorate, two categories of voters are considered: informed voters are assumed to know the policy positions of the candidates and vote based on those positions; uninformed voters are assumed not to know those positions, and their vote is influenced by campaign expenditures. Campaign contributions thus have a productive role as candidates compete for the uninformed vote by raising contributions from interest groups and others. Those contributions depend on the policy positions candidates choose, and those positions affect the votes of informed voters. To the extent that positions affect voting directly (as well as indirectly through their effect on the raising of campaign funds), candidates face competing incentives to seek the informed and the uninformed votes. Electoral competition is driven by these incentives, and an electoral equilibrium is parameterized by the proportions of informed and uninformed voters.

The median voter theorem (Black 1958) is the basic theory of candidate policy positions in voting institutions. That theorem is based on the assumption that voters are informed about the positions of candidates, and when all voters are informed, campaign

expenditures, if they were to be made, would not affect voting or hence the equilibrium positions of the candidates. McKelvey and Ordeshook (1985) present an alternative theory in which some voters are uninformed and learn how to vote based on information provided in polls. In equilibrium, all voters vote as if they were informed, and candidates choose positions at the median of the voters' ideal points. When voters are not perfectly informed, however, campaign expenditures, as well as polls, are a principal means of influencing voting. As will be indicated, the presence of uninformed voters can cause candidates to separate their policy positions so as to cater to interest groups and thus attract campaign contributions that can be expended to increase the candidate's share of the uninformed vote. Polarization thus can result from the presence of uninformed voters and interest group campaign contributions.

The theory presented here provides a set of testable propositions pertaining to the relationships among a set of endogenous variables (candidate policy positions, campaign contributions by interest groups, and electoral outcomes) and a set of exogenous variables (the proportions of informed and uninformed voters, the amount of public financing, and incumbency advantages). The relationships depend importantly on the nature of the policy dimensions on which the candidates compete. Two types of policies will be considered. The first is a particularistic policy, which provides benefits to particular interest groups and can be denied to noncontributors. To determine whether the presence of uninformed voters affects electoral competition more generally, the second type of policy considered is collective and affects all interest groups regardless of whether they contribute. I shall distinguish in more detail between particularistic and collective policies and their modeling, then introduce the model and address symmetric electoral competition in particularistic policies. I then consider public financing of elections and extend the model to an election in which one of the candidates has an incumbency advantage. Finally, I consider collective policy positions and discuss the results.

POLICY DIMENSIONS AND CAMPAIGN CONTRIBUTIONS

Candidates compete for votes and campaign contributions on a number of dimensions, four of which may be identified as constituent services, particularistic policies, collective policies, and majoritarian policies. Majoritarian policies are those in which interest groups and their campaign contributions have no significant effect on the election outcome. They are not considered here. Constituent service activities include services ranging from those associated with pork-barrel projects to obtaining immigration clearance for relatives of constituents. These constituent services are generally such that either candidate is willing to provide them. Either candidate, for example, is likely to be willing to provide assistance, perhaps in response to a campaign contribution, in obtaining a defense contract for a firm located in the district. These constituency service activities are not considered here, since both candidates are willing to provide them. Instead, the focus is on particularistic and collective policy dimensions, which have the property that a candidate is willing to support a policy that benefits one set of interest groups but is unwilling to support a policy that benefits another set of interest groups.

Particularistic and collective policy dimensions are distinguished here by (1) whether the benefits associated with policies can be denied to interest groups that do not contribute and (2) whether those policies impose costs on other interest groups sufficient to cause them to make countervailing campaign contributions. Particularistic policies are those that provide benefits to some interest groups and impose insufficient costs on other interest groups to induce them to make campaign contributions. Particularistic policies may impose costs on unorganized interests, such as consumers or taxpayers, but those costs are assumed to be more concentrated than the costs of taking political action (e.g., by campaign contributions), so the unorganized interests do not make countervailing contributions. Consumers and taxpayers who are informed, of course, vote based on those policies. Collective policies are those which provide significant benefits to some interest groups and impose significant costs on others. Providing benefits to one set of interest groups can thus induce interest groups on the other side to contribute to the other candidate. It is the consequences for organized interest groupsand the campaign contributions those consequences generate—that distinguish particularistic policies from collective policies.

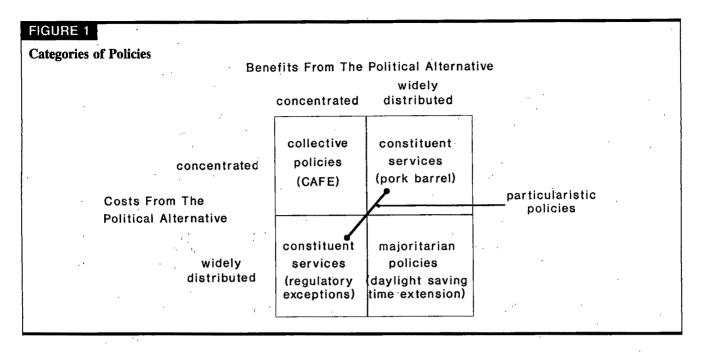
In the model, interest groups on the left of the policy dimension are assumed to be aligned with the candidate of the party on the left and those on the

right with the candidate of the party on the right. In the case of particularistic policies, if the candidate on the left wins the election, the interest groups on the left receive benefits corresponding to that candidate's policy, and the interest groups on the right incur only very small costs. Examples of particularistic policies are special provisions in bills, intervention with the bureaucracy on behalf of the contributor, regulatory exceptions (e.g., an environmental variance), and access to the candidate so that the contributor can present its concerns. In the case of trade policy, a particularistic policy might be a voluntary export restraint that applies only to a specific market or industry and imposes widely distributed costs on other interests, such as consumers. The policy dimension may then be thought of as catering to interest groups, where the candidate on the left caters to interest groups on the left and does not cater to interest groups on the right. For example, a particularistic policy of one candidate of providing a tax break or regulatory exception for small business may not generate contributions to the other candidate by other interest groups. A particularistic policy of providing trade adjustment assistance to labor may not generate contributions by small business to the other candidate. When candidates are willing to cater to any of the interest groups, that is the case of constituency services, which is not considered here.^

In the case of a collective policy, if the candidate on the left wins the election, the interest groups on the left receive the benefits and the interest groups on the right incur costs. Such policies are collective because they affect all interest groups in a significant, albeit differentiated, manner. Examples include labor legislation, trade policy that affects both imports and exports, and broad tax policy. The distinction between particularistic policies and collective policies is thus not in terms of the institutional context in which they are provided but, instead, depends on whether a policy that provides benefits to one interest group generates opposition by other interest groups.

The differences among these types of policies are illustrated in Figure 1 in terms of the corresponding distribution of the costs and benefits.³ A policy dimension that generates both concentrated costs for some interest groups and concentrated benefits for other interest groups is referred to as collective, and each set of interest groups has an incentive to make a contribution to the candidate on its side. An example is corporate average fuel economy standards, which pit automobile manufacturers and the United Auto Workers against environmental interest groups.

In contrast, the lower-right cell of Figure 1 corresponds to alternatives that have widely distributed benefits and costs. Wilson (1980) refers to such policies as majoritarian, and in the context of the model considered here, they correspond to the absence of interest group activity and hence of campaign contributions. An example is the 1986 extension of daylight saving time, which had broad and widely distributed costs and benefits. If the majoritarian policy alterna-



tive is in a single dimension, the median voter theorem is applicable.

If a policy generates concentrated benefits (costs) and widely distributed costs (benefits), taking action to obtain those benefits (reduce the costs) will generate little opposition. When either candidate is willing to provide those benefits, the policy can be thought of as constituent services. Delivering pork, assisting with immigration problems, and seeking exceptions to regulations are in this category.

Particularistic policies can be thought of as a pattern of providing such services to particular sets of interest groups. The diagonal line in Figure 1 is intended to illustrate a dimension of electoral competition in which (1) concentrated benefits are provided by one candidate to one set of interest groups and widely distributed costs are borne by others (e.g., consumers and taxpayers) and (2) concentrated benefits are provided by the other candidate to another set of groups, with that provision also generating widely distributed consequences for consumers and taxpayers. These policies may be thought of as catering to interest groups on the left of the policy dimension by the candidate on the left and to interest groups on the right by the candidate on the right. This catering is assumed to be observable to informed voters who have preferences over which interest group the candidates cater to. In this case, the candidate on the right can compete for the informed vote by moving to the left, but the interest groups on the right will then contribute less because that candidate is not catering as much to their interests.

Particularistic and collective policies are modeled differently in accord with these distinctions. For particularistic policies, the campaign contributions of an interest group are a function only of the policy of the candidate who caters to that interest group, since the policy does not have a sufficient impact on other interest groups to cause them to contribute to the other candidate. For collective policies, campaign contributions of interest groups depend on the policies of both candidates, since concentrated benefits and costs are imposed by whichever candidate wins.

In the literature, models assume a correspondence between particularistic and collective policies and whether the benefits can be denied by a candidate. In the model considered here, particularistic policies can be denied to an interest group by a candidate, whereas a collective policy affects all interest groups regardless of whether a contribution is made. A contribution can be denied in either of two conditions: (1) if no contribution is made or (2) if contributions are made to both candidates. In models of campaign contributions and electoral competition, this distinction is often reflected in the formulation of the model. In the case of particularistic policies, the interest groups are typically assumed to take the probability of a candidate winning as given, rather than as a function of their own contributions. A rational expectations condition is then used to close the model. In the case of collective policies, interest groups are typically assumed to act as if their contributions affect the probability of winning.

In the literature, Hinich and Munger (1989), Morton and Cameron (1991), Snyder (1990a and b) and I (Baron 1989a and b) consider either constituent services or particularistic policies. Denzau and Munger (1986) consider a nonelectoral model in which particularistic policy services in the form of the allocation of a legislator's effort are provided to elicit contributions from interest groups. Austen–Smith (1987), Brock and Magee (1978), Cameron and Enelow (1989), and Edelman (1990) consider models of electoral competition in collective policies. Some models accommodate aspects of both particularistic and collective policies (Baron and Mo 1993; Londregan and Romer 1993; Magee, Brock, and Young 1989, 248–50).

THE MODEL

The model is formulated in a parsimonious manner to highlight the incentives underlying electoral competition as structured by the presence of informed and uninformed voters and the opportunity to generate campaign contributions. To investigate these interactions, the model focuses on candidate competition for votes, rather than on competition for interest groups. Each candidate is thus assumed to be aligned with a set of interest groups.7 Although the number of interest groups aligned with each candidate is not affected by the choice of positions, the campaign contributions that they receive depend on those positions. Those positions also affect the votes of informed voters. Uninformed voters, however, are influenced only by campaign expenditures. To expose the electoral incentives, campaign contributions by individuals are assumed to be zero. Since the qualitative properties of the electoral equilibrium when individuals make contributions are similar, only the incentives that those contributions create are discussed.

Electoral competition takes place on a number of policy dimensions, but to simplify the model, policies are assumed to be one-dimensional. As indicated, the interpretation of the policy dimension depends on the nature of the policy in question. In the case of particularistic benefits, the policy dimension can have two interpretations. The first corresponds to a general left-right spectrum in which policies on the left provide benefits to interest groups on the left without imposing substantial costs on interest groups on the right.8 Policies on the right have an analogous interpretation. This policy dimension is thus best thought of as a catering dimension, with policies on the left corresponding to catering to interest groups on the left and policies on the right corresponding to catering to interest groups on the right. The location of interest groups on this dimension is consistent with the results of Poole and Romer (1985), who find that interest groups can be arrayed on one dimension and are located at the extremes of that dimension relative to candidate positions.

The second interpretation is that candidates choose positions in different policy dimensions. One candidate chooses a position to cater to one set of interest groups, and the other candidate chooses a position on another policy dimension to cater to a different set of interest groups. The two sets of interest groups could, for example, be those aligned with the parties of the candidates.

With either interpretation, candidate 1 is assumed to choose a position $p_1 \in [0, 1/2]$, and candidate 2 is assumed to choose a position $p_2 \in [1/2, 1]$. The policy position of a candidate is assumed to be meaningful in the sense that the winner will pursue that policy in the legislature. The legislative process is not modeled, but electoral positions would be expected to be correlated with legislative efforts. 10

A continuum of voters is assumed, with the total number of voters normalized to one. Voters are assumed to be of one of two types. A proportion *k* of the voters is uninformed about the policy positions of the candidates, and a proportion 1-k of the voters is perfectly informed about those positions. Informed voters are assumed to understand the positions of the candidates in terms of whose interests the candidates are serving and the policy positions that they will work to implement. Informed voters have preferences over which interests are served by the candidates; hence their preferences depend on the policy positions of the two candidates. The electoral incentives resulting from the presence of informed voters are the same as those underlying the median voter theorem and lead toward convergence of the candidate positions to the median of the informed voters. The median informed voter can be thought of as preferring that the candidates cater to interest groups located at the center of the policy space. In the terminology of Cox (1990), the presence of informed voters creates centripetal incentives for the candidates.

Informed voters include those who are members of interest groups and those who are not. The preferences of those informed voters who are not members of interest groups are represented by a quadratic utility function with an ideal point x_j for voter j. Informed voters are uncertain about how the uninformed voters will vote and hence are uncertain about the election outcome. The beliefs of informed voter j about the likelihood that candidate 1 will win is denoted by w_{j1} . The expected utility $u_j(p_1, p_2)$ of an informed voter who is not a member of an interest group is thus

$$u_j(p_1, p_2) = -w_{j1}(p_1 - x_j)^2 - (1 - w_{j1})(p_2 - x_j)^2,$$

where p_i is implemented if candidate i wins. The informed voter will vote for the candidate whose position is closer to his or her ideal point, so those with ideal points to the left (right) of the midpoint $m = (p_1 + p_2)/2$ of the policy positions will vote for the candidate on the left.

Informed voters who are members of interest groups are assumed to have ideal points on the same side of the median of the policy space. A member j of interest group k receives benefits b_{jk} if that interest group is catered to by the winning candidate. The interest group may also assess its members an amount d_{jk} , and the members of the interest group may be thought of as those who have benefits that are at least as great as the assessment. The expected utility of an informed voter who is a member of interest group k is specified as

$$u_j(p_1, p_2) = -w_{j1}(p_1 - x_j)^2 - (1 - w_{j1})(p_2 - x_j)^2 + (w_{j1}b_{jk} - d_{jk}),$$

where the last term is the expected benefits from membership in the interest group. The voter then will vote for the candidate on the left if and only if

$$2(p_1 - p_2)[x_j - m] + b_{jk} \ge 0.$$
 (1)

To simplify the analysis, the ideal points of both the informed voters who are and are not in interest groups are taken to be uniformly distributed over the policy set P = [0, 1]. A proportion α of the informed voters are assumed to be members of interest groups, and their vote depends on the distribution of benefits, as indicated in equation 1. All members of interest group k with ideal points to the left of the midpoint m will vote for the candidate on the left when m is to the left of the median $\mu = 1/2$ of the ideal points of the informed voters. Rather than specifying how the benefits b_{ik} are distributed within an interest group, a proportion ρ_l of those voters with ideal points in $(m, \mu]$ will be assumed to vote for candidate 1. If m is to the right of the median, all interest group members with ideal points to the left of the median vote for candidate 1 and a ρ_r proportion of those with ideal points in (μ, m) are assumed to vote for candidate 1. The number of informed voters who vote for candidate 1 is thus

$$(1-\alpha)m + \alpha(m+\rho_l(\mu-m))$$
 if $m < \mu$

and

$$(1-\alpha)m + \alpha(\mu + \rho_r(m-\mu))$$
 if $m > \mu$.

To simplify the analysis further, half the voters in the intervals (m, μ) and (μ, m) will be assumed to vote for candidate 1, so $\rho_1 = \rho_r = 1/2$. Candidate 1 thus receives s_1 of the informed vote, where

$$s_1 = m + \frac{\alpha}{2} (\mu - m).$$
 (2)

Uninformed voters are ignorant of the positions of the candidates, and their votes are assumed to be influenced by the messages they receive during the election campaign. Those messages may, for example, provide nonpolicy information about the candidates. These messages are funded by campaign expenditures, which are assumed to persuade uninformed voters to be more likely to vote for the candidate making the expenditure. How the uninformed voters will actually vote is uncertain, so let $\tilde{\nu}$ be a random variable representing the proportion of the uninformed voters who vote for candidate 1. The probability W_1 that candidate 1 wins then is

$$W_1 = Pr\left(k\tilde{\nu} + (1-k)s_1 \ge \frac{1}{2} \mid C_1, C_2\right)$$
$$= G\left(\frac{1}{2k} - \frac{1-k}{2k} s_1 \mid C_1, C_2\right),$$

where C_1 and C_2 are the campaign expenditures of candidates 1 and 2, respectively, and G is the complement of the distribution function of $\tilde{\nu}$. The probability W_1 is assumed to be increasing in candidate 1's expenditures C_1 and decreasing in candidate 2's expenditures C_2 .

The specific functional form of G depends on the theory used to represent how uninformed voters

decide to vote. Such theories include those based on probabilistic voting (e.g., Austen-Smith 1987; Hinich 1977) and Luce's (1959) theory of choice under incomplete information. A micromodel of how uninformed voters decide to vote will not be presented. Instead, the probability of winning will be specified in a manner intended to expose the incentives in the electoral competition and allow a closed-form characterization of equilibria. The probability W_i of candidate i's winning is specified as

$$W_1 = k \frac{C_1}{C_1 + C_2} + (1 - k) \left(m + \frac{\alpha}{2} (\mu - m) \right)$$
 (3)

and

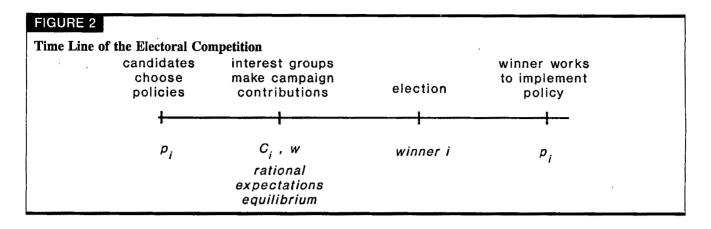
$$W_2 = k \frac{C_2}{C_1 + C_2} + (1 - k) \left(1 - \left(m + \frac{\alpha}{2} (\mu - m) \right) \right)^{12}.$$
(4)

The first term in equation 3 represents the effect on the probability of winning of the uninformed vote for candidate 1,¹³ and the second term is the effect of the informed vote from (2) for that candidate. The first terms in equations 3 and 4 are frequently used specifications in both the empirical and theoretical literature. Jacobson (1987) and Snyder (1990a and b) use this specification in empirical studies, and Snyder (1989) and I (Baron 1989a and b) use it in theoretical studies. It may be derived from axiomatic theories of (partially) uninformed voting (as presented by Luce) or from theories of probabilistic voting.¹⁴

ELECTORAL COMPETITION IN PARTICULARISTIC POLICY POSITIONS

In the case of particularistic policies, the policy p_1 of candidate 1 generates contributions from the interest groups aligned with candidate 1, and the policy p_2 of candidate 2 generates contributions from the interest groups aligned with candidate 2. The interest groups are assumed to be of the investment type and to make their contribution based on the return they can expect from their contribution. ¹⁵ The benefits—and thus the contributions-depend on the policy, and since the benefits can be denied if the interest group does not contribute, the interest groups and candidates can be thought of as bargaining over the contribution (Baron and Mo 1993). Since the benefits and the contributions depend on the policy, they represent a demand to pull the policy of a candidate toward that desired by the interest groups. Campaign contributions create what Cox (1990) calls centrifugal incentives, which are in opposition to the centripetal incentives created by the presence of informed voters.

The benefits to interest group k aligned with candidate i are denoted $b_k(p_i)$, and the total benefits provided by the catering of candidate i are $B_i(p_i) = \sum_k b_k(p_i)$. The benefits are provided only if the candidate to whom the contribution is made wins the election, so interest groups discount the benefits by



the probability of winning. Interest groups are assumed to have beliefs w about the probability that candidate 1 will win. Since there are many interest groups, their beliefs are assumed to be independent of the contribution they make. In equilibrium, those beliefs must be rational, so w must equal the actual probability of winning W_1 .

The assumption that the beliefs of interest groups are independent of their own contribution is consistent with the limits on campaign contributions and with some empirical results. Snyder (1990a and b) tests a model based on this assumption and finds that the data support the model. Chappell (1982) finds that the probability of winning does not respond significantly to an individual contribution. The assumption that interest groups take the probability of winning as given requires only a weak informational assumption, since interest groups only need to base their beliefs on publicly available information. For example, the beliefs w can be based on polls.

The bargaining between candidate 1 and interest groups can be thought of as resulting in contributions equaling some share of the aggregate expected benefits $wB_1(p_1)$. To facilitate a closed-form characterization of an equilibrium, the aggregate contributions C_1 are specified as a linear function:

$$C_1 = w(z_1 - p_1). (5)$$

Similarly, the contributions to candidate 2 are specified as

$$C_2 = (1 - w)(z_2 - (1 - p_2)),$$
 (6)

where z_i is a parameter that is assumed to satisfy $z_i \ge 1/2$, i = 1, 2, so that contributions are nonnegative. The specifications in equations 5 and 6 indicate that candidates raise more contributions, the more they cater (lower p_1 , higher p_2) to interest groups. This is consistent with Poole and Romer's study of interest group locations.

Figure 2 presents the time line of the electoral competition. Candidates move first by choosing positions, then interest groups make contributions, the election is held, and the policy of the winner is implemented. To focus on the relations among policy positions, voters, and campaign contributors (and not on the policy preferences of candidates) candi-

dates are assumed to maximize their probability of winning the election. An equilibrium (p_1^*, p_2^*, w^*) is thus policy positions and beliefs such that given the equilibrium beliefs of interest groups and the policy position of the other candidate, each candidate's policy maximizes his or her probability of winning. Given those policies, the equilibrium beliefs must agree with the actual probability of winning W_1 .

A candidate chooses a policy to maximize his or her probability of winning, taking the policy of (and thus the contributions to) the other candidate as given, as well as taking interest group beliefs w as given. Candidate 1 thus chooses p_1 according to 18

$$\max_{p_1} W_1 = k \frac{w(z_1 - p_1)}{w(z_1 - p_1) + C_2} + (1 - k) \left(\frac{p_1 + p_2}{2} + \frac{\alpha}{2} \left(\frac{1}{2} - \frac{p_1 + p_2}{2} \right) \right).$$

The necessary optimality conditions for interior optima $(p_1 \in (0, 1/2), p_2 \in (1/2, 1))$ for the two candidates are

$$-\frac{kwC_2}{(C_1+C_2)^2}+\frac{(1-k)(2-\alpha)}{4}=0$$
 (7)

and

$$\frac{k(1-w)C_1}{(C_1+C_2)^2} - \frac{(1-k)(2-\alpha)}{4} = 0,$$
 (8)

respectively. These conditions are also sufficient, since the probabilities of winning are strictly concave functions of the candidate's own policy and are strictly convex in the policy of the other candidate.

The conditions in equations 7 and 8 reflect the centripetal and centrifugal incentives. The first term in equation 7 is the centrifugal incentive to move away from the median so as to increase campaign contributions that can be expended to obtain the votes of uninformed voters. The second term in equation 7 is the incentive to move toward the median to obtain a greater share of the informed vote.

These two conditions imply that

$$wC_2 = (1 - w)C_1. (9)$$

Using equation 9 in equations 7 and 8 yields, for k < 1,

$$p_1 = z_1 - \frac{4kw(1-w)}{(1-k)(2-\alpha)}$$
 (10)

and

$$p_2 = 1 - z_2 + \frac{4kw(1-w)}{(1-k)(2-\alpha)}.$$
 (11)

To close the model, substitute equations 10 and 11 into equation 3 and use the rational expectations condition $W_1 = w$ to obtain for an interior equilibrium

$$w = kw + (1-k)\left(\frac{1+z_1-z_2}{2} + \left(\frac{\alpha}{2}\right)\frac{z_2-z_1}{2}\right).$$

This implies that the equilibrium probability of winning is

$$w^* = \frac{1}{2} + \frac{z_1 - z_2}{4} (2 - \alpha), \tag{12}$$

for those (z_1,z_2) such that $w^* \in [0,1]$. If $z_1=z_2=z$ so that the interest groups aligned with each candidate have symmetric demands for particularistic policies, the probability of winning is $w^*=1/2$. If $z_1>z_2$, candidate 1 is aligned with the higher-demand interest groups and thus has an interest group advantage. In equilibrium, that candidate is the more likely to win. The probability of winning is independent of the proportion of uninformed voters regardless of the interest group advantage.

The equilibrium policies for an interior equilibrium characterized by equations 10 and 11 are given by

$$p_1^* = z_1 - \frac{k}{(1-k)(2-\alpha)} \left(1 - (z_1 - z_2)^2 \left(\frac{2-\alpha}{2} \right)^2 \right)$$
 (13)

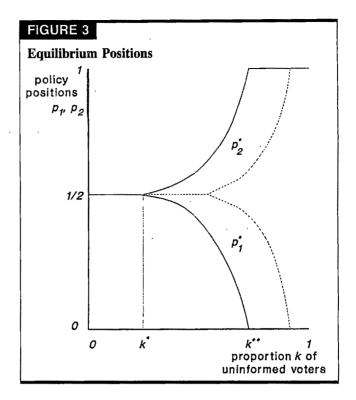
and

$$p_2^* = 1 - z_2 + \frac{k}{(1 - k)(2 - \alpha)} \cdot \left(1 - (z_1 - z_2)^2 \left(\frac{2 - \alpha}{2}\right)^2\right). \quad (14)$$

At an interior equilibrium, the policies satisfy

$$p_1^* + p_2^* = 1 + z_1 - z_2. {15}$$

If $z_1 = z_2$, equation 15 implies that the policies are symmetric about the median. If $z_1 > z_2$, candidate 1 is aligned with higher-demand interest groups and chooses a policy closer to the median than does candidate 2. The candidate with the higher-demand interest groups is more likely to win and can afford to sacrifice some contributions by moving closer to the median to attract more informed voters. As will be indicated, the equilibrium campaign contributions may also increase even though the candidate caters less to interest groups.



The equilibrium policies thus reflect the two incentives. The centripetal incentives underlying the median voter theorem induce candidates to choose policies closer to the median, so as to increase their share of the informed vote. The presence of uninformed voters, however, creates a centrifugal incentive to raise campaign contributions, and those contributions are greater the closer the policy is to that preferred by the interest groups. The equilibrium positions balance the response to these two incentives, taking into account the position of the other candidate.

These incentives depend on the proportion of informed voters. Figure 3 illustrates the equilibrium positions for the symmetric case in which $z_1 = z_2 = z$. The higher the fraction k of uninformed voters the stronger is the incentive to obtain campaign contributions, and the candidates respond by choosing policies (weakly) closer to the policies preferred by the interest groups. Consequently, the higher the proportion of uninformed voters, the more the candidates cater to interest groups and the more divergent are their policies.

If the proportion of uninformed voters is sufficiently low, that is

$$k \le k^* \equiv 1 - \frac{2}{2 + (2 - \alpha)(2z - 1)}$$

the incentives that underlie the median voter theorem are sufficiently strong that both candidates choose positions at the median. It is not necessary that k=0 for these incentives to dominate. As the proportion k of uninformed voters increases for

$$k \in \left(k^*, k^{**} \equiv \frac{(2-\alpha)z}{1+(2-\alpha)z}\right),$$

the candidates' equilibrium policies move toward those preferred by the interest groups, reflecting the higher electoral productivity of contributions. For $k \ge k^{**}$, the incentive to raise contributions dominates, and the candidates are fully responsive to the preferences of the interest groups.

The equilibrium policies also depend on the interest groups' valuations, z_1 and z_2 . For the symmetric case, an increase in z shifts the policies in Figure 3 to the right, indicating that candidates cater less to interest groups, the more valuable their policies are to those groups. That is, the more valuable the catering is to the interest groups, the easier it is to raise campaign contributions, and the candidate can afford to move toward the median. Higher-demand interest groups thus strengthen the centripetal incentive to move toward the median to attract a higher share of the informed vote. Hence, the higher the demand of interest groups for particularistic policies the less responsive, and more independent, are the candidates to interest group preferences.

The higher the proportion α of informed voters who are members of interest groups, the (weakly) farther from the median the equilibrium positions are. A higher proportion of interest group members decreases the centripetal incentives because it is easier to obtain their votes; hence, candidates compete less hard for the informed vote and cater more to the interest groups, so as to generate greater contributions

The contributions generated in an interior equilibrium $(k \in (k^*, k^{**}))$ are

$$C_1^* = \frac{k}{16(1-k)(2-\alpha)} (2 + (z_1 - z_2)(2-\alpha))^2 \cdot (2 - (z_1 - z_2)(2-\alpha))$$
 (16)

$$C_2^* = \frac{k}{16(1-k)(2-\alpha)} (2 + (z_1 - z_2)(2-\alpha))$$

$$(2-(z_1-z_2)(2-\alpha))^2$$
. (17)

The contributions in equations 16 and 17 are strictly increasing functions of k. Campaign contributions are greater, the higher the proportion of uninformed voters, because campaign expenditures are more productive. Candidates thus have stronger incentives to compete for the uninformed vote by catering more to the interest groups. For the symmetric case, the contributions are independent of the valuation parameter z. In the asymmetric case, the contributions to the candidate with the higher-demand interest groups are increasing in z_i for

$$z_i < z_j + \frac{4}{3(2-\alpha)}, j \neq i.^{21}$$

In an interior equilibrium, the contributions satisfy equation 9, which implies that the ratio of the contributions equals the odds ratio for the election, that is,

$$\frac{C_1^*}{C_2^*} = \frac{w^*}{1 - w^*}.$$
 (18)

Consequently, even though the candidates compete for the informed vote, the odds of candidate 1 winning equal the ratio of the contributions. The prediction in equation 18 involves two endogenous variables and has been tested by Snyder (1990a and b). His results support the prediction. Proposition 1 summarizes these results.

PROPOSITION 1. An interior equilibrium is characterized by equations 12–14: (a) The equilibrium probability of winning is (weakly) greater for the candidate with the interest group advantage, that is, the one aligned with interest groups that have the higher valuations of the particularistic policies.

- (b) As the proportion of uninformed voters increases, the equilibrium policies diverge and move (weakly) away from the median.
- (c) As the symmetric valuation $z_1 = z_2 = z$ increases, the equilibrium policies move (weakly) toward the median, since the candidates find it easier to raise contributions and need less to cater to interest groups.
- (d) The higher is the proportion α of informed voters who are members of interest groups, the more the candidates cater to the interest groups by choosing positions (weakly) farther from the median.
- (e) In an interior equilibrium, campaign contributions are a strictly increasing function of the proportion of uninformed voters and for $z_1 = z_2$ are a strictly increasing function of the proportion α of informed voters who are members of interest groups.
- (f) The (interior) equilibrium probability of winning is independent of the proportion of uninformed voters.
- (g) In an interior equilibrium, the ratio of the contributions equals the odds ratio for the election.

PUBLIC FINANCING OF ELECTIONS

At the federal level, public financing is limited to the presidential election, although broader public financing is considered regularly in Congress. This section investigates the effect of that financing and shows that the probability of winning becomes (weakly) closer to one-half. Public financing of elections thus makes the election closer. This results because public financing mitigates the interest group advantage of a candidate.

If the government provides a lump sum a to each candidate, the interior equilibrium (p_1^+, p_2^+, w^+) satisfies

$$p_1^+ = z_1 + \frac{a}{w^+} - \frac{4k}{(1-k)(2-\alpha)} (w^+(1-w^+)), \tag{19}$$

$$p_2^+ = 1 - z_2 - \frac{a}{1 - w^+} + \frac{4k}{(1 - k)(2 - \alpha)} (w^+(1 - w^+)), \tag{20}$$

and

$$w^{+} = \frac{1}{2} + \frac{2 - \alpha}{4} (z_1 - z_2) + \frac{a(1 - 2w^{+})}{w^{+}(1 - w^{+})}.$$
 (21)

Note that $z_1 > z_2$ implies that $w^+ > 1/2$. The following results then obtain.

PROPOSITION 2. a) In an interior equilibrium, higher levels of public financing result in particularistic policies closer to the median, so that candidates cater less to interest groups and compete harder for the informed vote (in this sense, public financing offsets voter ignorance); b) the (interior) equilibrium probability of winning is independent of the proportion k of uninformed voters; and c) public financing of elections has an "underdog property," since an increase in public financing increases the probability of winning of the candidate with the lower probability of winning—that is,

$$\frac{dw^+}{db}$$
 < (=)(>) 0 if w^+ > (=)(<) $\frac{1}{2}$.

The probability w^+ of winning thus is weakly closer to one-half with public financing than the probability w^* without that financing; that is,

$$z_1 > z_2 \rightarrow w^* > w^+ > \frac{1}{2}$$

$$z_1 < z_2 \rightarrow \frac{1}{2} > w^+ > w^*$$

$$z_1 = z_2 \rightarrow w^* = w^+ = \frac{1}{2}$$
.

This underdog property holds for both interior equilibria and equilibria with both policies at the extremes or at the median. Public financing thus results in closer elections by mitigating, but not overturning, the interest group advantage (e.g., $z_1 > z_2$) of a candidate.

Proof. The proofs of Proposition 2a and b are immediate from equations 19–21. As for 2c, subtracting the probability w^* in equation 12 from equation 21 yields

$$w^+ - w^* = \frac{a(1 - 2w^+)}{2w^+(1 - w^+)}.$$

Thus, the sign of w^+-w^* is the same as the sign of $(1-2w^+)$; and the result follows. Q.E.D.

Public financing of elections thus results in closer elections and in less catering to interest groups. In terms of electoral incentives, public financing weakens the centrifugal incentive to provide particularistic benefits to interest groups and strengthens the centrifugal incentive to compete for the informed vote.

These results support the conventional wisdom that public financing of elections allows candidates to be more independent of interest groups.

Campaign contributions by interest groups are decreasing in a, but total contributions $C_1^+ + C_2^+ + 2a$ are increasing in a, since at an interior equilibrium

$$\frac{d(C_1^+ + C_2^+ + 2a)}{da} = \frac{4k}{(1-k)(2-\alpha)} (1-2w^+) \frac{dw^+}{da},$$

and the sign of dw^+/da is the same as the sign of $(1 - 2w^+)$. These results are summarized in a formal proposition.

PROPOSITION 3. Public financing of elections results in higher total campaign expenditures, and the expenditures are increasing in the proportion k of uninformed voters.

Individuals, rather than interest groups or government, make the bulk of the campaign contributions. Individual contributions affect the incentive to raise contributions from interest groups and hence affect the policy positions of the candidates and the total contributions generated in the election. In a model in which each informed voter not a member of an interest group is assumed to make a contribution to the candidate with the closer policy position, the equilibrium policy positions are weakly closer to the median, as with public financing (Baron 1992). For symmetric equilibria, policy positions are closer to the median with individual contributions than with public financing, because candidates have an additional centripetal incentive to compete not only for the votes of the informed voters but also for their campaign contributions. Indeed, in contrast to public financing, total campaign contributions are decreasing in the amount of individual contributions, since interest groups decrease their contributions at a faster rate than individual contributors increase theirs. Individual contributions thus decrease the dependence of candidate on interest groups more effectively than does public financing. Individual contributions, however, suffer from the free-rider problem.

INCUMBENCY ADVANTAGES

This model is electorally symmetric in the sense that neither candidate has an a priori advantage in the electorate. The model thus may be thought of as corresponding to an open-seat election. If one of the candidates is an incumbent, however, symmetry is not a good assumption. Incumbents have a variety of advantages, two of which are considered here.

One incumbency advantage arises from how much interest groups value the policy efforts of the candidates. A candidate with seniority may have more influence in the legislature and thus be more likely to be successful in legislative matters. In that case, the valuations of their policy efforts by interest groups would satisfy $z_1 > z_2$. As indicated, the policy p_1^* in equation 13 is strictly increasing in z_1 , as is the equilibrium probability w^* in equation 12. The incum-

bent then has a higher probability of winning because he or she can afford to be more independent of interest groups, since those groups value the policy more highly. The incumbent is thus more responsive to the informed voters and chooses a policy closer to the median than does the challenger. Challengers cater more to interest groups than do incumbents, and incumbents are more independent of interest groups. If the incumbency advantage increases with seniority, then, over time, an incumbent caters less to interest groups.

Other sources of incumbency advantage are party identification and name recognition, which might, for example, come from past campaigns or the use of franking privileges. In the context of the model, these advantages do not affect the informed voters, who continue to vote according to the positions of the candidates, but do affect the uninformed voters. The simplest representation of this advantage is to specify the probability of candidate 1 winning as²²

$$W_1 = k \frac{\beta w(z_1 - p_1)}{\beta w(z_1 - p_1) + C_2} + (1 - k) \left(\frac{p_1 + p_2}{2} + \frac{\alpha}{2} \left(\frac{1}{2} - \frac{p_1 + p_2}{2} \right) \right), \quad (22)$$

where candidate 1 is assumed to be the incumbent and $\beta > 1$ parameterizes the incumbency advantage.

For an interior equilibrium, the following results obtain:

$$p_{1} = z_{1} - \frac{4kw(1-w)\beta}{(1-k)(2-\alpha)(w\beta + (1-w))^{2}}$$

$$p_{2} = 1 - z_{2} + \frac{4kw(1-w)\beta}{(1-k)(2-\alpha)(w\beta + (1-w))^{2}}$$

$$p_{1} + p_{2} = 1 + z_{1} - z_{2}$$

$$C_{1} = \frac{4kw^{2}(1-w)\beta}{(1-k)(2-\alpha)(w\beta + (1-w))^{2}}$$

$$C_{2} = \frac{4kw(1-w)^{2}\beta}{(1-k)(2-\alpha)(w\beta + (1-w))^{2}}$$
(24)

$$W_{1} = k \frac{w\beta}{w\beta + (1 - w)} + (1 - k) \left(\frac{\alpha}{4} + (2 - \alpha) \left(\frac{1 + z_{1} - z_{2}}{2}\right)\right).$$
 (25)

The rational expectations condition $W_1 = w^*$ yields

$$w^* = \frac{1}{2(\beta - 1)} (1 - k\beta - (\beta - 1)\eta + \sqrt{(1 - k\beta - (\beta - 1)\eta)^2 + 4(\beta - 1)\eta}),$$

where

$$\eta \equiv \frac{1-k}{4} \left(\alpha + (2-\alpha)(1+C-z_2)\right).$$

Since in equation 25,

$$\frac{w\beta}{w\beta + (1-w)} > w,$$

the equilibrium probability of winning satisfies $w^* > 1/2$ when $\beta > 1$ and $z_1 \ge z_2$. In equilibrium, the incumbent is thus the more likely to win.

The equilibrium w^* is a strictly increasing, strictly concave function of β , so a greater incumbency advantage results in a higher probability of winning. The probability of winning is strictly increasing in z_1 and strictly decreasing in z_2 , so a greater interest group advantage results in a higher probability of winning. If there is no interest group advantage, the probability of winning is constant in the common valuation z. The equilibrium probability w^* is a decreasing function of the proportion α of informed voters who are members of interest groups as long as the challenger does not have a substantial interest group advantage; that is, $1/2 < z_2 - z_1$. In contrast to the case with no incumbency advantage, the equilibrium probability of winning is a strictly decreasing function of the proportion of uninformed voters. This results because the challenger can compete more effectively the more productive his or her campaign expenditures are.

To investigate the effect of the incumbency advantage β on candidate positions, consider the case in which $z_1 = z_2 = z$. The policy positions then are symmetric, and as a function of β , those positions respond as

$$\frac{dp_1^*}{d\beta} = \left(\frac{4k}{(1-k)(2-\alpha)}\right)$$

$$\left(\frac{w^*\beta - (1-w^*)}{(w^*\beta + 1 - w^*)^3}\right) \left[\beta \frac{dw^*}{d\beta} + w^*(1-w^*)\right]$$

and

$$\frac{dp_2^*}{d\beta} = -\frac{dp_1^*}{d\beta}.$$

An incumbency advantage implies that $w^* > 1/2$; and since $\beta > 1$, the term $(w^*\beta - (1 - w^*))$ is positive, and so is $dp_1^*/d\beta$. A greater incumbency advantage increases the incumbent's share of the uninformed vote, and the incumbent moves toward the median to capture more of the informed vote. The challenger also moves toward the median because contributions are less productive as a result of the advantage of the incumbent.

The ratio of the equilibrium campaign contributions in equations 23 and 24 continues to equal the odds ratio for the election as in equation 18. The contributions C_1^* and C_2^* are strictly increasing and convex in the proportion k of uninformed voters, as in the case in which there is no incumbency advan-

tage. Total contributions, however, are strictly decreasing in β . Campaign contributions are thus greater in closer elections (β close to 1) than in elections in which a candidate has a substantial incumbency advantage.

The following proposition summarizes these results.

PROPOSITION 4. The probability of the incumbent winning is a strictly increasing function of his or her incumbency advantage and interest group advantage and a strictly decreasing function of the proportion of uninformed voters. The particularistic policy of the incumbent is closer to the median, the greater the incumbency advantage, and the challenger also moves closer to the median. Campaign contributions are decreasing in the incumbency advantage and are increasing in the proportion of uninformed voters. Total contributions are higher, the closer the election.

COLLECTIVE POLICIES

Collective policies differ from particularistic policies because they cannot be denied to an interest group that does not contribute to a candidate. In addition, collective policies affect all the interest groups, so the contributions by an interest group depend on the policies of both candidates. The policy in this case can be thought of as the scale of a public good provided. For the case of collective policies and informed voters, the median voter theorem implies that candidates choose policy positions at the median. I shall investigate whether this result is robust with respect to the presence of uninformed voters.

Since the collective policy of the winner will be implemented regardless of whether an interest group makes a contribution, the fundamental issue is why an interest group would contribute. Indeed, if an interest group takes the probability of winning as given, there is no reason to contribute. For this reason, models involving collective policies typically make the assumption that the interest group can affect the probability of winning. I shall thus consider the case in which each interest group explicitly takes into account how its contribution affects, at the margin, the probability of winning. Instead of the candidate and the interest groups bargaining over the contribution to be made, each interest group chooses its contribution, taking as given the policies of the candidates. For interest groups to be able to do so, they must know the functional relation between their contribution and the probability of winning. This requires that they have information about the aggregate contributions of other interest groups to the candidates, which is a strong informational assumption.²³ Even with this assumption, for the specifications used earlier, both candidates choose policies at the median and are unable to generate any contri-

For interest group j, the expected net benefits D_i

from contributions c_{ij} to candidate i, i = 1, 2, are specified as

$$D_{J} = W_{1}\eta(z_{1} - p_{1}) + (1 - W_{1})\eta(z_{1} - p_{2}) - c_{1j} - c_{2j},$$
(26)

where η , $\eta > 0$, is a parameter and W_1 is given in equation 22.²⁴ The interest groups are assumed to take the positions of the candidates and the contributions of the other interest groups as given when they make their contributions. It is straightforward to show that if $p_2 > p_1$, an interest group aligned with the candidate on the left will contribute only to that candidate.²⁵ The optimal contribution satisfies the first-order condition

$$\eta(p_2 - p_1) \frac{\partial W_1}{\partial c_1} - 1 = 0,$$
 (27)

where

$$\frac{\partial W_1}{\partial c_1} = k \frac{\beta C_2}{(\beta C_1 + C_2)^2} \tag{28}$$

and $C_i = \sum_j c_{ij}$, i = 1, 2. Using equation 28 in equation 27 yields the implicit relation

$$\eta(p_2 - p_1)k \frac{\beta C_2}{(\beta C_1 + C_2)^2} = 1.$$
 (29)

In a similar manner, the optimal contributions to candidate 2 by interest groups aligned with that candidate imply

$$\eta(p_2 - p_1)k \frac{\beta C_1}{(\beta C_1 + C_2)^2} = 1.$$
 (30)

The expressions in equations 29 and 30 are the (implicit) aggregate response functions of the interest groups aligned with the two candidates. Note that the contributions depend on the difference between the policies of the candidates.

Substituting the response functions in equations 29 and 30 into candidate 1's probability of winning vields²⁶

$$W_1 = k \frac{\beta}{\beta + 1} + \frac{1 - k}{4} (\alpha + (2 - \alpha)(p_1 + p_2)).$$
 (31)

The first term in equation 31 corresponds to the uninformed voters and is independent of the policy positions. Recognizing this, the candidates compete only for the informed vote. Both candidates thus locate at the median. From equation 27, the contributions are then zero, since the interest groups are indifferent as to who wins. This result is summarized in a formal proposition.

Proposition 5. In electoral competition based on collective policies, if each interest group believes that its contribution affects the probability of winning, both candidates locate at the median, and contributions are zero. Competition for the informed vote thus dominates.

This result obtains because the contributions to each candidate depend only on the difference in their policy positions. Hence, a movement to the center by candidate 1 decreases both the candidate's and the opponent's contributions proportionately, and the ratio of his contributions to the total contributions remains constant. A candidate is thus unable to influence his or her share of the uninformed vote, because the interest groups adjust their contributions to any change in policy. Being unable to affect their shares of the uninformed vote, the candidates compete for the informed vote, and the centripetal incentives lead them to the median. Contributors thus lose their power in elections in which the competition is on collective policies.

These results are more general than the linear specification of benefits in equation 26. If $v_i(p_j)$ denotes the value of the benefits to the interest group aligned with candidate i when policy p_j is pursued in the legislature, a sufficient condition for both candidates to locate at the median is that the term

$$\frac{v_2(p_2) - v_2(p_1)}{v_1(p_1) - v_1(p_2)}$$
 (32)

be constant in the policies. Whenever the candidates locate at the median, the contributions are zero for each candidate.

The results for particularistic and collective policies suggest that contributions are generated less by collective policies than by particularistic policies. If collective policies do generate contributions from interest groups, it calls into question one of two assumptions: (1) that the interest groups optimize, rather than bargain with candidates, or (2) either that the contributions do not depend on the difference between the collective policy positions as in the expression in equation 32 or the probability of winning have forms different from those considered here.

DISCUSSION

Most candidates devote considerable effort to raising campaign contributions and recognize that the policy positions that they take affect the amount of those contributions. Those policies also affect the voting of the informed portion of the electorate. Candidates thus must take into account the effect of their policy positions both directly, on the voting of informed voters, and indirectly, through the effect of campaign expenditures on uninformed voters. The two types of policy dimensions considered here are particularistic policies, which provide benefits to certain interest groups while imposing widely distributed costs on those who cannot be expected to take political action, and collective policies, which yield costs and benefits for all interest groups. These policies are distinguished by two characteristics. First, particularistic policies are provided only in exchange for contributions, whereas collective policies are provided by the winning candidate regardless of whether a contribution is made. Second, for particularistic policies, contributions to a candidate depend only on his or her position, whereas for collective policies, contributions depend on the positions of both candidates.

The model of electoral competition yields a number of testable hypotheses. Any test should, however, be specific as to the nature of the policies with which candidates compete. In the case of particularistic policies, the equilibrium policy positions of candidates depend on the proportion of uninformed voters. If that proportion is low, the dominant electoral incentives are provided by the informed vote. These are the same incentives as those underlying the median voter theorem; hence, both candidates choose positions at the median of the ideal points of the informed voters. This results because when there are relatively few uninformed voters to influence through campaign expenditures, the productivity of those expenditures is low, and candidates compete for the informed vote by moving toward the median, rather than catering to interest groups to raise campaign contributions. If the proportion of uninformed voters is high, however, candidates cater to interest groups and use the campaign contributions received to compete for the uninformed vote. The incentives to compete for the uninformed vote can lead candidates to separate their positions. For intermediate proportions of the uninformed vote, candidate positions are strictly farther from the median, the higher the proportion of the uninformed vote. Polarization can thus result from the conjunction of uninformed voters and interest groups' contributions.

The equilibrium policies and the election outcome also depend on the value of the particularistic policies to the interest groups aligned with each candidate. If one candidate is aligned with higher-demand interest groups, that candidate has an advantage in raising contributions and can afford to choose a policy position closer to the median. The candidate aligned with the higher-demand interest groups thus caters less to interest groups, competes harder for the informed vote by moving closer to the median, and in equilibrium has the higher probability of winning. The candidate aligned with higher-demand interest groups is thus more independent of interest groups in the sense that his or her policy is more centrist. If informed voters cast contributions, in addition to votes, the incentives underlying the median voter theorem are strengthened.

Public financing of elections increases the probability of winning of the candidate who is the underdog, that is, who has the lower probability of winning. The equilibrium locations of the candidates are closer to the median as the amount of public financing increases. Public financing thus results in closer elections and less catering to interest groups, but total campaign expenditures are higher than when there is no public financing.

Most congressional elections involve an incumbent, and incumbents may have an advantage with respect to name recognition, reputation, or party identification. Such advantages presumably pertain

to uninformed voters, so the incumbency advantage is modeled in terms of the effectiveness of the incumbent's campaign expenditures in attracting the uninformed vote. In equilibrium, the probability of the incumbent winning is a strictly increasing function of the incumbency advantage, and the particularistic policy position of the incumbent is closer to the median, the greater the incumbency advantage. Because of that advantage, the incumbent has a lower demand for campaign contributions and competes harder for the informed vote by choosing a position closer to the median. The incumbent is thus more independent of interest groups than is the challenger. Total campaign expenditures are decreasing in the incumbency advantage, so expenditures are higher, the closer the election.

Collective policy positions affect all interest groups regardless of whether they contribute. With collective policies, there is no incentive to contribute unless the contributions are believed to affect the probability of winning. If the probability of winning depends on the contribution of an individual interest group and if interest groups maximize the net benefits from their contributions, then in a symmetric election both candidates locate at the median and contributions are zero. This result depends on the relation between the benefits to interest groups and candidate positions, but it suggests that campaign contributions may be raised more by particularistic policies than by collective policies.

The empirical literature on campaign contributions and policies presents a variety of conflicting results, and the model considered here provides an explanation of some of those disparate results. In particular, the model indicates the importance of distinguishing between particularistic and collective policies. For particularistic policies, the model yields the prediction from equation 9 that the probability of winning is equal to that candidate's fraction of the total campaign contributions. Snyder (1990a and b) tests this proposition for contributions by investor political action committees, and the data for open-seat elections in the House and for Senate elections support that prediction. The model also predicts that the same relation holds for elections with an incumbency advantage and with public financing.

The model also provides predictions about the relation between campaign contributions and policies. Those policies presumably are reflected ex post in the winner's legislative voting, so an endogenous relation between campaign contributions and legislative voting is predicted. That relation, however, depends importantly on the type of policy. Chappell (1982) studied policies for which the proportion of uninformed voters should be high and found weak evidence of a relation between contributions and congressional voting.²⁷ The model presented here predicts that the relation should depend on the proportion of uninformed voters, as well as whether the winning candidate had an interest group advantage or an incumbency advantage. A high percentage of winners are incumbents, and the model predicts

that with a strong incumbency advantage the position of the winner could be close to the median, which may provide an explanation for Chappell's findings.

Wright (1990) studied two collective policy issues and found no relation between contributions and congressional voting. The model predicts that candidate policies should be at the median and that no contributions should be raised from these policies. Wright's findings are consistent with the first prediction of the model but not with the second. It is, of course, possible that the contributions are made to obtain access for lobbying directed toward particularistic policies. In that case, the model predicts that contributions would be generated.

Wilhite and Theilmann (1987) studied congressional voting on labor issues and found a positive relation between labor political action committee contributions and congressional voting. If the labor issues are collective, the model indicates that a test should include business contributions as well. The relation between labor political action contributions and congressional voting may reflect the alignment of labor interest groups with a set of candidates on the same side of issues as labor.

The theory presented here warrants at least five types of extension. One is to consider a sequence of elections in which candidates can build seniority and incumbency advantage and incumbents can generate campaign contributions through current, as well as anticipated, actions. Second, a theory of electoral involving campaign contributions competition should be extended to include lobbying of current officeholders. Third, electoral competition on policy dimensions should include more than one category of policy and should be multidimensional. Fourth, the effect of campaign expenditures on the voting of the uninformed electorate should be represented not by a reduced form but rather by a micro theory of voter information and behavior. Fifth, to the extent that campaign contributions are informative, the proportion of informed voters should depend on the campaign expenditures of the two candidates.

Notes

This research has been supported by National Science Foundation Grants No. SES-8808211 and SES-9109707. Jonathon Bendor, Keith Krehbiel, Susanne Lohmann, Rebecca Morton, and Raymond Riezman provided very helpful comments.

- 1. Wright (1990) studied the relationship between campaign contributions, subsequent lobbying, and legislative voting and found that the effect of contributions is through lobbying, which suggests that access is facilitated by contributions. Wilhite and Theilmann (1987) find that contributions by labor political action committees affect congressional voting on labor issues.
- Equilibrium models of electoral competition based on the constituency service activities are from Baron 1989a and b.
- 3. This representation is developed from Lowi 1964 and Wilson 1980.

4. See Baron 1993, 125-30. Some interest groups eventually did form on this issue.

5. Denzau and Munger's (1986) model does not include an electoral challenger. They consider an electorate that is rationally ignorant and an electorate that is fully informed, but in contrast to the model here, their focus is on the allocation of a legislator's effort between interest groups and the electorate.

6. Londregan and Romer (1993) consider a model of electoral competition in which parties compete both on a collective policy dimension and on constituent services. The collective policy position is known, but voters receive only a noisy signal about the ability of the candidates to provide services. They find that the more voters value the services, the more separated are the candidates' policies on the collective dimension. In an election in which one candidate is an incumbent, the incumbency advantage is greater, the stronger voters' preferences for services are.

7. A coauthor and I study a model in which interest groups can switch between the candidates based on which candidate makes the more attractive offer of constituent services (Baron and Mo 1993).

8. If a policy imposes significant costs on interest groups on the other side, the policy is said to be collective.

Although the policy sets of the candidates may result from party identifications, parties are not actors in the model.

10. Banks (1990) provides a theory in which candidate positions are signals of future legislative actions. Chappell (1982) studied issues affecting only a single interest group and found only a weak relationship between contributions and subsequent legislative voting. The role of reelection interests in generating credible commitments is considered in Austen-Smith and Banks 1989 and Baron 1989b.

11. A voter does not choose an interest group, but instead, interest groups are formed from among voters who have similar characteristics such as occupation.

12. That is, G has the form

$$G(\phi|\theta) = (\theta - \phi)k + \frac{1}{2},$$

where

$$\phi = \frac{1}{2k} - \frac{1-k}{k} s_1$$
 and $\theta = \frac{C_1}{C_1 + C_2}$.

13. If $C_1 = C_2 = 0$, this term is defined to be 1/2.

14. Another interpretation of the specification in equation 3 comes from models of probabilistic voting. If t (which depends on both positions and campaign expenditures) denotes the probability that a randomly selected voter will vote for candidate 1, then the probability T of candidate 1 winning is

$$T = \sum_{j=\frac{N+1}{2}}^{N} \frac{N!}{j!(N-j)!} t^{j} (1-t)^{N-j},$$

where N is the number of voters. For large N, maximizing the expected number of votes is equivalent to maximizing the probability of winning (Hinich 1977). Austen-Smith (1987) then assumes that the probability of winning can be approximated by the expected number of votes divided by N, which corresponds to W_1 .

15. See Snyder 1990a.

16. The contributions function C_1 in equation 5 is specified as strictly decreasing in p_1 , but since interest group preferences can be expected to be distributed across the policy space, there is no natural reason for this response function to be monotonic. If contributions reach a maximum at an interior point, \bar{p}_1 , of [0, 1/2], the candidate would never choose a position farther from the median than that position. Consequently, the results indicating that the candidates separate their positions can be interpreted as meaning that candidate 1 never chooses a position to the left of max $\{0, \bar{p}_1\}$. To simplify the notation, \bar{p}_1 will be assumed to be 0, and the corresponding \bar{p}_2 will be assumed to be 1.

17. The specifications in equations 5 and 6 are based on the perspective that an interest group takes the probability of winning as independent of its contribution and that the candidate and the interest group bargain over the amount of the contribution. An alternative formulation is that an interest group believes that its contribution affects the probability of winning and thus chooses its contribution optimally based on its effect on that probability. This formulation is considered in Baron 1992, and the resulting equilibrium is shown to be qualitatively the same as the equilibrium based on equations 5 and 6. For a symmetric election, the equilibrium policies and contributions are identical in the two formulations.

18. Note that the beliefs w are not a direct function of policies. Beliefs adjust to policies only in accord with the contributions those policies generate.

19. The valuations z_i may be related to α , but I shall discuss the partial effects holding constant the other parameters.

20. For the asymmetric case, the responses to changes in the valuation parameters, for an interior equilibrium, are

$$\frac{dp_1^*}{dz_1} = 1 + \frac{2k}{(1-k)(2-\alpha)} (z_1 - z_2)$$

$$dp_2^* = 2k$$

$$\frac{dp_2^*}{dz_2} = -1 + \frac{2k}{(1-k)(2-\alpha)}(z_1 - z_2)$$

$$\frac{dp_1^*}{dz_2} = \frac{dp_2^*}{dz_1} = -\frac{2k}{(1-k)(2-\alpha)}(z_1-z_2).$$

If $z_1 > z_2$, so that the interest groups aligned with candidate 1 have a higher valuation than the interest groups aligned with candidate 2, an increase in z_1 moves both policies toward the median. This results because candidate 1 finds it easier to raise campaign contributions and thus can choose a policy that attracts more of the informed voters. Candidate 2 responds to candidate 1's move by also moving closer to the median. If $z_2 > z_1$, an increase in z_2 causes both candidates to choose policies closer to the median. If $z_1 > z_2$, however, an increase in z_2 causes candidate 1 to move away from the median. Depending on the values of k and α , candidate 2 may choose a policy closer to or farther from the median as z_2 increases. For k high, candidate 2 may move away from the median.

21. For the asymmetric case, as a function of z_1 ,

$$\frac{dC_1^*}{dz_1} = \frac{k}{2(1-k)(2-\alpha)} (2 + (z_1 - z_2)(2-\alpha))$$

$$\cdot (2-3(2-\alpha)(z_1-z_2))$$

$$\frac{dC_2^*}{dz_1} = -\frac{k}{2(1-k)(2-\alpha)} (2-(z_1-z_2)(2-\alpha))$$

$$\cdot (2 + 3(2 - \alpha)(z_1 - z_2)).$$

22. Public financing is not included here.

23. Campaign contribution reporting requirements provide such information but only with a lag.

24. For particularistic policies, the contributions were represented as $w(z_1-p_1)$, and here, benefits are $w\eta(z_1-p_1)$. If $\eta=2$ and interest groups and candidates bargain to a 50–50 division of the benefits, then C_1 in equation 5 is half the benefits.

25. Austen-Smith (1987) considers electoral competition in collective policy positions and allows interest groups to contribute to both candidates. He shows that an interest group will never contribute to both. Empirical evidence indicates that most political action committees contribute to only one candidate.

26. Analogous to the specification just made, the term $\beta C_1/(\beta C_1 + C_2)$ is specified as $\beta/(\beta + 1)$ when $C_1 = C_2 = 0$. 27. The policies considered by Chappell (1982) include a

27. The policies considered by Chappell (1982) include a mixture of particularistic and collective policies. Those policies are mortgage disclosure requirements, milk price supports, truck weight limits, tax rebates for oil companies,

funding for the B-1 bomber, auto emissions controls, and maritime cargo preference.

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THE NATURE OF CONGRESSIONAL COMMITTEE JURISDICTIONS

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urisdictions are the defining characteristics of committee systems, and they are central in any discussions about the U.S. Congress; yet we know little about them. Where do committee property rights come from? Are they rigid? Are they flexible? I introduce a distinction between statutory jurisdictions (which are written in the House and Senate Rules) and common law jurisdictions (which emerge through bill referral precedents). Turf is gained through common law advances, not through formal rules changes (like the "reforms" passed by the House in 1946, 1974, and 1980). Jurisdictional change is ongoing and incremental. The analysis draws on an examination of hearings held by the House Commerce Committee from 1947 through 1990.

urisdictions ("turf," or "policy property rights") are the defining characteristics of committee systems, and they are central in any discussions about Congress. Unfortunately, we know little about them. That is, we know that members seek seats on committees based largely on turf (Shepsle 1978), and we know that who has jurisdiction over an issue can have a tremendous impact on what policies, if any, eventually emerge (Jones, Baumgartner, and Talbert 1993; Tiefer 1989). But how did we end up with the committee system we now have? Where do jurisdictions come from in the first place? How do they change? These are critical questions.

If jurisdictions rarely change, then legislatures are not good at adapting when new issues like child care, sex discrimination, and industrial competitiveness emerge. If turf is carved out so that lawmakers can institutionalize logrolls and make it easier to distribute benefits back home (Weingast and Marshall 1988), then many of us might want to undermine those property rights in the name of the public interest. However, if property rights are granted to committees as rewards for specializing in complicated policy areas (Krehbiel 1991), then there might be good reasons for reinforcing the ways jurisdictions are arranged.

Where do jurisdictions come from? Are they rigid? Are they flexible? These questions are rarely asked, because jurisdictions have been fundamentally misconceived by many political scientists. Jurisdictions are not rigid institutional facts that rarely change. Rather, they are turbulent battle grounds on which policy entrepreneurs seek to expand their influence. What Washington insiders call "the jurisdiction game" is widely played on Capitol Hill, and that game has much to teach us about the dynamics of legislative institutions.

I shall describe the nature of committee jurisdictions, with an emphasis on understanding the role of periodic jurisdictional reforms (like those in 1946 and 1974). As such, the foundation is laid for a theory of jurisdictional change discussed in more detail elsewhere (King 1992). Most of my examples will draw on the House of Representatives, though the basic nature of committee jurisdictions is the same in

the Senate and is similar in a majority of the state legislatures.

First, I introduce a critical distinction between common law jurisdictions, which emerge out of bill referral precedents, and statutory jurisdictions, which are written down in the House and Senate rules. I then examine the House Commerce Committee's jurisdiction for the emergence of common law issues. Finally, I show that jurisdictional "reforms" amount to little more than the codifications of common law jurisdictions. So if we want to understand how jurisdictions (and legislatures more generally) change, we should focus less on reforms and more on incremental day-to-day adaptations in unwritten (but often binding) rules.

THE SOURCES OF JURISDICTIONAL LEGITIMACY

Turf is hotly contested in Congress, and for good reason. One committee staff director described it this way: "Jurisdiction boils down to whether you'll have a seat at the table when important decisions are being made. If you're not at the table, you're a nobody."

Statutory Jurisdictions

There are two ways to get a seat at the table. One is to be on a committee that has turf hard-coded into the House or Senate rules, which I shall call "statutory jurisdiction," because turf is voted on by a majority of the House and Senate when adopting written rules. In practice, the rules of a preceding Congress are used by subsequent sessions with minor and infrequent changes.

Most committees have 10 to 15 specific issues listed under their jurisdiction in the rules. For example, child labor is under the control of the House Education and Labor Committee because it is included among the 14 issues granted the committee in House Rule X, clause 1. Political scientists almost exclusively studied statutory jurisdictions and concluded that jurisdictions rarely change merely because the writ-

ten rules rarely change (Maass 1983; Collie and Cooper 1989; Smith and Deering 1990). Statutory jurisdictions are easy to find, footnote, and quantify, but the written rules are just one source of jurisdictional legitimacy.

Common Law Jurisdictions

The second way to get a "seat at the table when important decisions are being made" is to be on a committee that has been granted common law jurisdiction. When jurisdictionally ambiguous bills are introduced, they still have to be referred to one committee (or sometimes several committees) within 24 hours. As will be explained, the House and Senate parliamentarians—unelected but powerful clerks refer bills and resolve jurisdictional ambiguities. These referrals establish binding precedents for all future bills on the same subjects, thereby resolving jurisdictional ambiguities. For example, in addition to child labor laws, House Education and Labor (as opposed to the Judiciary Committee) has long claimed a set of juvenile delinquency bills, but that property right is not actually written down in the rules (Congressional Record 1959, 1027).

"The law should be stable," wrote Justice Holmes, "but never stand still." So, too, for jurisdictions. The comparison between statutory and common law jurisdictions parallels the distinction lawyers and judges make between statutory and case law. Edward Levi writes that "the basic pattern of legal reasoning is reasoning by example" (1949, 1), and this also

applies to committee property rights.

With few exceptions (Davidson and Oleszek 1994; Jones, Baumgartner, and Talbert 1993; Oleszek 1989), political scientists have acted as if the written rules are the sole source of jurisdictions. Smith and Deering (1990) count the number of statutory issues in a committee's jurisdiction to estimate fragmentation. Sullivan (1984) does something similar when distinguishing between "general" and "functional" committees. Such measures completely miss issues that committees gain through the strategic introduction of bills and the establishment of referral precedents. And while the written rules in the House and Senate rarely change, some observers wrongly believe that jurisdictions also rarely change. "Stories of jurisdictional infighting are legion," write Collie and Cooper "and turf protection is so pronounced as to have frustrated all but the most minor changes in committee jurisdictions since $1945^{\prime\prime}$ (1989, 253).

I shall demonstrate that the received wisdom, reflected in the quote from Collie and Cooper, is simply not true. Stories of jurisdictional infighting are legion, and so are jurisdictional advances made through the strategic use of bill referral precedents. Consumer protection, national energy strategy, insurance regulation, and "fair" trade are just a few not-so-minor issues that have been claimed by committees since 1945. And for every committee that wins jurisdiction, there are many more that do not. Some committees now employ staffers called "border

cops," whose jobs involve protecting turf and looking out for new areas to conquer.

Precedent-setting referral decisions are made by the House and Senate parliamentarians routinely (in several senses of the word). They use a predictable and nonpartisan set of decision rules for handing out turf (King 1992), and players in the jurisdiction game have come to count on these decision rules when drafting legislation (Tiefer 1989; Zorack 1990). The resolution of jurisdictional ambiguities is also commonplace. From 1935 through 1963, an average of 18 new common law expansions were recorded every year in Cannon's Procedure, and that underestimates the true number, because many were only recorded in the Congressional Record (Cannon 1963).

Interview evidence consistently points to changing jurisdictions. When one of the House parliamentarians learned that some observers think of turf as what is written down in the rules, implying that jurisdictions are largely static, he replied

PARLIAMENTARIAN: I can see why, from a political scientist's standpoint, there would be some confusion. But we simply have to use past referral decisions to guide what we do. You just never know what new things are going to come up.

INTERVIEWER: Then through referral precedents, jurisdictions can change. Do you think they have changed much in your time here?

PARLIAMENTARIAN: Oh heavens yes, they're much different than when I came to Congress. Yes there are changes. There are always changes. Look at the battle over OCS [outercontinental shelf]. Look at the jurisdiction over national parks and the Interior Committee. Look at what constitutes a fee and what is a tax. There are too many like these to name. (Interview by author, 14 June 1991)

The strategic value of playing the jurisdiction game is well known to members of Congress. Because they have considerable say over their own agendas, committee and subcommittee chairs are more likely than junior members to lead jurisdictional forays. Former representative James Florio (D-NJ) was a subcommittee chair in the mid 1980s, and the following statement by him is typical of many I encountered during not-for-attribution interviews: "We're expanding our jurisdiction. We've got authority over the FTC [Federal Trade Commission], and that gets you to antitrust and regulation. We've begun to deal with some trade issues. There was a headline the other day, "Florio on Trade." The legislative credentials and the jurisdiction give you a forum on almost everything. From the forum you affect public opinion and from that you get clout" (quoted in Loomis 1988, 168).

Any current list of House committees gaining common law turf is topped by Energy and Commerce, chaired by John Dingell (D-MI), followed perhaps by Ways and Means under Dan Rostenkowski (D-IL); Natural Resources since 1991, when George Miller (D-CA) took over as chair; and Science, Space, and

Technology since 1990, when George Brown (D-CA) became chair. The list of jurisdictionally expansive committees may change from time to time, but the basic underlying strategies for gaining common law turf have been used since the institutionalization of the parliamentarian's office following the 1910 revolt against Speaker Cannon (King 1992).

Common law jurisdictions advance into the "grey areas" between committees when bills on jurisdictionally ambiguous issues are referred by the parliamentarians. Is there any value to political scientists in distinguishing between common law and statutory jurisdictions? Yes, for three reasons. First, it is a distinction that players in the jurisdiction game make implicitly. Some also distinguish between "literal" and "unwritten" jurisdictions. Others talk about "formal" and "informal" turf. But in any event, common law jurisdictions are just as binding as what is written in the rules. Second, if it can be shown that jurisdictions change through bill referral precedents, then we may gain insight into how legislatures change more generally. Most observers have focused on specific reform periods in which the written rules changed (Rieselbach 1986), but if institutional change is ongoing and reforms simply codify common law changes, then we should pay more attention to how institutions are changing day to day. Third, if institutional change happens through common law adaptations, understanding that process will help us better evaluate the competing claims of distributive politics theorists (Weingast and Marshall 1988) and organization and informational efficiency theorists (Krehbiel 1991; Polsby 1968).

THE PARLIAMENTARIANS

House and Senate parliamentarians are central to the jurisdiction game, but these institutional guardians have been long overlooked by scholars, perhaps because they guard their anonymity carefully. Journalists on Capitol Hill know the parliamentarians as important players who are reluctant to be interviewed and do not like to be photographed. In the House, the parliamentarians update the books of precedents, assist the Rules Committee on technical issues related to floor management, advise members of both parties on parliamentary procedure, and refer all bills to committees (no matter how jurisdictionally ambiguous those bills may be). As congressional employees, the parliamentarians are unelected either by voters or by members of Congress (Siff and Weil 1975).

According to the House rules, the Speaker is charged with referring bills to committees, but in practice the Speaker has rarely been involved in these decisions since the revolt against Speaker Cannon in 1910. It is well understood by members, their staffs, and Washington lobbyists that the parliamentarians are the central figures in bill referrals (Tiefer 1989).

There are five parliamentarians in the House, four of whom work under the head parliamentarian, who serves, ostensibly, "at the pleasure of the Speaker." But the parliamentarians are surprisingly independent of the Speaker (King 1992; Tiefer 1989; Zorack 1990), and they pride themselves on making nonpartisan decisions. It is also common for members of the minority party to praise the parliamentarians as nonpartisan, which one would not expect if they were simply doing the Speaker's handiwork. Trained as lawyers, the parliamentarians are expected to serve Congress for most of their professional lives (Lewis Deschler was head House parliamentarian from 1928 to 1974, serving under nine Speakers), and they think of themselves as institutional guardians.

When deciding where to send a jurisdictionally ambiguous bill (thereby granting common law turf to the committee that gets the bill), the parliamentarians use a decision rule called "the weight of the bill," which is similar to the legal construct "the weight of the evidence." This decision rule is self-consciously used so that the expertise of members in a "close-by" jurisdiction can be tapped. In this way, the parliamentarians help reinforce the kind of informationally efficient committee system that Krehbiel (1991) envisions. But like judges, the parliamentarians can only adjudicate jurisdictional disputes after a case is brought to them in a bill referral, and the entrepreneurial activity among members that drives the search for "hot" jurisdictionally ambiguous issues is best understood in a distributive framework (Weingast and Marshall 1988).

With respect to one jurisdictionally ambiguous issue—magnetically levitated trains—this dynamic has been tested empirically (King 1992). Institutional change involves both distributional incentives at the individual level (Weingast and Marshall 1988) and an institutional response, through the parliamentarians, that promotes an informationally efficient committee system (Krehbiel 1991).

Almost all jurisdictional change happens in "new" areas or in old areas that are being recast in the light of new events (Jones, Baumgartner and Talbert 1993). Most border wars are not fought along well trenched lines with one committee trying to take away another committee's turf. A few recent works have focused on attempted takeaways (LaRue and Rothenberg 1992; Shipan 1992, 1993; Krehbiel and Lavin 1993), but House and Senate rules are strongly protective of established property rights. Rather, the real action is over unclaimed territory, over the resolution of jurisdictional ambiguities. When the parliamentarians adjudicate jurisdictional ambiguities by referring a bill to one committee (and sometimes several committees), their decisions are binding. As one former staff director put it, "If you lose with the parliamentarians, you've lost forever."

Multiple Referrals

Since 1975 in the House (and informally, many years earlier in the Senate), it has been possible for one bill to be referred to more than one committee (Davidson and Oleszek 1992; Young and Cooper 1993). In recent

years, 15–20% of all House bills have been multiply referred, though the actual percentage of all public laws that were first multiply referred is somewhat less.² There are, in practice, two types of multiple referrals: joint and sequential.³ Joint referrals, in which one bill is sent to more than one committee at the same time, is the most common, comprising more than 94% of all multiple referrals in the Ninty-Ninth Congress, 1986–86 (Davidson, Olsezek and Kephart 1988). With sequential referrals, additional committees may get a chance at a bill only after (and only if) it is reported out of a lead committee.

At first, it might appear that multiple referrals could lead to jurisdictional free-for-alls, with identical bills going to multiple committees. Collie and Cooper (1989) argue that through multiple referrals, committees have sacrificed autonomy over their own issues for jurisdiction over issues in other committees. In practice, committees have most assuredly not surrendered autonomy. Though not specified in the written rules, the practice under joint referrals is that committees are limited to working only on issues within their established domain (Young and Cooper 1993). To do anything else welcomes challenges at the Rules Committee and on the floor, something most committees are eager to avoid. Jointly referred bills reinforce, rather than tear down, jurisdictional walls (King 1992). Committees under multiple referrals may share a bill, but they do not share jurisdictions. In fact, much of the jurisdiction game is geared to avoiding sharing turf at all.

I turn now to an examination of the House Commerce Committee's jurisdiction. On Capitol Hill, Commerce is considered the most jurisdictionally expansive House committee. We look at this committee to give the null hypothesis (the hypothesis that jurisdictions are static) its toughest test. If we fail to find common law adaptations here, we should not expect to find them anywhere. The strategies that committee entrepreneurs employ when going after new issues and appealing to the parliamentarians are the same regardless of the committee.

JURISDICTION OF THE HOUSE COMMERCE COMMITTEE

Originally named the Committee on Commerce and Manufacturers and dating from 1795, Commerce is one of the House's oldest committees. Its broad mandate was to "take into consideration all such petitions and matters of things touching the commerce and manufacturers of the United States" (U.S. House 1795, 376). The committee's jurisdiction was split in two in 1819, creating the Committee on Manufacturers (which was eliminated in 1911) and the Committee on Commerce. Commerce was renamed Interstate and Foreign Commerce in 1892 and became Energy and Commerce in 1981.

Statutory Jurisdiction of House Commerce

House Commerce's statutory jurisdiction is reported in Figure 1. The issues are listed in the order in which they appeared in the Rules. Much of the committee's statutory jurisdiction dates to the 1946 Legislative Reorganization Act (LRA). Beyond "interstate and foreign commerce generally," the committee was given property rights over civil aeronautics, communications, securities and exchanges, and public health, among other things. Some apparent gains and losses happened in 1974 and 1980, based on the 1974 Hansen–Bolling reforms (HR 988, 93d Congress) and the 1980 energy reforms (HR 549, 96th Congress).

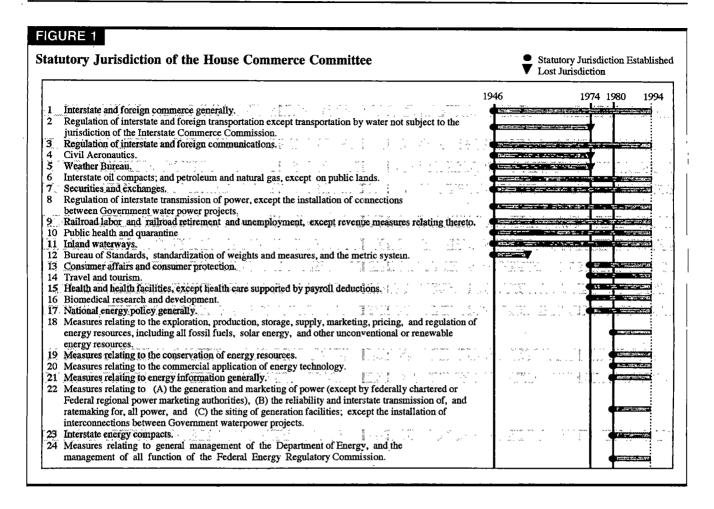
Commerce's jurisdiction is exceptionally broad, partly because the committee's fortunes have been tied to the Constitution's commerce clause, which has been a base from which federal powers have

expanded since the Great Depression.

In order to assess the relative importance and size of statutory as opposed to common law jurisdictions, we need a way to measure both. How should one go about it? The Smith and Deering (1990) approach, in which the number of issues listed in the rules plays an important part, fails to capture common law jurisdictions. A second approach might exploit committee calendars, but indexing schemes vary widely from committee to committee and from year to year. A third method—counting the number of entries in the books of precedents—is better. But Cannon's *Procedure* has not been published since 1963, and even if it were published regularly, not all referral precedents are recorded in such lists.⁴

I employ a method based on committee hearings.⁵ The Congressional Information Service maintains a computerized listing of all published hearings. For Commerce Committee records, I extracted the number of days the committee held hearings, the number of pages of hearing documents printed, and the number of bills associated with each hearing. After reading one-to-two-page descriptions, each subject raised in a hearing was coded. From 1947 through 1990, 2,534 hearings were examined, and 221 distinct topics were identified. This allows me to construct a time series of the rise and fall of issues in the Commerce Committee.

Beyond identifying issues, each topic was categorized as either based on the committee's statutory jurisdiction or based on bill referral precedents. Here it was important to be conservative and to err on the side of overestimating the percentage of the committee's activities based on the House rules. For example, for 1947, the following hearing topics were all identified as justifiable based on the committee's statutory jurisdiction (in parentheses): agreements between carriers (interstate trucking), Alaska airports (civil aeronautics), amendments to the Natural Gas Act (interstate pipelines), and iodized salt (public health). But also in 1947, the committee authorized the creation of the National Science Foundation, which may have its merits but is not even remotely mentioned in the 1947 House Rules Manual. The



National Science Foundation was coded as part of the committee's common law jurisdiction.

Figure 2 shows the percentage of the Commerce Committee's hearing activity on issues in the committee's statutory jurisdiction.⁶ Oversight hearings in which no bills were referred are excluded from the data set because it is the referrals, not simply the hearings, that establish binding precedents. The data span the period between the 1946 and 1974 reform acts.⁷ Hearing activity is measured by the percentage of the printed pages in hearings that were devoted to each topic.

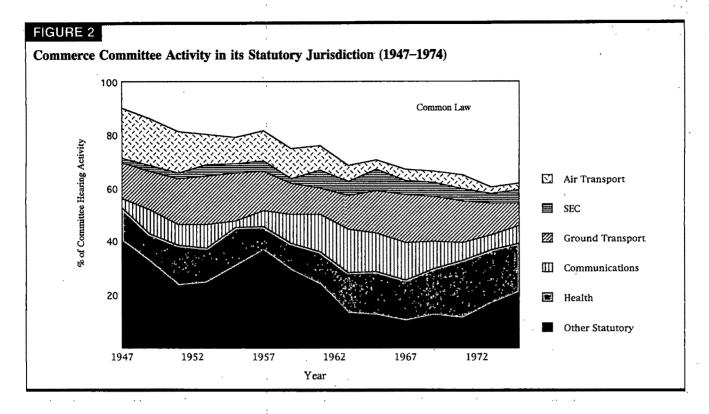
Several things stand out in Figure 2. First, immediately after the 1946 LRA, more than 90% of the committee's hearing activity was on issues in the committee's statutory jurisdiction. The rules of this period were remarkably accurate descriptions of what the committee was actually doing. For the most part, George Galloway (1946) and the other reformers apparently accomplished what they set out to do, but in the decades that followed, their jurisdictional specifications slowly collapsed in the face of new issues and gradual adjustments. Second, by 1974, the year before new jurisdictional wordings were incorporated into the rules, more than a third of the Commerce Committee's activities were on issues not in the statutory jurisdiction of the committee. If we extended the time line, we would see that merely a third of the committee's activities in 1990 were

granted to them in the rules in 1947. The remaining two-thirds of the committee's hearings have been on issues that were either given the committee through the 1974 and 1980 rules changes or taken by the committee through bill referral precedents. Further, the committee's common law jurisdiction more than tripled from 1946 to 1974 as the committee's turf expanded to include (among other things) certain patent infringements, daylight savings time, automobile insurance, and solid waste disposal.

Common Law Jurisdiction of House Commerce

Common law jurisdictions grow when an argument can be made that a new issue is closely related to something that a committee is already doing. One referral precedent is used to justify making another referral precedent, and so on. As a result, the nature of committee jurisdictions is that their expansion is dependent on the path that previous bill referrals have taken. Like the view of evolution in Stephen Jay Gould's (1989) Wonderful Life, committee jurisdictions would emerge differently if we reran the clock of history (see also Arthur 1988; David 1975; Dopfer 1991). Just one changed bill referral a generation ago could have made for a dramatically different constellation of jurisdictions today.

The path-dependent nature of jurisdictional change is evident in the Commerce Committee's



acquisition of health policy issues in the 1800s. Beginning with the "protection of American international and domestic trade markets and shipping rights," the committee was referred bills relating to navigation safety, which soon came to include quarantine provisions at seaports. Through the Commerce Committee in 1798, Congress created the Marine Hospital Service to oversee quarantines of immigrants at entry ports (United States, Public Health Service, 1976).

A second part of the path leading to the health turf grew from the dangers of steamboat travel. Beginning in the 1830s and continuing through the end of the century, steam-powered merchant ships ran a booming business. Boiler explosions were common, especially during the 1850s. From 1848 through 1852, 1,155 lives were lost in boiler explosions on the western rivers alone (Hunter 1949). The Marine Hospital Service located hospitals along heavily traveled rivers where boiler explosions and other maritime disasters were likely. When yellow fever and cholera epidemics hit later in the century, the Marine Hospital Service, soon renamed the U.S. Public Health Service, was the only federal health organization capable of helping out. ⁸ By the close of the nineteenth century, the Commerce Committee had gained "broad jurisdiction over bills relating to the subject of health generally" (Hinds 1907, 4:743). None of this was put into the committee's statutory jurisdiction until 1946. It was clever to link quarantine bills and navigation safety to exploding boilers and marine hospitals, but the strategy of drawing analogies between issues that a committee wants and issues that it already controls is typical.

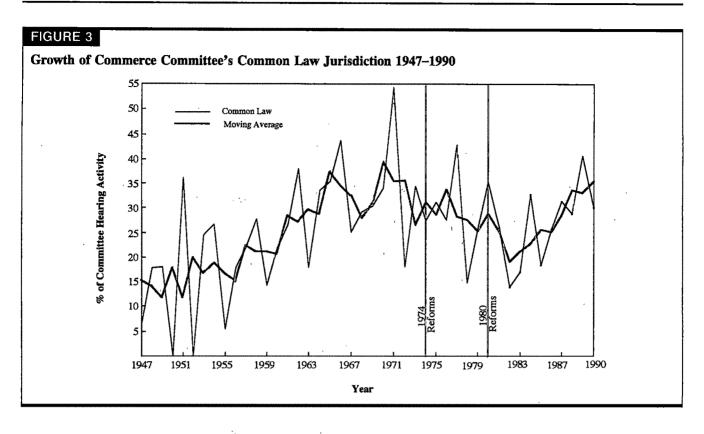
From 1947 through 1990, the Commerce Committee was referred bills on 133 issues that expanded the

committee's jurisdiction. That is an average of three precedent-setting bill referrals every year. Often, those expansions were minor. Commerce used its jurisdiction over travel and tourism in 1983 to solidify its claim to a bill related to international sporting events (United States, House, Committee on Energy and Commerce 1983). But the committee also took major steps into new territory. From its jurisdiction over interstate and foreign transportation, the committee gained jurisdiction over automobile safety in 1956. And the committee was then well positioned to respond to the regulatory challenges posed by Ralph Nader in the mid-1960s.

Of the 133 issues over which the Commerce Committee established a referral precedent from 1947 through 1990, 110 (82.7%) were issues raised in conjunction with a bill over which the committee had already established jurisdiction. Again, jurisdictional change is path-dependent.

Figure 2 provided some evidence that the Commerce Committee's common law jurisdiction expanded between the 1946 and 1974 reforms. A closer look at the growth of common law turf is provided in Figure 3. The figure is based on a content analysis of 1,485 hearing documents published by the Commerce Committee from 1947 through 1990. Only hearings on referred bills (52% of all hearings during the period) are reported.

From 1947 to 1974, the Commerce Committee's agenda gradually included more and more common law issues. Much of the increase was from consumer protection problems like the testing of food additives (1957), product-labeling laws (1958), seat-belt regulations (1962), the safety of children's toys (1969), and deceptive advertising (1971).



There are two trend lines in Figure 3: the percentage of common law hearing activity and a three-year moving average. The moving average indicates gradual upward change, but there are also wide swings from year to year as the committee launched into new areas. In 1958, for example, Commerce worked on a medical education bill (which could have gone to Education and Labor) and on air pollution legislation. The next year, its common law activity dropped by half as it turned its attention to the Securities and Exchange Commission and to railroad unemployment, both of which are in its statutory jurisdiction. Still, an upward trend is unmistakable between 1947 and 1974. Use of the committee's common law agenda peaked at 54.23% in 1971 with extensive hearings on air pollution control, solid waste recycling, cigarette labeling, and automobile safety, none of which was yet in the committee's statutory jurisdiction.

Between the 1974 and 1980 reforms, the percentage of the committee's hearing activity spent on common law issues averaged about 30% with no general trend up or down. The common law percentage did not drop after the 1974 reforms because Commerce launched into energy issues not yet in its statutory jurisdiction just as consumer protection issues (codified in the rules in 1974) began to die down. During the late 1970s, nearly all of the committee's common law activities were on energy-related issues such as synthetic liquid fuels, energy conservation, solar energy, and a national energy strategy. None of these issues was in the jurisdiction of any other committee during this period, so the committee staked a claim and then worked to protect it.

The 1980 reforms locked the Commerce Committee's energy jurisdiction into the House rules, and the committee's common law agenda fell from 35% to 15% in two years. The gradual, incremental process of common law expansion began anew: the committee has successfully staked claims to jurisdiction over the insurance industry, international trade, and some securities-related banking activities.

The rate of jurisdictional expansion under Chairman John Dingell (D-MI) in the 1980s was almost identical to the rate of expansion under Oren Harris (D-AK), who chaired the committee from 1957 through 1965. This might surprise some observers of Congress, because John Dingell is a "recognized master of territorial expansion" (Congressional Quarterly Almanac 1988, 357), while Oren Harris was sometimes dismissed as an unaggressive old southern Democrat. However, there is much more to the jurisdiction game than personalities. Put John Dingell in charge of the Merchant Marine and Fisheries Committee and he would still be a policy entrepreneur, but he would not have as many successes as he has had on Commerce because the Merchant Marine's jurisdiction is not as broad to begin with. Entrepreneurs need to be able to make a plausible claim that a jurisdictionally ambiguous issue is proximate to their committee's established turf. In the jurisdiction game, the rich get richer and the poor get the same old bills.

Two lessons seem clear from this look at the Commerce Committee's jurisdiction. First, jurisdictions are malleable through common law referral precedents. Second, the trend toward common law issues appears incremental, as committees gradually move away from some issues and embrace others. What, then, of periodic jurisdictional reforms?

JURISDICTIONS AND THE CONSEQUENCES OF REFORMS

Reform seems to imply a plan, a thoughtful way to get from how things are to how things ought to be. Congressional reforms are marked by commissions, special committees, and hoopla. The 1946 LRA was widely supported in and reported by the news media, and attention focused on congressional reform a generation later through the 1974 Bolling Committee. In both cases, efficiency experts were called upon to help Congress run more "efficiently" and "effectively." And in both cases, Congress passed reform legislation.

Perhaps, as implied by the way the 1946 and 1974 jurisdiction reforms are sometimes portrayed, congressional reforms are purposeful and collectively ratified breaks from the past (Galloway 1955; Rieselbach 1986). Thus one interpretation of congressional reforms is that they are sporadic but highly significant tools for changing the institution: proposals are carefully planned and collectively ratified. Call this the rational problem-solving model.

Former Speaker Thomas Reed described reform situations as times when "an indefinable something is to be done, in a way nobody knows how, at a time nobody knows when, that will accomplish nobody knows what" (quoted in Henning 1989, 233). Rational problem solving is often unrealistic: "However attractive such an orderly reformation might be, its preconditions—consensus on goals and precise calculations of means—ends relationships—are rarely realizable in real-world situations" (Davidson, Kovenock and O'Leary 1966, xii).

Of course, the truth about what congressional reforms accomplish likely lies someplace between Speaker Reed's characterization and the rational problem-solving model. When legislatures vote on reform proposals, the reforms are indeed wellplanned and highly publicized events, but what do they really change? Some reforms, like those following the revolt against Speaker Cannon in 1910, are clear and significant breaks from the past, and it is now in fashion to call recent Congresses "postreform," as if the mid-1970s qualitatively changed the institution. What of the landmark 1946 LRA, the 1974 Bolling Committee jurisdiction reforms, and the 1980 changes in energy jurisdictions (Davidson and Oleszek 1977; Galloway 1955; Uslaner 1989)? These three reforms are first pointed to when one takes jurisdictional change seriously. If these are to be our models for how the internal structures of Congress are reformed, what lessons can they teach us?

I shall argue that formal rules changes (like the writing of statutory jurisdictions) often *follow* institutional changes (such as common law adaptations in committee turf). If we want to understand change,

we should focus not on "reforms" but on the incremental day-to-day changes in the unwritten rules. Former representative Richard Bolling (D-MO) understood the limitations of formal rules changes, calling them "the product of modification, change, and codification" (1965, 110). Codification comes last.

1946 Legislative Reorganization Act

World War II Congresses were widely ridiculed for alleged inefficiency, myopia, and (most of all) obstruction of President Roosevelt's war efforts (La Follette 1943; Perkins 1944). Congress, designed for a horse-and-buggy age, was said to be unprepared for the blitzkrieg of politics in the 1940s and beyond. President Franklin Roosevelt seemed to have had the upper hand in legislative battles, resorting to veiled threats if Congress failed to pass his bills by set deadlines.

In early 1941, the American Political Science Association's Committee on Congress (1945) launched a study of congressional mechanisms (see also Matthews 1981). Both the Committee on Congress report, The Reorganization of Congress and the National Planning Association's Strengthening the Congress (Heller 1945) emphasized the need to restructure committee jurisdictions and powers so that Congress could be a more potent check on the executive branch. These reports helped bring about the formation of the Joint Committee on the Organization of Congress in 1945, which was cochaired by Wisconsin Senator Robert La Follette, Jr. and Oklahoma Representative Mike Monroney. Perhaps the most important impact of the American Political Science Association committee's report was that it helped launch George Galloway, the report's author, into the position of staff director of Congress's special joint committee on reform.

Galloway, La Follette, and Monroney were the primary architects of the 1946 LRA. Leading their list of objectives were streamlining committee structures, eliminating the use of select committees, and clarifying committee jurisdictions (Galloway 1955, 591). The reformers were also political operators, and they knew they would have to confront committee chairs who were content with their perks and powers under the status quo. Reformers also had to mollify the House and Senate Rules Committees, which specifically precluded the Joint Committee from making recommendations that would overturn any of the House or Senate Rules.

The 1946 LRA reduced the number of standing committees from 33 to 15 in the Senate and from 48 to 19 in the House. Fifty-eight percent of the committees disappeared overnight. Also, for the first time in the history of Congress, committee jurisdictions were carefully delineated in the House and Senate Rules. ¹² On the face of it, these seem like remarkable achievements. Galloway set the tone for most subsequent commentaries on the act, calling it "the outstanding development in the organization and operation of Congress during the past fifty years" (1961, 57). Then

again, Galloway had a vested interest in writing a favorable history.

The 1946 LRA passed with the help of committee chairs. Since over half of them were guaranteed to lose their positions, why did they support the reforms? The answer is that chairs of inactive committees were assured that they would benefit from the increase in staffs even though they would lose their committee staffs, and less-senior chairs were promised seats on more prestigious panels (Ripley 1969; Polsby, Gallaher and Rundquist 1969). Politically pragmatic deals like these, not a keen eye for the most "efficient" committee structure, drove the consolidation of committees.

How would one go about streamlining a committee system that begins with 48 committees in the House? Reformers began by aiming for committees with 25 to 27 members each, and they wanted to limit almost every member to one committee assignment. The number of postreform committees was simply derived by dividing the number of representatives (435) by the desired size of committees (25) and rounding

up, yielding 19.

Starting with 48 committees, there were 1,128 possible pairwise combinations that could have been used to create 19 committees, and the apparent decision rule employed in selecting among all the possible pairwise combinations was to emphasize prereform patterns of shared committee memberships, because that is what was politically expedient. It was expedient because committees that shared large numbers of members were, in a sense, already working together, and the prereform committees could be made into postreform subcommittees on a single panel.

The hypothesis that prereform intercommittee memberships were central to the postreform structure is tested in Table 1, where possible pairwise combinations of committees are ranked by the sum of joint memberships (Two committees each sharing 25% of their members would score 50%.) Nine committees¹³ with nine or fewer members are excluded because their small size would skew the percentages and because eight were combined into the House Administration Committee. This leaves 39 committees, or 741 possible pairwise combinations of prereform panels. For these 39 committees, 44 pairwise combinations were actually used. Of all the possible ways of shoehorning the committees together, 10 of the 15 committees with the highest shared memberships ended up together. That rate is much higher than one would expect by chance $(p = .000454 \times$ 10^{-6}).

For a committee system in which most legislators served on only one panel, the results in Table 1 are striking. Intercommittee memberships drive the results. Sixty percent of the members of the Public Lands Committee also served on the Committee on Irrigation and Reclamation, and 39% of the Public Lands Committee served on Mines and Mining. Likewise, 37% of the Mines and Mining members served on Indian Affairs, and 30% of the Indian

TABLE 1

Intercommittee Memberships of House Committees Consolidated by the 1946 Legislative Reorganization Act

	POSSIBLE PAIRWISE	SUM OF JOINT MEMBERSHIP ^b	COMMITTEE AFTER
	COMBINATIONS ^a	(%)	1946 LRA ^c
	Public Lands, Irrigation	117	Interior
	Mining, Indian Affairs	76	Interior
	Public Lands, Mining	72	Interior
	Public Lands, Indian		
Ì	Affiars	70	Interior
İ	Public Buildings,		
١	Pensions	66	 '
İ	Irrigation, Insular Affairs	65	Interior
Ì	Irrigation, Indian Affairs	62	Interior
l	Civil Service, Claims	5 9	
l	Flood Control, Roads	58	Public Works
Ì	Public Buildings,		
į	Patents	58	
	Claims, Revision of		
١	Laws	56	Judiciary
١	Expenditures, Accounts	55	
	Irrigation, Mining	53	Interior
l	Civil Service, Census	52	Post Office
I	Rivers, Territories	52	

This analysis excludes the nine prereform committees that had nine or fewer members. This leaves 741 possible pairwise combinations of prereform committees. Eight of the nine small committees excluded from this analysis were combined into one committee, House Administration. The ninth, UnAmerican Activities, was unaffected by the reforms.

This column reports the sum of two percentages: (1) the percentage of committee A that is also in committee B and (2) the percentage of committee B that is also in committee A. (These two percentages are rarely the same, because committee size varies.)

Of the 741 possible pairwise combinations, 44 were used in the 1946 LRA. This column reports the destination committee for the combinations that were actually used.

Affairs members were on Irrigation. Is it any surprise that these committees were bundled together? The postreform committees embraced membership patterns found before the reforms, thereby reinforcing coalitions rather than forging new ones.

Were committee jurisdictions significantly changed by the 1946 LRA? No. Jurisdictions were codified, but they were not significantly changed. Ten of the 19 postreform committees were spared being consolidated with other committees, and they were jurisdictionally identical in every way to their prereform counterparts. The remaining 9 committees embraced established patterns of jurisdictional coordination. In almost every case, the specific descriptive words used to list committee jurisdictions, though new to the written rules after 1946, were taken verbatim from earlier books of precedents compiled by Asher Hinds and Clarence Cannon.

1974 Jurisdictional Reforms

Committee reform was on the agenda once again in 1974 in the form of the Bolling Committee, whose

express purpose was "a wholesale realignment of jurisdictions and a limitation of one major committee per member" (Congressional Quarterly Almanac 1974, 634). The Bolling Committee proposals faced stiff opposition from committee chairs, and almost all of the reform suggestions were defeated in the House Democratic Caucus (Davidson and Oleszek 1977). A second committee, chaired by Washington Democrat Julia Butler Hansen, was established by the Democratic Caucus to offer scaled-down alternatives to the Bolling Committee recommendations. HR 988 primarily reflected the more modest Hansen Committee recommendations and passed the House on 8 October 1974. Noticeable changes were made in the statutory jurisdictions of committees, including transportation, health, and banking. Most transportation issues were transferred to the Public Works Committee, renamed the Committee on Public Works and Transportation. This included aviation and surface transportation (from Interstate and Foreign Commerce) and mass transit (from the Committee on Banking and Currency). Interstate and Foreign Commerce was given jurisdiction over "health and health facilities, except health care supported by payroll deductions" (which went to the Ways and Means Committee). The Banking and Currency Committee, renamed Banking, Currency, and Housing, was given jurisdiction over federal monetary policy, urban development, and international finance, among other issues. The Foreign Affairs Committee lost jurisdiction over international monetary organizations.

Like the realignment of committees in 1946, these and other jurisdictional realignments appear to have been significant reforms shifting issues from committee to committee and adding otherwise overlooked subjects to committee agendas. The 1974 reforms changed the Commerce Committee's statutory jurisdiction by adding four issues and deleting three. The committee gained consumer affairs and consumer protection, travel and tourism, health and health facilities, and biomedical research and development. Lost to other committees was the committee's jurisdiction over the Weather Bureau, civil aeronautics, and almost all transportation issues except railroads.

Looking just at the Rules Manual (recall Figure 1), Commerce's acquisition of consumer protection issues seems to be an important (though delayed) institutional response to the calls for consumer protection legislation throughout the late 1960s. In fact, though, the Commerce Committee helped define what we mean by "consumer protection" by its actions in the 1960s (Nadel 1971). Even before consumer protection began to be thought of as a certain cluster of issues, the Commerce Committee expanded its common law jurisdiction to include health and safety problems. Paired with its statutory jurisdiction covering aviation, the committee regulated food inspections of agricultural products shipped by air in 1954. In 1957, the committee held hearings on seven related bills to "prohibit the use of new chemical food additives without adequate pretesting for safety" (U.S. House Committee on Interstate and Foreign Commerce 1957). The committee began staking out jurisdiction on the deceptive labeling of automobile stickers a year later. Hearings on the regulation of cigarette advertising were held in 1965, followed by investigations into the safety of children's toys and radiation from household electric devices. Through the active efforts of California Democrat John Moss, the committee was referred a series of product labeling bills, and by the time "consumer protection" began to be understood in its modern sense, the Commerce Committee had already established a precedent-setting track record on consumer issues. Consumer protection had unquestionably become part of the committee's jurisdiction. When the Consumer Product Safety Act was introduced in Congress in 1971, the bill was naturally, and without objection, referred to the Commerce Committeethis, four years before the Hansen Committee added consumer protection to Commerce's statutory juris-

A similar pattern of referral precedents confirms that most health issues were in the Commerce Committee's jurisdiction before the 1974 reforms, as well. Because of the 1946 LRA, Commerce claimed "public health and quarantine" as part of its statutory jurisdiction, and throughout the 1950s, more than a quarter of the committee's activity was related to this general jurisdictional grant. However, the committee also moved into health issues that were not necessarily implied by the 1946 reforms. The committee was working on a national health insurance program by 1949. By 1957, committee staffers were investigating the quality of instruction at dental schools, an issue that could have been legitimately claimed by the Committee on Education and Labor. The humane treatment of animals used in medical research was the subject of two bills referred to the committee in 1962. And as drug abuse problems became more acute, the committee claimed new and politically relevant turf by drawing parallels between drug abuse and public health problems. This eventually led to the committee's oversight of several programs overseen by the Drug Enforcement Agency, a function that might seem more naturally to belong to the Judiciary Committee.

The Bolling Committee's original plan was to recast the Committee on Interstate and Foreign Commerce as the Committee on Commerce and Health, and this was to include wrestling Medicare issues from the Ways and Means Committee. Medicare issues were often approached through amendments to the Social Security Act, which had been in the statutory jurisdiction of the Ways and Means Committee since 1946. The Hansen Committee compromise maintained the preexisting distinction between the committees by precluding the Commerce Committee from extending into "health care supported by payroll deductions." With respect to health, the net effect of the 1974 reforms was to write down in the Rules Manual the established common law jurisdictions governing bill referrals on health issues.

Travel and tourism issues were codified in the

House Rules in 1974, but the Commerce Committee reported a bill establishing the U.S. Travel Bureau as early as 1948. The committee also established the Office of International Travel and Tourism in 1960 to encourage foreign tourism in the United States. The Travel Bureau was established within the Department of the Interior, but it was subject to Commerce oversight (not Interior Committee oversight) because of the referral precedent. When the Interior Committee was referred a bill on the Arctic Winter Games in 1972, Chair Wayne Aspinall (D-CO) asked that the issue be rereferred to the Commerce Committee because it involved tourism. Finally, bills related to biomedical research and development had been referred to both the Commerce and Science Committees, but Commerce was working actively on the issue at the time that the jurisdictional reforms were being contemplated. During September 1974 alone, the Commerce Committee held hearings on 11 bills dealing with biomedical research and development.

In the four areas (consumer protection, health, tourism, and biomedical research) over which the Commerce Committee gained statutory jurisdiction in 1974, common law jurisdictions had already been established. The Hansen Committee took the path of least resistance, embracing the status quo in the name of reform. However, the Commerce Committee also lost three areas of jurisdiction in 1974. If statutory jurisdictions ratify common law jurisdictions, then we would expect to find that Commerce's activity on the issues that were lost (primarily transportation-related) had diminished significantly from 1947 to 1974.

This jurisdictional "loss" can be examined by looking at the percentage of committee hearings that were related to transportation issues. Immediately following the 1946 act, about a quarter of the committee's attention went to transportation (primarily aviation). The committee oversaw the development of a national system of airports and held investigations into accidents. Committee staffers in the Eightieth and Eighty-First Congresses devoted to transportation a great deal of attention that could easily have gone to other issues. By 1974, the committee activity on transportation issues had dropped below 5% of the committee's pages of hearings per Congress. Of that 5%, almost all of the attention went to the routine business of overseeing and maintaining long-established airports. The committee was not sponsoring new transportation projects. Removing transportation policy from the Commerce Committee's statutory jurisdiction was no great loss because they had let the turf lay fallow for years.

While the Commerce Committee largely ignored transportation issues throughout the 1960s and early 1970s, Public Works was increasingly active, in part because it oversaw the Highway Trust Fund and could pay for new programs out of its own committee. When the 1974 reforms were codified, jurisdictional changes in transportation that had been underway since the end of World War II were simply written into the Rules. The 1974 "reforms," whether expanding or subtracting from a committee's statu-

tory jurisdiction, reflected the incremental common law changes that had been in force, in some cases, for decades.

1980 Energy Jurisdiction Reforms

In 1980, the Congress passed a jurisdictional reform proposal that was originally intended to focus energy issues in one committee, but like the 1974 reforms, this latest round of statutory changes merely updated the rules and locked in common law turf that had been taken during the 1970s.

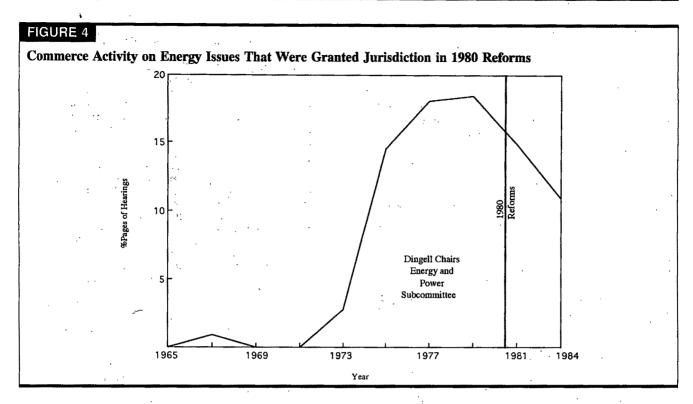
The jurisdictional tangle on energy issues seemed especially acute in the late 1970s as the country faced its second energy crisis in the decade (Jones and Strahan 1985). A report by the House Select Committee on Committees decried the consequences of common law jurisdictional expansions, noting: "The present operational structure in Congress for energy jurisdiction is fragmented among numerous committees. It is now as much a product of legislation initiated by a particular committee or assigned to it by the Parliamentarian as it is the result of carefully circumscribed rules and procedures" (quoted in Davidson and Oleszek 1977, 53).

After several years of what Eric Uslaner calls "jungle warfare over jurisdictions," two reform proposals, one by the House Select Committee on Committees and another last minute substitute by the Select Committee's chair Jerry Patterson (D-CA), were aimed at creating a new Energy Committee and taking all energy issues away from Commerce, Interior, and Public Works.

John Dingell, on the verge of becoming the Commerce Committee's chair, led the other committee chairs in a floor fight against the Select Committee proposal, which would have stripped Dingell's Energy and Power Subcommittee of much of its jurisdiction and given it to a new and separate energy committee. A Commerce Committee alternative proposal was offered that solidified Commerce as the panel with principal jurisdiction over national energy policy. The attempt to take energy away from Commerce was thwarted, and the apparent reforms amounted to little more than updating the written rules to reflect reality.

The real battle over energy jurisdiction did not happen between Dingell and the 1980 Select Committee. Rather, the Commerce Committee gained important parts of the energy turf during the mid-to-late 1970s. From his newly created Subcommittee on Energy and Power, John Dingell managed, during the late 1970s, to gain jurisdiction over a wide variety of jurisdictionally ambiguous energy issues, like conservation and a "national" energy policy. Though the Commerce Committee had done little to expand its energy jurisdiction before 1974, that was certainly not true of other committees.

Figure 4 reports the Commerce Committee's hearing activity on energy issues that were granted to the committee in the 1980 reforms. In the year before the reforms, more than 18% of the committee's efforts



were on energy issues not yet formally granted to them in the rules. From the time that John Dingell's Subcommittee on Energy and Power was created in 1975, the committee sought to establish binding referral precedents over otherwise unclaimed energy turf.

With a subcommittee in place, hearings on proposed oil import taxes were held, followed quickly by investigations into synthetic liquid fuels, solar energy, and energy from helium. Soon the committee began publishing a biennial compilation of energy-related acts within the jurisdiction of the committee. Dingell also hosted widely publicized hearings outlining a national energy strategy, hearings so extensive that they filled eight large volumes. In 1977, Commerce reported a bill creating the Department of Energy: its oversight would firmly establish the Commerce Committee as a major player in all energy legislation.

As with the earlier reforms, the Commerce Committee's common law jurisdiction had expanded before its statutory jurisdiction was recast. Uslaner aptly called the 1980 jurisdictional changes "deja vu" reforms, "a reinforcing of the status quo in the name of reform" (1989, 154).

Reform is not synonymous with change. Our focus should not be on the temporary floor majorities mustered to pass the 1946, 1974, and 1980 "reforms." Rather, jurisdictional change happens through bill referrals, and our attention should be on policy entrepreneurs and the parliamentarians.

DISCUSSION

It is time to recast how political scientists talk about congressional committee jurisdictions and how they think about congressional reform. Within the rules of the jurisdiction game, turf is up for grabs in legislatures. With clever bill drafting and incremental moves into new territories (strategies discussed in King 1992), the nature of congressional committee jurisdictions is lively indeed. Jurisdictional fragmentation is a direct result of so many committees trying to stake out claims to pieces of larger issues like the environment and national health care. Devoid of common law jurisdictions and the politics of bill referrals, no static notions about Congress can account for the ongoing border wars among committees.

Few things in Congress are more highly sought than committee turf over politically "hot" issues. Subsequent jurisdictional changes ripple through the institution, affecting the power and prestige of the committees. It is no secret why House Commerce has become an especially attractive committee assignment in the last 20 years (Munger 1988): its turf is large and rewarding.

One can scarcely read journalistic reports about Congress without seeing evidence of border wars (Moore 1993; Stanfield 1988). Yet, oddly, political scientists are only now beginning to pay close attention to institutional change in Congress. For the last 20 years at least, the scholarly emphasis has been on finding sources of institutional stability and on specifying the nature of equilibria (Shepsle 1986). But for all the attention equilibria have received, we should never lose sight of the forces that push institutions toward incremental change (Riker 1980; Rowe 1989).

We need a new vocabulary for talking about jurisdictions. Statutory jurisdictions (the usual way political scientists talk about turf) are written down in the House and Senate Rules. Common law jurisdictions emerge through binding bill referral precedents. The

real dynamics of committee jurisdictions are found in referral precedents, not in dusty old books of rules. Also, we need to think about institutional change. It has been shown that jurisdictional "reforms" (i.e., the 1946, 1974, and 1980 changes in the House Rules) simply codified bill referral precedents. Common law jurisdictions were written into the rules, yielding little more than the appearance of change. In the work-a-day world of Congress, institutional change may be glacial, but it is always happening. Instead of focusing all of our attention on what brings about periodic reforms (Rieselbach 1986), we should pay more attention to how rational-actor legislators interact with and shape institutions day to day (Dodd 1986, 1993).

The policy consequences of the jurisdiction game are only now beginning to be understood. Baumgartner and Jones (1993) show how committees can redefine old issues (e.g., by recasting tobacco policy from "agricultural" to "health"). Over time, this breaks down issue monopolies. At the same time, however, the relentless pursuit of turf has left the House and Senate committee systems highly fragmented. There seems to be an almost inevitable tension between gridlock and how accessible a committee system is to various groups. Maybe it is the old trade-off between representation and governance (Shepsle 1988) but in a slightly different form. Only with a view of jurisdictions as malleable and committees as political battle grounds over turf can we begin to think about the causes and consequences of jurisdictional fragmentation.

I do not know how common it is for political institutions to go through "reforms" only to codify incremental and long-standing changes. It happens in the U.S. Congress, and I suspect it happens in every majority-rule institution. Because it takes a majority to update the written rules of most political bodies, the easiest thing to agree on is to embrace the status quo. Does that mean that the status quo is itself stable? Certainly not. It does not take an act of Congress to change the status quo. That happens every time written rules are reinterpreted, every time behavior is modified, and every time a precedent is set. We need to pay more attention to how and why these things are done.

Notes

Thanks are due Lawrence Dodd, Richard Hall, John Jackson, Bryan Jones, John Kingdon, Kirsten Syverson, and the congressional lobbyists, staffers, and legislators who shared their

insights with me.

1. LaRue and Rothenberg (1992), Shipan (1993), and Krehbiel and Lavin (1993) all focus on a turf war involving the House Appropriations Committee. But turf wars involving the Appropriations Committee are atypical and may give the impression that property rights can be easily undermined: they cannot. In general, turf wars are over new territory, not over established ground. The Appropriations Committee is not supposed to write (authorize) legislation on spending bills. By the House Rules, authorizing legislation is something the authorizing (or policy) committees are supposed to do

(House Rule XXI-2). That kind of "turf war" is often done with the acquiescence of the authorizing committee, and it

sets no binding precedent.

2. During the Ninety-Eighth Congress, 7.2% of all jointly referred measures were ultimately passed on the House floor. This compares to a success rate of nearly 17% for all singly referred measures during the same period. Oleszek, Davidson, and Kephart speculate that jointly referred bills are less likely to succeed because "by their very nature, bills that are sent to two or more committees are complex and multifaceted. As a general proposition, this raises the possibility of conflict and controversy" (1986, 44). But the parliamentarians suggest another reason. "A great many jointly referred bills are dead on arrival anyway," said one, "so there's no harm in giving everyone a piece of the action."

3. A third type of multiple referral—a split referral—is allowed through the House Rules, but it is no longer used. With split referrals, a bill is supposed to be divided up by sections or titles and given to the committees of jurisdiction. That is time-consuming. Instead, joint referrals serve the same function. Whole bills are given to multiple committees with the explicit understanding that they will work only on sections over which their committees have jurisdiction.

4. Cannon's (1963) Procedure has been replaced by Deschler's (1977) Precedents, which does not list common law

issues separately.

5. In some respects, this approach is a descendant of the uses of hearings by Dodd and Schott (1979, 168–79) and Price (1979). My coding differs in that I specifically look at the distinction between common law and statutory issues and record how and when new issues are linked in a hearing to

- existing issues.
 6. Figure 2 is based on 746 hearing documents from 1947 through 1974. This number only includes hearings on referred bills. A three-year moving average is reported. Subjects in Figure 2 were categorized in the following manner. Health included public health and quarantine generally, drug abuse, alcohol abuse, and children's health. Communications included the regulation of interstate and foreign communications generally, telephones, telegraphs, radios, and television. Ground transportation included regulation of interstate railroads, hazardous materials transportation, regulation of interstate busses, regulation of interstate trucks, regulation of interstate pipelines, railroad labor, railroad retirement, and railroad unemployment. Securities and exchanges included Security Exchange Commission issues generally and mergers and acquisitions. Air transportation included civil aeronautics (primarily accident investigations and airport construction) and postal rates on air mail. Other statutory issues not otherwise classified included interstate and foreign commerce generally, barriers to foreign trade, the Weather Bureau, interstate oil compacts and petroleum and natural gas (except on public lands), inland waterways, the Bureau of Standards, standardization of weights and measures, the metric system, the regulation of interstate transmission of power (except the installation of connections between government water-power projects), war claims, committee housekeeping, and general oversight hearings.
- 7. To reiterate a point made in the text, Figure 2 excludes hearings for which there was no bill referral, because committees can hold oversight hearings on issues not in their legislative jurisdiction. Since the mid-1970s, only about half of the committee's hearings have focused on referred bills, but during 1947-74, 83% of the hearings addressed referred bills.

8. It is because of the lineage of the Marine Hospital Service that the U.S. surgeon general's official seal still includes a ship's anchor. That, in turn, came from the House Commerce Committee's jurisdiction over international ship-

ping.

9. This followed the creation by Commerce of the House Special Subcommittee on Traffic Safety. In a fairly typical pattern, the committee first received a bill, HR 9836, in the Eighty-Fourth Congress establishing Interstate Commerce Commission authority over the transportation safety regulations for migrant farm workers. This was justified at the federal level because migrant farm workers often cross state lines (see United States, House, Interstate and Foreign Commerce Committee, 1956b). One month later, the Traffic Safety Subcommittee conducted investigatory hearings into other safety problems (see U.S., House, Interstate and Foreign Commerce Committee 1956a).

10. As is discussed in the text, the hearing documents are maintained by the Congressional Information Service and were accessed through their CD-ROM records.

11. Of course, the meaning of the words efficient and effective cannot be divorced from one's political views. Alexander Hamilton, as secretary of the treasury in Washington's first administration, complained that it would be inefficient to create a House Ways and Means Committee to oversee the national treasury. He pushed hard to keep the purse in the executive's hands by waving the "efficiency" flag. Among the 1946 reformers, one's view of what an effective legislature might look like was strongly related to whether one chafed at President Franklin Roosevelt's programs. A strong president seemed to imply a weak Congress, which was more troubling

to Republicans than Democrats in the 1940s. 12. The 1946 Legislative Reorganization Act most important accomplishment has nothing to do with these two reform elements and is often overlooked; it set in motion the professionalization (and proliferation) of congressional staff (Davidson 1990; Dodd and Schott 1979).

13. The nine excluded were Disposition of Executive Papers; Elections 1, 2, and 3; Enrolled Bills; the Library Committee; Memorials; Printing; and the Committee on Un-American Activities.

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SHORTCUTS VERSUS ENCYCLOPEDIAS: INFORMATION AND VOTING BEHAVIOR IN CALIFORNIA INSURANCE REFORM ELECTIONS

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oters in mass elections are notorious for their apparent lack of information about relevant political matters. While some scholars argue that an electorate of well-informed voters is necessary for the production of responsive electoral outcomes, others argue that apparently ignorant voters will suffice because they can adapt their behavior to the complexity of electoral choice. To evaluate the validity of these arguments, I develop and analyze a survey of California voters who faced five complicated insurance reform ballot initiatives. I find that access to a particular class of widely available information shortcuts allowed badly informed voters to emulate the behavior of relatively well informed voters. This finding is suggestive of the conditions under which voters who lack encyclopedic information about the content of electoral debates can nevertheless use information shortcuts to vote as though they were well informed.

with countless examples of voter ignorance. While the existence of the badly informed voter is now a central part of political science's intellectual heritage, the implications of voter ignorance continue to be vigorously debated. Many scholars and pundits argue that widespread voter ignorance leads to voting behavior and electoral outcomes that are meaningless representations of voter interests. Other political observers argue that voters find ways to adapt to their apparent lack of information. These scholars conclude that voting behavior and electoral outcomes provide valuable measures of voter interests.

To understand whether or not voters can successfully overcome their lack of information, it is instructive to consider briefly how voters can obtain information about the personal consequences of electoral outcomes. I conduct this review in the particularly appropriate context of a direct legislation election. In direct legislation elections (e.g., the initiative and the referendum), voters make selections from an exogenously determined menu of specific policy alternatives called propositions. A defining characteristic of many propositions is complexity.3 Since voters who encounter a complex proposition for the first time are likely to be confused about the consequences of its approval, the extent to which voters can adapt to their initial condition of ignorance will determine how well they can promote their own interests through the act of voting.

Voters who have an interest in the outcome of a direct legislation election might first consider gathering facts about a proposition from the official document that describes its content. However, these documents are usually lengthy and/or filled with technical language. As a result, voters in large electorates who consider their opportunity costs may decide that the acquisition of "encyclopedic" information is not a worthwhile activity.

As an alternative to the costly acquisition of ency-

clopedic information, voters may choose to employ information shortcuts. For example, voters can acquire information about the preferences or opinions of friends, coworkers, political parties, or other groups, which they may then use to infer how a proposition will affect them. The appeal of these information shortcuts is that they generally require relatively little effort to acquire. The drawback of these shortcuts is that they may be unreliable, since relatively well informed information providers may have incentives to mislead relatively uninformed voters.

Because many of my colleagues and I were curious about the extent to which relatively uninformed voters could use information shortcuts to cast the same votes they would have cast if better informed, I designed and administered an exit poll of California voters who, in 1988, were confronted by five distinct and complex insurance reform propositions. The survey responses allow me to identify a class of voters who, while appearing to possess relatively low levels of encyclopedic knowledge about the content of insurance reform initiatives, used an information shortcut that allowed them to emulate the behavior of well-informed voters. Specifically, I find that relatively uninformed voters who could correctly identify the insurance industry's official position on a particular proposition were much more likely to emulate the behavior of relatively well informed voters on that proposition than were similarly uninformed voters who did not know the insurance industry's position. I conclude from this finding that if relatively well informed voters are most likely to cast the votes that are consistent with their own interests, then knowledge of the insurance industry's position allowed voters who might otherwise be classified as ignorant to act as they would have if they had taken the time and effort necessary to acquire encyclopedic information. While the analysis I present does not lead me to conclude that shortcuts will always be sufficient to help uninformed voters overcome their lack of knowledge, I believe that it is suggestive of the conditions under which voters who have not acquired encyclopedic knowledge can vote as though they had.

Next, I offer a brief history of the events leading up to the insurance reform elections. I then place relevant aspects of this history within the context of previous theoretical and empirical research on voter decision making in order to generate predictions about how different types of information should affect voting behavior. I then offer a description of the survey instrument's construction and execution and employ a number of statistical tests to identify the effects of certain types of information on respondent voting behavior. With this survey, and related theoretical and experimental research on the responsiveness of direct legislation, I hope to advance more general debates about the nature of voter competence and the substantive meaning of electoral outcomes.

INSURANCE REFORM AND THE INFORMATION PROBLEM

What events led up to the insurance reform elections and how do they figure in the context of previous work on voter decision making?

A Brief History

In 1987, California drivers paid the third highest auto insurance rates in the nation. Both the state's trial lawyers-who receive half of their case load from automobile accident claims (Reich 1988a)-and the insurance industry recognized the widespread public support for regulatory reform and both were interested in influencing the legislative reform agenda. Each group publicly blamed the other for the recent rapid increase in auto insurance premiums. Insurance industry spokesmen argued that higher rates were caused by skyrocketing legal costs and publicly supported regulatory reforms that assigned most of the costs of reform to trial lawyers (e.g., laws that limit attorney fees, decrease the likelihood of large settlements, or both.) In contrast, the California Trial Lawyers Association (CTLA) publicly portrayed the insurance industry as greedy oligopolists who were conspiring against consumers. The CTLA supported regulatory reform that assigned most of the costs of reform to the insurance industry (e.g., the elimination of the insurance industry's exemption from many of California's antitrust laws).

Both groups attempted to influence insurance reform through their lobbying efforts in the state capitol. In this domain, the insurance reform debate was a battle of Titans. Both the insurers and attorneys had what were among the most influential lobbies in California (Reich 1987a). Perhaps coincidentally, all attempts at reform died in state legislative committees.⁵

When the legislative stalemate seemed destined to outlast calendar year 1987, a number of consumer

groups, who had been effectively shut out of the insurance reform debate in the legislature, voiced their intent to place an initiative on the November 1988 ballot. By submitting an initiative, consumer groups would not only gain control of the reform agenda, but they might also compel other groups, who would be opposed to their initiative, to spend millions of dollars to protect the relatively favorable status quo. In order to avoid a costly initiative campaign, insurance industry representatives and the CTLA made several attempts to form coalitions with the consumer groups that were preparing an electoral strategy. In addition, insurer and attorney groups attempted to form legislation-supporting coalitions with each other and each drafted its own separate initiative in the event that no agreement could be reached. By the time the deadline for qualifying ballot measures had arrived, however, no coalitions had formed.⁶ As a consequence, the three groups (the insurance industry, the trial lawyers, and the consumer activists) placed five different insurance reform initiatives on the November 1988 ballot.

Only one of the five qualifying propositions passed on Election Day: Proposition 103 was sponsored by the consumer activist group Voter Revolt (whose primary spokesman during the campaign was Ralph Nader) and received 51.1% of the vote. Proposition 103 called for the removal of the insurance industry's antitrust exemption, public hearings as a prerequisite for rate changes, auto insurance premiums to be determined primarily by driving record (as opposed to where one lived), a premium discount for "good drivers," election of the state's insurance commissioner (rather than gubernatorial appointment), and a mandatory 20% reduction on all auto insurance premiums.

Of the four initiatives that lost, one was sponsored by trial lawyer interests, and three were sponsored by insurance industry interests. Proposition 100 was sponsored by the CTLA and received 40.9% of the vote. This proposition called for the reduction of "good driver" rates by 20% and the institution of health insurance rate regulation. It also allowed banks to sell insurance and allowed claimants to sue insurance companies for acting in "bad faith." Proposition 104, the no-fault initiative, was the insurance industry's most favored proposition (as evidenced by the fact that the insurance industry spent much more to promote this proposition than it did to promote the others). Despite this status, it was approved by only 25.4% of the voters. This proposition called for the establishment of a no-fault system of auto insurance (thus eliminating the need for many types of legal recourse in the event of an accident), the reduction of some premiums by 20% for two years, a restriction on future insurance regulation legislation, limitations on damage awards against insurance companies, limitations on attorney contingency fees, and the preservation of the insurance industry's antitrust status. Proposition 101 was sponsored primarily by one insurance company, received 13.3% of the vote, called for a temporary reduction of the bodily injury portion of insurance premiums and limited injury claims for pain and suffering and required that all other sources of compensation be exhausted before an insurance company was required to pay. Proposition 106 was sponsored by the insurance industry and received 46.9% of the vote. This measure placed limits on attorney contingency fees in tort cases.

Several characteristics of the insurance reform elections made them particularly appropriate for testing the effect of different types of information on voting behavior. Two of these characteristics led me to believe that most voters would not take the time or effort needed to become well informed about many of the issues relevant to the insurance reform debate. The first of these characteristics is that the text of the five insurance reform ballot initiatives was lengthy (totaling over 26,000 words) and technical. Second, insurance reform voters were likely to have been occupied with other political matters, since the November 1988 general election ballot in California was unusually long.⁷

Other characteristics led me to believe that there would also be a vigorous campaign. The first of these characteristics is that reform of the state's insurance regulation was—and still is—a highly salient issue. Each of the five initiatives corresponded to a significant change in the insurance regulatory framework and each had the potential to affect the price or expected value of many insurance policies, as well as insurance industry profits and trial-lawyer case loads. Second, the potential effect of different electoral outcomes on their profit margins led me to expect, a priori, that there would be high levels of campaign expenditure by the insurance industry and trial lawyers. (Over \$82 million was actually spent on the insurance reform campaigns.) Because the insurance reform elections had these four characteristics, I expected that there would be complex electoral decisions, many confused voters, a vigorous campaign, and a unique opportunity to examine how different types of information affect voter behavior.

Sources of Information

Insurance reform voters had access to different types of information than voters in candidate-based elections typically do. The most notable difference was the absence of two types of information that are widely believed to affect voting behavior in elections involving candidates. The first type of missing information was the *partisan cue*. Not one of the initiatives had a party label attached to it.⁸ This absence is particularly relevant because much of the contemporary understanding of voting behavior relies on the notion of voters taking cues from party labels.

The second familiar type of missing information was *the past*. Retrospective evaluations, of the type considered by Downs (1957), Key (1966), and Fiorina (1981) are thought to help voters simplify their evaluations of electoral alternatives and depend on the existence of relevant past histories. For instance, retrospective voting hypotheses tell us that voters can

evaluate a candidate's credibility and potential effectiveness by reviewing particular features of the candidate's past. While credibility is less of an issue for the content of a ballot proposition, an electorate's ability to evaluate the consequences of a particular proposition retrospectively could help it better understand the future consequences of the proposition's acceptance. Unfortunately for voters, no such history was available for the five insurance reform propositions, since none of the proposed laws had ever previously been enacted in California.

In addition to the absence of some common types of information, the insurance reform elections included two sources of information that are not generally available in other electoral environments. First, the state provided a *summary* of each initiative. The summaries for the five insurance reform initiatives appear in Appendix A and were available in many places, including on the ballot itself. The summaries were between 25 and 100 words long and were intended to help voters distinguish between the initiatives. While each summary provided some information about an initiative, certain characteristics of the summary limited its effectiveness. For instance, a summary's brevity often resulted in the omission of important components of a complex initiative.

A second source of state-provided information was the *California Ballot Pamphlet*. The pamphlet for the November 1988 election was a thick document that contained, for each proposition, the summary just described, one signed argument by proponents, one signed counterargument by opponents, one signed argument by opponents, one signed argument by proponents, and the actual text of the proposed changes to the law. The fact that voters could obtain a great deal of information from the California Ballot Pamphlet is not in question. However, whether voters might reasonably be expected to spend the time and effort required to learn from the pamphlet is questionable and undermines its potential effectiveness.

A final source of information was the campaign waged by the three affected interest groups: the insurance industry, the CTLA, and consumer activists. Together, these three groups spent more than \$82 million on the campaign. Table 1 details the names, preferences, and expenditures of all the campaign organizations that were involved in the insurance reform campaign and registered with the state board of elections. Each organization is classified by its primary source of funding. After reading through the campaign receipt and expenditure documents that were filed with the state, I can confidently assert that there did not exist an organization for which this classification was not obvious. (All contributors of \$25 or more must be listed by name and occupation in a California direct legislation campaign's contribution

A cursory inspection of Table 1 reveals more about the campaign than just dollars expended. All of the large groups claim to be consumer- or citizen-oriented. These claims are indicative of the campaign

CONTESTANT ORGANIZATION	RECOMMENDED VOTE	EXPENDITURE (\$)
Insurance Industry		The state of the s
Citizens for No Fault	yes on 104 and 106, no on 100 and 103	\$41,402,392
Californians against Unfair Rate Increases	no on 100 and 103	14,951,162
Consumers for Lower Auto Insurance Rates	yes on 101	5,401,934
Committee for Fair Lawyers Insurance Fees	yes on 106	2,523,599
American Insurance Association	yes on 104	730,575
Committee for Fair Auto Insurance Ratings	no on 100 and 103	39,323
Total	· ·	\$65,048,985
Trial lawyers		
Good Driver Initiative	yes on 103	\$13,786,653
No on 106	no on 106	624,449
Consumer Coalition against 106	no on 106	401,704
No on Proposition 106 Committee	no on 106	172,737
Consumer/Legal Equal Justice Committee	no on 106	20,000
Total		\$15,005,543
Consumer activists		
Voter Revolt for Lower Insurance Rates	yes on 103	\$ 1,932,902
Californians for Honest Insurance	no on 101,104, and 106	24,005
Friends of Motorcycling	yes on 100, no on 101, 104, and 106	22,500
Santa Cruz Committee for Consumer Justice	yes on 100, no on 104	6,117
		\$ 1,985,524
Total	Minister	\$82,040,052

strategies employed by the large information providers. In the advertisements purchased by the insurance industry and trial lawyers, the identity of the sponsor was as well hidden as the law would allow. Except for the small print at the bottom of print advertisements or the rapidly disappearing disclaimer that surfaced in broadcast media, the fact that a particular message was associated with the preferences of the insurance industry or trial lawyers was not mentioned. These strategies gave all of the paid advertisements the quality that groups supporting an initiative attempted to represent themselves, and their initiative, as "proconsumer," regardless of their source of financial support, while groups opposing an initiative represented the initiative they were campaigning against as "anticonsumer". So, unlike either scientists and engineers (Kuklinski, Metlay, and May 1982) or major political parties in candidatecentered elections, the insurance industry and trial lawyers did not want to be used as reference groups by the voters—a circumstance that further complicated the voters' ability to understand the likely effects of five complex propositions.

Shortcuts and Voter Inference

If insurance reform voters faced complex alternatives, were too busy or disinterested to acquire encyclopedic information, and could rely on neither the past nor partisan cues, how did they decide which propositions to vote for? To place this case study in a broader context and to motivate the empirical tests that follow, I briefly review some research that is

relevant in attempting to understand the behavior of insurance reform voters.

Particularly appropriate for this case study are the dynamics described in strategic models of communication, more commonly known as "signaling" games. In a signaling game, a relatively well informed information provider has information that is relevant to a relatively uninformed decision maker. The information provider can attempt to affect the decision maker's behavior by sending a "signal" about the consequences of the decision maker's actions. The inferences that the decision maker is able to draw from the content of the signal depend on prior beliefs about both the information provider's knowledge and the information provider's incentives for truth telling. Since the information providers in the insurance reform example also drafted the propositions, I proceed as though it were common knowledge that the information providers understood the content of the propositions and focus this review on the relationship between an information provider's credibility and voter inference.

When either the information provider or the content of the signal is known to be perfectly credible, several scholars have argued that voters can use the content of a signal to make more accurate inferences about the personal consequences of an electoral outcome. Calvert (1985), McKelvey and Ordeshook (1985, 1986), and Grofman and Norrander (1990) use spatial models of candidate-centered elections, while I (Lupia 1992) used a spatial model of direct legislation, to identify conditions under which the existence of perfectly credible signals are sufficient to allow in-

completely informed voters to emulate the behavior of better or completely informed voters. In these studies, uninformed voters tend to find it in their interests to cast the same vote as the information provider when they know that they and the information provider have similar preferences over outcomes. The same dynamic allows relatively uninformed voters to emulate the behavior they would exhibit if well informed by voting against the information provider's preferred alternative when they know that the information provider's interests are contrary to their own. Unfortunately, these arguments are of limited helpfulness when we attempt to understand voter decision making in circumstances where information providers are not perfectly credible and may, in fact, have an incentive to mislead

Sobel (1985) and Brady and Sniderman (1985) have presented arguments suggesting that the existence of a "reputation for honesty" or "likability" is sufficient to allow voters to condition their voting behavior on the content of a signal provided by an information provider who is not perfectly credible. Sobel uses a repeated-play signaling model to show that if an information provider (who is not initially assumed to be perfectly credible) can establish a reputation for some consistent quality, like honesty, then badly informed decision makers can make more accurate inferences about the options available to them from the content of a signal whose truthfulness cannot otherwise be easily established. Brady and Sniderman use the National Election Studies to show that voters in candidate-centered elections rely on their feelings toward information-providing groups better to understand the relationship between their own preferences and those of the candidate. 10

In the absence of perfectly credible signals, could insurance reform voters have used their knowledge or perceptions of a particular information provider's reputation to help them understand the personal consequences of a particular electoral outcome? The theories I have reviewed—and others like themsuggest that the answer is yes, but only if voter perceptions of an information provider's preferences (i.e., the information provider's reputation for supporting certain types of policy) were correct. If correct, perceived information-provider preferences are equivalent to the information providers having a known and informative reputation for supporting certain types of policy. If incorrect (which is quite possible in this case considering the insurance industry and trial-lawyer attempts to shield their identities), then perceived information-provider preferences will not be sufficient to allow badly informed voters to overcome their lack of encyclopedic knowledge.

For our purposes, it is unfortunate that evaluating the accuracy of insurance reform voter perceptions is quite difficult. Fortunately, we need not make this explicit evaluation to determine whether knowledge of a prominent information provider's preferences allowed otherwise uninformed voters to vote as though they were well informed. We can simply compare the behavior of voters who differ only in the amount and types of information they possess. With this type of comparison in mind, I developed and executed an exit poll of insurance reform voters. In an attempt to distinguish relatively informed voters from those who were relatively uninformed, I designed the survey to provide individual-level measures of the amount of encyclopedic information that a respondent possessed. To determine what impact, if any, information shortcuts had on voting behavior, I also asked respondents to report their perceptions of opinions that could have been used as shortcuts, relevant demographic questions, and their voting behavior.

My plan was to use these measures to compare the behavior of well-informed voters, relatively uninformed voters who knew a shortcut, and relatively uninformed voters who did not know that shortcut. Finding that the voting behavior of "relatively uninformed voters who knew a shortcut" was significantly different from the voting behavior of "relatively uninformed voters who did not know a shortcut" would lead me to reject the hypothesis that knowing the shortcut did not affect voting behavior. Finding that the voting behavior of "relatively uninformed voters who knew a shortcut" was also very similar to the voting behavior of "relatively informed voters" would provide support for the assertion that voters who appeared to possess low levels of encyclopedic knowledge used certain types of shortcuts to emulate the voting behavior they would have exhibited if they were relatively well informed. In contrast, finding that the voting behavior of "relatively uninformed voters who knew a shortcut" was very different from the voting behavior of "relatively informed voters" would suggest that knowing the shortcut was insufficient for voters to overcome their lack of encyclopedic information.

A SURVEY TO TEST FOR THE EFFECTS OF INFORMATION

The survey consists of exit interviews with 339 voters in Los Angeles County. Respondents were asked how they voted on the insurance reform propositions, socioeconomic and insurance-rate-related questions, and a series of questions designed to elicit the level of information (or confusion) that each respondent had on the issue of insurance reform. The information questions were designed to learn not only what respondents knew about the content of the insurance reform debate but also to gauge respondent beliefs about information-provider preferences. The information questions used in the analysis appear in Appendix B.

To execute the survey instrument, I recruited 30 undergraduates from the California Institute of Technology, California State University (Northridge), and Pasadena City College. The pollsters received extra

credit in political science and economics classes in exchange for attending two instructional sessions and administering the poll for four hours on Election Day. Lecturers at these institutions received a day off from lecturing in exchange for providing me with access to their students. In effect, I received low-cost pollsters in exchange for lecturing about the results of the exit poll in the students' classes. The entire survey was conducted for less than three hundred dollars.

In the instructional sessions, I stressed careful execution of the polling, as opposed to maximizing the number of respondents. I directed the pollsters to use a randomizing mechanism to select respondents in the hope that this would dampen any selection bias. Specifically, I instructed pollsters to choose every fifth exiting voter, with counting to begin only after an interview was completed. In addition, there was no reward to a pollster for the number of surveys completed.

The sample we obtained is neither large enough nor diverse enough to make broad generalizations about insurance reform voting behavior. Fortunately, making these generalizations was not our intention. Our purpose was to contact voters with varying amounts and kinds of information in an attempt to learn more about the effect of different types of information on voting behavior. Financial and physical limitations led me to restrict our polling universe to a ten-city area located in Los Angeles County and centered about the city of Pasadena. I chose polling places in areas across which there was a variance in the cost of auto insurance, the average number of accidents, and the cost of insurance claims.11 I reasoned that these variables would be correlated with insurance rates, interest in the insurance reform issue, and the propensity to obtain information. Given these constraints and goals, I was able to obtain a sample that included enough variation for me to explore the relationships I have described. Appendix C shows how the 339 respondents compared to the voters of Los Angeles County and the State of California.

For the purposes of analysis, I feel justified in assuming that our respondents were also consumers of insurance, since nearly all of our respondents (98.7%) reported living in a household where a car was insured. Correspondingly, I also assume that (1) respondent preferences over electoral outcomes do not vary systematically with the propensity to possess certain types of information, (2) the ability to determine the relationship between the proposition and one's own well-being does vary with the propensity to possess certain types of information, and (3) all respondents prefer electoral outcomes that result in either a lower premium or a higher expected value of compensation in the event of an accident. 12

Finally, each respondent was given a two-sided card at the beginning of the interview. One side of the card had a description of each insurance proposition exactly as it appeared on the official ballot (see Appendix A). We allowed respondents to see the summaries during the interview in order to minimize

the effects of variance in non-information-related recall ability. The other side of the card was used for asking potentially sensitive socioeconomic and insurance-rate-related questions. Instead of giving numbers for income, age, traffic violations, or insurance premiums, respondents selected from lettered categories that were listed on the card.

Empirical Model

I shall now use a series of multivariate logit regressions to identify the determinants of respondent voting behavior. I present one logit for each of the five propositions. In each logit, the dependent variable is the binary variable *Vote*. *Vote* equals 1 for a *yes* vote on the proposition and 0 for a *no* vote. Thus, positive coefficients for the independent (explanatory) variables imply that having the characteristic presumed to generate the independent variable is associated with a greater propensity to cast a *yes* vote. The model used in the estimation is as follows:

Vote = $\alpha + \beta_1$ INSURANCE + β_2 LAWYER + β_3 NADER

- + β_4 KNOWLEDGE + β_5 INSURANCE * KNOWLEDGE
- + β_6 NADER * KNOWLEDGE + β_7 LAWYER * KNOWLEDGE
- + $\beta_{8,...,11}$ [other insurance-reform-related variables].

The first three independent variables are measures of a respondent's possession of a certain type of information shortcut-knowledge of an informationprovider's preferences. Insurance equals 1 if the respondent correctly identifies the insurance industry preference on the proposition and 0 otherwise (correct responses are yes on 101, 104, and 106, no on 100 and 103). Similarly, LAWYER equals 1 only if the respondent correctly identifies trial-lawyer preferences (yes on 100, no on 106); and NADER equals 1 only if the respondent can identify Ralph Nader's preferences (yes on 103). As defined, the values of INSUR-ANCE, LAWYER and NADER are proposition-specific. For example, if a respondent could identify correctly the insurance industry's preference with respect to Propositions 100 and 104, then the value of INSUR-ANCE would be 1 for the Proposition 100 estimation, 1 for the Proposition 104 estimation, and 0 for the other three estimations for that respondent. Also notice that each of these variables is used only in estimations that correspond to campaigns with which the relevant information provider was directly involved (see Table 1).

To determine the extent to which a respondents' knowledge of these endorsements enhanced their ability to vote as though they were relatively well informed, I must first show that "knowing the shortcut" affected voting behavior. To test for this effect, I use the survey responses to evaluate the null hypothesis, namely, "The information shortcut has no effect on voting behavior (e.g., $H_0: \beta_4 = 0$). When the coefficient of INSURANCE (or LAWYER or NADER) is significantly different from zero, I can reject the null

hypothesis. 13 The ability to reject these hypotheses is a first indication that these information shortcuts affected insurance reform voting behavior.

After determining whether or not these shortcuts affected voting behavior, I then want to address the question that motivated this research project: To what extent did possession of a shortcut affect relatively uninformed respondents' ability to cast the same votes they would have cast if they had been well informed? The first step in addressing this question is to find a measure of a respondent's encyclopedic knowledge. The independent variable, KNOWLEDGE, is such a measure.

Knowledge is derived from responses to the four questions (listed in Appendix B) of the form, "Which of the propositions have *characteristic X?*" For each of the questions, there were five correct responses, since each proposition either had *characteristic X* or did not have it. Knowledge takes on its highest value, 20, only if respondents provided all correct responses, and its lowest value, 0, for respondents who provided no correct responses. ¹⁴ I assume that respondents for whom knowledge was high were more likely to understand the personal consequences of a particular proposition's approval than were respondents for whom knowledge was low. ¹⁵

If respondents prefer lower premiums and highervalued policies (as their status as consumers had led me to assume), then they should be likely to favor propositions that mandate lower insurance premiums (100, 103, 104) and increase competition in the provision of insurance (100, 103). For the same reasons, respondents should also be likely to oppose propositions that restrict a claimant's ability to collect from an insurance company (101, 104, 106). Since I expect that respondents with greater KNOWLEDGE would be relatively likely to understand which propositions were consistent with these interests, I predict that the sign of KNOWLEDGE will be positive for Propositions 100 and 103 and negative for Propositions 101 and 106. The sign of KNOWLEDGE for Proposition 104 depends on the respondents' willingness to trade off lower premiums for a decrease in the amount that they can expect to receive in the event of an accident. Since I possess neither a measure of, nor a compelling theory about, our respondents' willingness to make this trade, I cannot predict the sign of KNOWLEDGE for Proposition 104.

The second step in determining whether or not relatively uninformed voters could adapt to their lack of encyclopedic information is to determine the extent to which the shortcuts represented by the variables insurance, lawyer, and/or nader were effective substitutes for encyclopedic knowledge. Since I expected each of these shortcuts to be effective, I expected that the coefficients of insurance, lawyer, and/or nader would have the same sign as the coefficient of knowledge. If these coefficients are also significantly different from zero, then I have supporting evidence for the assertion that these shortcuts were an effective substitute for the acquisition of encyclopedic information. Given my previous predic-

tion of the signs of KNOWLEDGE, I expect positive and significant signs on the three information-provider coefficients for Propositions 100 and 103 and negative and significant signs on these coefficients for Propositions 101 and 106.

The theories reviewed earlier suggest that knowledge of an information-provider's preferences should have a greater effect on the inferences and voting behavior of relatively uninformed respondents (who have more to learn) than it does on the behavior of relatively well informed respondents. Therefore, I include interactive explanatory variables that condition the effect of the INSURANCE, LAWYER, and NADER variables on the respondent's level of KNOWLEDGE. Because relatively uninformed respondents have more to learn, I expect the signs of the interactive term coefficients to be different from the signs of its components. In addition, I expect the magnitude of the interactive term coefficients to be smaller than the coefficients of INSURANCE, LAWYER, and NADER, since even relatively well informed persons may be able to form more accurate inferences about proposition content from their acquisition of knowledge about the information-providers' preferences.

In each of the estimations, I also include measures of potential within-group variance in preferences over insurance reform initiatives to account for the possibility that some respondents may care more about the insurance reform issue than others and, therefore, may feel even more strongly about obtaining lower insurance premiums or higher-valued policies. These variables are described in Appendix D.

Results

Table 2 reports the logit results. Since I have predicted that many of the coefficients will have the same sign, I used a singular value decomposition to determine the existence of multicollinearity. This test (explained in Belsley, Kuh, and Welch 1980) failed to show significant levels of collinearity. ¹⁶

I first discuss the effect of the variable INSURANCE. To determine whether or not knowledge of insurance industry preferences was an effective shortcut, we must first determine whether or not INSURANCE affected voting behavior. My analysis suggests that respondents who knew the insurance industry's preferences on a particular proposition exhibited very different voting behavior from that of respondents who did not have this information. Table 2 shows that in three of the five elections, knowledge of the insurance industry's preferences was a statistically significant determinant of voting behavior. In the other two cases, conventional levels of significance were narrowly missed. To better understand how meaningful the INSURANCE coefficients are, consider the fact that the existence of 20 categories in the KNOWLEDGE variable make it relatively unlikely that any effects I capture with variables like INSURANCE are actually the result of within-group variance in the KNOWLEDGE categories. This formulation allows me to make a meaningful distinction between the effect of

TABLE 2						
Effects of Information an	d Data	Determinants	on.	Voting	Robovic	

INDEPENDENT	•		PROPOSITION		
VARIABLES	100	101	103	104	106
Constant	1.50	90	-3.95	57	.29
	(1.11)	(1.48)	(1.26)	(1.16)	(1.13)
KNOWLEDGE	.05	09*	.22**	06	07
	(.05)	(.05)	(.08)	(.05)	(.05)
INSURANCE	2.5 7**	-1.89	3.35**	1.15	-2.28**
	(.91)	(1.28)	(.90)	(.82)	(1.14)
INSURANCE * KNOWLEDGE	10	.41	20**	.04	.02
	(.07)	(1.19)	(.07)	(.07)	(.10)
LAWYER	-2.42** (1.04)	AMERICANIA	_		-1.25 (.86)
LAWYER * KNOWLEDGE	.15* (.08)	, someone			.04 (.07)
NADER		anticharph	.86 (.85)		
NADER * KNOWLEDGE	_	*****	09 (.07)		
Traffic violator	24	.09	.07	.22	.32
	(.37)	(.60)	(.36)	(.44)	(.43)
Male-under-25 violator	.06	-8.23	−.18	02	12
	(.68)	(42.25)	(.70)	(.72)	(.74)
CARTYPE	.22	.01	.21	32	16
	(.22)	(.34)	(.22)	(.27)	(.24)
Income	.05	.00	.32**	.14	.01
	(.15)	(.22)	(.14)	(.16)	(.15)
N	219	220	236	221	220
Begin-log likelihood	-151.8	-152.49	163.58	153.19	152.49
End-log likelihood	-135.55	-65.13	139.25	112.51	119.64
% correctly predicted	67	90		78	75

Note: The dependent variable is binary, equals 1 if the respondent reported voting for the proposition in question, and equals 0 if the respondent reported voting against the proposition in question. The observations used in this analysis include only those respondents who voted on the proposition in question and answered all of the relevant knowledge and demographic questions. The coefficients presented are unstandardized and the numbers in parentheses are standard errors. The coefficients and standard errors were derived using the logit macro in the SST program (Dubin/Rivers Research, Pasadena, California).

encyclopedic information on voting behavior and the effect of the shortcuts. Therefore, given the strength of the test and the relatively small sample size, the levels of significance in all five cases are quite remark-

Since logit coefficients do not provide much intuition about the absolute magnitude of the hypothesized relationships, I report relevant "first differences" in Table 3. A "first difference" is a straightforward translation of a logit coefficient into a percentage. The first differences I present are estimates of how much the probability of voting yes on the relevant proposition would change, given a particular change in a single independent variable (or set of independent variables) while holding the other independent variables constant at their mean values. For example, the first differences tell us that a respondent who was average in every way except that she provided no correct responses to the proposition content questions had a .17 higher probability of voting for Proposition 100 than did a respondent who was similarly average except for her ability to provide 20 correct responses.

An examination of Table 3 once again shows the relatively large effect that knowing the insurance industry's position had on respondent voting behavior. The lower part of Table 3 shows that whether they knew the insurance industry's preferred electoral outcome or not was the largest single determinant of low-knowledge respondents' voting behavior. Notice also that this effect decreased as KNOWLEDGE increased (because INSURANCE and IN-SURANCE * KNOWLEDGE always had different signs, as expected). For example, knowing that the insurance industry was against Proposition 100 increased by 48 percentage points the probability that a respondent who could provide no correct answers to proposition knowledge questions voted for Proposition 100. The

 $p \le .10$. $p \le .05$.

KNOWLEDGE = 15

TABLE 3							
First Differences: Change in t	he Probability of a "YES" Vote						
INDEPENDENT VARIABLE	CATEGORY CHANGE		PROPOSITION				
OR CONDITION	FROM	TO	100	101	103	104	106
KNOWLEDGE	lowest (0)	Highest (20)	17	-7	19	-5	-22
INSURANCE	Does not know Insurance perferences	Knows `	34	-7	26	-11	-34
LAWYER	Does not know Lawyer preferences	Knows	-17			_	-15
NADER	Does not know Nader preferences	Knows			-3		
Household Income	\$22,500 <u></u> \$40,000	over \$75,000	0	0	14	3	2
INSURANCE, if	F.						
KNOWLEDGE = 0	Does not know Insurance preferences	Knows	48	-18	68	-24	-46
KNOWLEDGE = 10	Does not know Insurance preferences	Knows	36	-8	32	-14	-35
KNOWLEDGE = 15	Does not know Insurance preferences	Knows	26	-5	8	-8	-30
LAWYER, if							
KNOWLEDGE = 0	Does not know Lawyer preferences	Knows	-40			_	-28
KNOWLEDGE = 10	Does not know Lawyer preferences	Knows	-21			_	-16

Note: A concise explanation of when and how this and related methods can make limited-dependent-variable-model coefficients more intuitive is provided in King 1989 chap. 4. The first differences of KNOWLEDGE, INSURANCE, NADER, and LAWYER and their interactive terms are computed using the appropriate conditional means. For instance, the change in the dependent variable due to knowledge of the insurance industry's preferences is determined by increasing INSURANCE from 0 to 1 and INSURANCE * KNOWLEDGE from 0 to the mean of KNOWLEDGE.

Knows

Does not know Lawyer preferences

magnitude of this effect was roughly double that of a respondent who could provide correct responses on 15 of 20 occasions.

I now use the data to evaluate the extent to which use of the INFORMATION shortcut allowed low-KNOWLEDGE respondents to emulate the behavior of high-KNOWLEDGE respondents. Tables 2 and 3 provide evidence for the prediction that relatively uninformed respondents used their knowledge of the insurance industry's preferences to emulate the behavior of relatively well informed respondents. In every logit, the sign of INSURANCE is the same as the sign of KNOWLEDGE. This implies that whether respondents used encyclopedic information or their knowledge of insurance industry preferences, they formed the same types of inferences about the relationship between a proposition and their own well-being.

To show the effect of the INSURANCE shortcut in a more concrete manner, in Table 4 I divide the sample into three mutually exclusive categories based on the

respondents' values of KNOWLEDGE and INSURANCE. Presented in Table 4, column 1, is the behavior of respondents who had relatively high KNOWLEDGE and who knew the insurance industry's preferences. Table 4, column 2, contains the behavior of respondents who had relatively low KNOWLEDGE, but (like the respondents in column 1) knew the insurance industry's preference. In column 3 are respondents who also had relatively low knowledge but (unlike the second group) did not know the insurance industry's preferences. A comparison of the behavior of these three groups makes my main point in a dramatic fashion. Voters in the first two columns look very much like each other, while voters in column 3 look very different from voters in the first two columns. The relationship between these three groups strongly supports the assertion that knowledge of the insurance industry's preferences provided an effective shortcut for voters whose (encyclopedic) KNOWLEDGE of proposition content was low.

Percent Voting	Yes by Information Category		
		INFORMATION CATEGORY	
PROPOSITION	HIGH KNOWLEDGE (10–20) WITH KNOWLEDGE OF INSURANCE INDUSTRY PREFERENCES	LOW KNOWLEDGE (0-9) WITH KNOWLEDGE OF INSURANCE INDUSTRY PREFERENCES	LOW KNOWLEDGE (0-9) WITHOUT KNOWLEDGE OI INSURANCE INDUSTRY PREFERENCES
100	53	53	27
101	8	5	15
103	72.5	73	26
104	17	17	34
106	11	12.5	45

It is also interesting to note that the coefficients of INSURANCE (and KNOWLEDGE) are negative for the three propositions that insurance industry favored (101, 104, and 106) and positive for the two propositions that the insurance industry opposed (100 and 103). In all cases, the signs of KNOWLEDGE and INSUR-ANCE are as predicted. The similarity of the two sets of coefficients supports the notion that relatively informed respondents concluded—and relatively uninformed respondents perceived—that the insurance industry's preferences over electoral outcomes were contrary to their own. 18 This finding reflects well on the insurance industry's attempt to hide its identity, in that respondents who were able to identify its preferences tended to use them as a negative reference. Given the closeness of the vote on Proposition 103 (only 11 out of every 1,000 voters would have had to cast a different vote to change the outcome), we can conjecture that a slightly more successful attempt to detach the insurance industry's identity from their endorsements may have lead to a different electoral

For voting behavior on Proposition 100, the effect of the variable LAWYER is similar to the effect of INSURANCE but less dramatic. The coefficient for LAW-YER is significant at the .05 level in the Proposition 100 logit. The negative, significant, and correctly predicted sign of LAWYER in the Proposition 100 logit is consistent with the assertion that respondents perceived the trial lawyers' preferences to be different from their own. However, the magnitude of this coefficient suggests that this shortcut did not affect voter inferences as much knowledge of the insurance industry's preferences did. In contrast, the negative coefficient of LAWYER in the Proposition 106 logit is of a different sign from that of either KNOWLEDGE or INSURANCE and thus contrary to my prediction. These findings suggest that knowledge of the LAWYER endorsement was not as effective a shortcut as was knowledge of the insurance endorsement. Unfortunately, I do not have sufficient data to determine whether this signal was less effective because the lawyers waged a smaller campaign than the insurance industry (less costly and observable effort) or because respondents were relatively uncertain about lawyer views on the value of an insurance policy (lack of reputation).19

Finally, the relatively small coefficient on the NADER variable may surprise those who recall Nader's role in this election. The interpretation that Ralph Nader had no effect on voting behavior, however, would be going a bit too far. Unlike Voter Revolt (the sponsors of Proposition 103), Ralph Nader had a relatively well known past. I believe that Nader's past and his widely covered and frequent speeches about the insurance industry's big money campaign against 103 were Voter Revolt's greatest asset. With Nader, Voter Revolt was able to prevent the insurance industry from monopolizing the transmission of widely accessible (i.e., mass-media) information. Nader's presence certainly made it easier for some voters to locate the insurance industry's preferences over out-

comes. Without someone like Nader, it is unlikely that Voter Revolt could have either received the media attention it did or acquired credibility sufficient to affect voter opinions for those occasions when it did have access to the media.

CONCLUSION

I used a survey instrument to identify the effect of different types of information on voting behavior in the complex insurance reform initiatives of 1988. I showed that respondents who possessed relatively low levels of factual (or encyclopedic) knowledge about the initiatives used their knowledge of insurance industry preferences to emulate the behavior of those respondents who had relatively high levels of factual knowledge. If we believe that well-informed voters make the best possible decisions, then the fact that relatively uninformed voters can emulate them suggests that the availability of certain types of information cues allows voters to use their limited resources efficiently while influencing electoral outcomes in ways that they would have if they had taken the time and effort necessary to acquire encyclopedic information. In closing, I believe that the policy implications of this analysis are quite straightforward: while scholars and pundits propose that we educate the public about politics in order to lessen the impact of uninformed votes on the responsiveness of democratic decision-making institutions, a deeper understanding of how people adapt to the uncertainty that characterizes many of their important decisions suggests that directing our efforts into the provision of credible and widely accessible "signals" may be a more effective and cost-efficient way to ensure the responsiveness of electoral outcomes to the electorate's preferences.

APPENDIX A: SUMMARIES OF INSURANCE REFORM PROPOSITIONS

Each of the following summaries are presented here exactly as they appeared on the ballot, in the California Ballot Pamphlet, and on the card given to each respondent during the inter-

100 INSURANCE RATES, REGULATION, INITIATIVE. Reduces good driver rates. Requires automobile, other property/casualty, health insurance rate approval. Adopts anti-price-fixing, antidiscrimination laws.

101 AUTOMOBILE ACCIDENT CLAIMS AND INSUR-ANCE RATES. INITIATIVE. Reduces automobile insurance rates, limits compensation for non-economic losses for four years.

103 INSURANCE RATES, REGULATION, COMMISSIONER, INITIATIVE. Reduces auto, other property/casualty rates. Requires elected Insurance Commissioner's approval of rates. Prohibits price-fixing, discrimination.

104 AUTOMOBILE AND OTHER INSURANCE. INITIA-TIVE. Establishes no-fault insurance for automobile accidents. Reduces rates for two years. Restricts future regulation.

TABLE B-1						
Information Questions						
			RESPO	ONSES		
QUESTIONS	100	101	103	104	106	DK
About information provider preferences	-					
Which of the propositions do you believe were supported by the <i>insurance industry?</i>	65	129*	39	171*	93*	61
Which of the propositions do you believe were supported by the <i>trial lawyers?</i>	78*	35	26	27	58	119
Which of the propositions do you believe were supported by Ralph Nader?	15	16	188*	15	10	71
About Proposition Content						
Which proposition(s) establish a <i>no-fault</i> system of auto insurance?	21	19	17	191*	8	54
Which proposition(s) mandate giving discounts in insurance premiums to good drivers?	117*	35	65*	16	4	52
Which propostion(s) limit attorney contingency fees?	7	13*	14	22*	193*	59
Which proposition(s) mandate insurance rates which are not based on where you live?	56	19	88*	16	7	140

106 ATTORNEY FEES LIMIT FOR TORT CLAIMS. INITIATIVE. Limits amount of contingency fees which an attorney may collect in tort cases.

APPENDIX B: INFORMATION ABOUT INFORMATION MEASURES

The starred cell entries in Table B-1 identify correct responses. Respondents were permitted to give multiple responses to each of the following questions. Notice that for some questions there were multiple correct responses.

APPENDIX C: ELECTION AND POLL RESULTS

The cell entries in Table C-1 are percentages voting in favor of the proposition. The source of state and county level election results is the Statement of Vote provided by the California Secretary of State, Sacramento.

APPENDIX D: OTHER INDEPENDENT VARIABLES

VIOLATOR is a binary variable that equals 1 if the respondent admitted to a traffic violation in the last three years. Since violators face higher premiums than nonviolators, the out

come of insurance reform elections are likely to have a greater effect on violators than nonviolators. Mv25 equals 1 if the respondent was a male "violator" under 25 years of age, and 0 otherwise. As a member of this group at the time I designed the exit poll, I knew that male respondents who were under age 25 would pay higher premiums than other violators. Therefore, the outcome of insurance reform elections should have had a greater effect on these respondents than on other violators.

The variable Cartype is a discrete variable with values 1–5. As cartype increases, so does the retail price of the particular model. Since insurance rates are correlated with the price of the respondent's car, I expect that (all else held constant) respondents having higher-priced cars will be more affected by the electoral outcome than will respondents with lower-priced cars. Income is a discrete variable that is valued 1–5 and increases with household income. Cutoff levels for the discrete categories were \$15,000, \$22,500, \$40,000, and \$75,000. Insurance reform should have a relatively large effect on persons with higher incomes, who tend to purchase more insurance.

As interesting as these concepts are, the variables described in this appendix do not produce significant coefficients. In fact, only two out of these variables' 20 coefficients have coefficients that are larger than their standard errors. While the inclusion (or exclusion) of these variables does not alter the results I have presented, I include them because I expect many readers to question the viability of a statistical model that tests for the effects of different types of information by including only independent variables that are measures of information.

TABLE C-1	<u> </u>						
Comparison to County and State Vote							
PROPOSITION	SPONSOR	STATE	LA COUNTY	SURVEY (N = 339)			
100 (good driver initiative)	Trial lawyers	40.9	50.4	44.3			
101 (Polanco initiative)	Insurance industry	13.3	15.5	10.8			
103 (Voter Revolt/Nader initiative)	Consumer activists	51.1	62.6	65.2			
104 (no-fault initiative)	Insurance industry	25.4	22.3	21.5			
106 (contingency fee initiative)	Insurance industry	46.9	43.6	29.4			

Notes

I wish to thank Richard McKelvey and Peter Ordeshook for guidance and support through all stages of this project. Helpful comments and suggestions were offered by Elisabeth Gerber, Mat McCubbins, Chris Achen, Henry Brady, Bruce Cain, Gary Cox, Jonathan Katz, Rod Kiewiet, Sharyn O'Halloran, Sam Popkin, Larry Rothenberg, Roger Solin, Jim Stimson, and numerous seminar participants. I am in the debt of Phyllis Pugh, Joy Hansen, Shawn Kantor, Ramy Farid, Susan Davis, and 30 enthusiastic pollsters whose participation and/or cooperation were necessary conditions for the successful execution of the poll. Financial support for this project was supplied by the Division of Humanities and Social Sciences at the California Institute of Technology.

1. For a review of these arguments as they apply to direct legislation elections, see Cronin 1989.

2. For examples of this type of argument as they apply to direct legislation, see, e.g., Kuklinski, Metlay, and May 1982 and Lupia 1992. Popkin 1991 provides a more general review of the consequences of voter information problems.

3. For example, the most comprehensive analysis of proposition complexity that I am aware of (Magleby 1984) shows that the average number of words in the propositions that appeared on the California ballot during the 1970s was 8,121. He goes on to argue that over 17 years of formal education would be required to comprehend the text of the average proposition.

4. Theoretical analyses are developed in Lupia 1992, 1993a. For experimental tests of the theory, see Lupia 1993b.

- 5. I have reviewed the votes of insurance-related bills in the three years preceding the election and have found two systematic relationships: insurance-industry-supported tort reform bills die in the Assembly Judiciary Committee, while lawyer-supported insurance reform bills die in either the Assembly Finance and Insurance Committee or the Ways and Means Committee. Republicans (the minority party in both houses of the California legislature) tended to support tort reform measures. Democrats on these committees tended to support bills more favorable to the trial lawyers, yet each of these measures tended to fall one or two votes short of passage in committee through the defection of two to four Democrats.
- 6. In October 1987, an insurance industry spokesperson in the course of a conversation about a consumer-activist-supported insurance reform ballot proposition estimated, "We could spend \$10 million and still not be assured of beating it" (from Reich 1987b). One wonders how the negotiations might have changed had the insurance industry and trial lawyers anticipated that campaign expenditures would total in excess of \$80 million and that a consumer-sponsored initiative would be the only resulting change to the status quo.
- 7. In addition to nonpolitical concerns, there were elections for the office of president and U.S. Senator; seats in Congress, the state senate, and the state assembly; local ballot measures; and 22 other statewide measures on the same ballot.
- 8. Some prominent figures of both parties took public stands on the insurance reform issue, but these endorsements provide no evidence of a clear partisan consensus. See Campbell et al. 1960 for a definitive statement of the effect of partisan cues.
- 9. For well-known examples of this type of research, see Crawford and Sobel 1982; Milgrom and Roberts 1986; Spence 1973.
- 10. Related research on "cue taking" merits attention. Kuklinski, Metlay, and May (1982) showed that voters in nuclear power referenda conditioned their strategies on the stated positions of reference groups. Feldman and Conover (1983) identified emotional bases for certain types of voter responses to new political information. Popkin (1991) argued that certain images allow voters to simplify their voting calculus. Page and Shapiro (1992) examine responses to over

one thousand survey questions to support their argument that electorates have somewhat parallel adaptive capabilities.

- 11. The sources used to make these evaluations included California, Department of Insurance 1988a; California, Department of Insurance, Consumer Affairs Division 1987; and California, Department of Insurance, Statistical Services Unit 1988a-c.
- 12. This is not to say that respondents knew, initially, which propositions would achieve this goal. If they had, the campaign would have been meaningless. In addition, two respondents in the sample admitted to working for insurance agencies. Both voted in a manner consistent with a "job security" hypothesis. Otherwise, there was nothing in the data suggesting that this voter preference hypothesis did not represent respondent preferences.
- 13. As it is important not only to provide support for one causal hypothesis but also to reject other plausible causal hypotheses, I reran each of the estimations in Table 2 four times, replacing INSURANCE with an equivalently defined binary variable that measured a voter's ability to answer one of the four questions about proposition content. For instance, the first of the four questions that links a proposition to an issue is "Which proposition(s) establish a no-fault system of auto insurance?" (see Appendix B) Responses to this question were used to produce five binary variables that equaled 1 when the respondent correctly identified the relationship between the statement and the proposition and 0 otherwise. In 19 of the 20 cases, the replacement variable performed very badly. The T-statistic for the replacement variables were usually below 1 and often below .5. In addition, the difference between the initial likelihood function and the final likelihood function was generally much smaller than the equivalent estimations using INSURANCE. The one exception to this rule was the respondent's ability to correctly identify Proposition 104 as setting up a no-fault system of insurance. As Proposition 104 was referred to as the no-fault initiative, the effect of this particular variable in the 104 logit is hardly surprising. I reran the 104 logit once more, using INSURANCE, the no-fault replacement variable, and the corresponding interaction terms to find that the coefficient of neither variable was affected by the simultaneous inclusion of the other.

I also reran each estimation adding the respondent's reported partisanship. Implemented a number of theoretically consistent ways, the effect of party never attained conventional levels of significance and was omitted to preserve degrees of freedom. This lack of effect was especially true of PARTY's interaction with NADER, where the use of a party term seems most appropriate. In this case, the standard error was 30 times greater than the coefficient.

Since some readers may still be concerned about the absence of a party term in this analysis, an additional history of these elections may be insightful. Recall that insurance reform was a very touchy issue, one that the revealed behavior of California legislators suggests that few were willing to take public positions on. Despite repeated attempts over the five years preceding the November 1988 election, neither house of the California legislature could get an insurance reform bill out of committee and onto the floor for a vote. This inability to get out of committee is especially surprising, given the relatively strong Democratic control of both chambers. If Democrats had reached some type of consensus on how to reform the insurance industry, it would have been easy for them to use their control of the legislature to produce a bill for the governor to sign. In this case, "Democrat" might have served as an effective cue. However, the state legislature never achieved this consensus.

Likewise, voters who followed the actions of Republican governor George Deukmejian may have been confused about either his or his party's stand on insurance reform. Prior to the passage of Proposition 103, the insurance commissioner was appointed by the governor. Deukmejian's appointee, Roxani Gillespie, was previously a lobbyist for a large insurance company. This action might lead voters to believe that the Republican position was proinsurance. However, Deukmejian also encouraged voters to vote against all five proposi-

tions, including the three drafted by the insurance industry. That the governor and the legislature never once publicly locked horns on this issue, that the issue had to be decided using the initiative, and that during the campaign season the few partisans who did take stands differed in their stances is suggestive of the lack of observable partisan consensus on the insurance reform issue. Therefore, we should not be surprised that party was not an effective cue for relatively uninformed voters.

- 14. The distribution of respondents achieving each knowledge score (from 0 to 20) was 14, 1, 3, 11, 4, 7, 2, 25, 15, 7, 26, 16, 43, 26, 10, 35, 16, 33, 7, 2, and 0.
- 15. The arguments underlying this type of assumption are evaluated in greater detail by Bartels (1990) and Gerber and Lupia (1993).
- 16. The values of the singular value decomposition for each of the logits, ordered by proposition number, from lowest to highest, were 23.79, 20.48, 30.59, 22.14, and 23.37. Values of over 100 indicate collinearity, values from 30 to 100 indicate mild collinearity, and values under 10 indicate no collinearity. While the Proposition 103 logit is just over the threshold for mild collinearity, I reran it, collapsing several KNOWLEDGE categories and achieved very similar results to those presented, with less collinearity.

17. In case of the 101 and 104 logits, it is worth noting that (1) the dependent variable had relatively little variance and (2) this variable did achieve conventional levels of significance when I used weaker tests of the effect of INSURANCE. Of the respondents who voted on the relevant proposition and answered all of the information and demographic questions, only 22 of 220 voted yes on 101 and only 49 of 221 voted yes on 104. Thus, by attempting to make inferences about voting behavior that rely heavily on the observable differences between these yes-voters and the rest of the sample, we are asking a lot of the data. When we combine the limited variance of the dependent with our relatively small sample size, the fact that our coefficients even come close to conventional levels of significance (both are about one-and-a-half times the size of the standard error) is encouraging. Another part of the failure to achieve conventional levels of significance in the two extreme cases may be that I set up an empirical test in which rejection of the null hypothesis was quite difficult. The advantage of the difficult test is that I am more confident that the magnitude of the INSURANCE coefficient represents the effect of that particular shortcut. The drawback is that if processes that generate INSURANCE and KNOWLEDGE are correlated, this makes the likelihood of a large T-statistic relatively small. I reran the estimations replacing the KNOWLEDGE variable with limited measures of encyclopedic information that had two or 3 categories (low knowledge, somewhat knowledgeable, high knowledge), instead of 20. I found that all of the INSURANCE coefficients achieve conventional levels of significance in these reestimations.

18. Notice that for me to conclude that INSURANCE served as an effective shortcut, it matters not whether low-knowledge respondents knew that they were economizing on their information costs by relying on the insurance industry preference cue or whether they just did not like insurance companies. For my conclusion to be valid, it is sufficient that use of the shortcut allowed relatively uninformed respondents to emulate the behavior of relatively well informed respondents who were otherwise like themselves (my estimate of how these respondents would have voted if better informed).

19. The insurance industry was able to stifle early triallawyer attempts to advance their preferences by running a series of advertisements pointing out the lawyer sponsorship of certain advertisements. This particular series of advertisements proved so effective that not only were lawyer-sponsored campaign advertisements removed from the airwaves in the final weeks of the campaign, but so were noncampaign related advertisements for trial-lawyer services, at the request of CTLA (Reich 1988b).

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FISCAL CONSTRAINTS AND ELECTORAL MANIPULATION IN AMERICAN SOCIAL WELFARE

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merican social welfare policy involves a politically significant fiscal interaction between short-run, pay-as-you-go constraints and long-run equilibrium constraints motivated by the contributory principle and the concept of actuarial soundness. The fiscal constraints induce conflict between benefit recipients and payroll-tax-payers. To support small election-year reductions in payroll taxes, means-tested program benefits are slightly reduced in election years (compared to non-election-year levels under similar economic conditions). The existence of the constraints and of the election-year changes is tested using a multivariate time series regression model of monthly transfer payments and contributions for social insurance during years 1948–87. Short-run dynamics are found to be weakly incrementalist, but otherwise the results support the argument. Extraordinary manipulations are identified during 1972. Special technical features of the econometric analysis are a nonlinear, dynamic specification; robust generalized method of moments estimation; and near cointegration.

The taxing side of American social welfare policy is as important as the array of payments; that is, who pays is as important, politically, as who gains (see Browning 1986; Page 1983; Pechman 1985; Reynolds and Smolensky 1977). Ever since its beginning in 1935, the contributory design of American social insurance has forged strong links between benefits received by individuals and taxes they have paid (Achenbaum 1986; Bernstein and Bernstein 1988; Derthick 1979). One effect of this design has been a widespread feeling that in particular, each individual personally deserves the Social Security benefit payments that he or she ultimately receives: many believe that the benefits each person gets are exactly the benefits personally paid in throughout the working life. The view that social insurance benefits such as Social Security are earned awards has helped drive the escalation of payroll taxes needed to cover the costs of rising benefit levels in what has long been an unfunded, pay-as-you-go system (Bernstein and Bernstein 1988; Brittain 1972; Munnell 1977; Munnell and Blais 1984). This political consequence of the contributory design reflects Roosevelt's original conception, as epitomized by his remark, "We put those payroll contributions there so as to give the contributors a legal, moral, and political right to collect their pensions and unemployment benefits. With those taxes in there, no damn politician can ever scrap my social security program" (Schlesinger 1958, 309).

I argue that American social welfare's contributory design has indeed produced important political effects. Long-run constraints between aggregate taxes and aggregate benefits have been used to implement the design. The long-run political viability of the social welfare system depends on the relationship between the long-run constraints and the short-run constraints of pay-as-you-go. In the absence of effective long-run constraints, it is difficult to see how the opposing interests of taxpayers and benefit recipients

might be reconciled. But even effective constraints will leave some tension between payers and recipients. Free-riding incentives will always motivate demands for small, personally favorable adjustments. Each recipient will want a small increase in his or her benefits, while each taxpayer will want a small decrease in his or her taxes. In conjunction with differences among groups in political participation, the residual tension makes manipulation tied to the electoral calendar virtually inevitable. Incumbents have a strong incentive to take from recipients of benefits from means-tested programs, in election years, in order to give to groups more likely to reward them with votes.

I shall present this argument in greater detail, then describe and report the results from a multivariate time series regression model used to test the key assumptions and predictions of the argument. The dependent variables are the monthly levels of transfer payments and personal and employer contributions for social insurance during years 1948–87. The explanatory variables include several social and economic variables and several variables that measure political circumstances during the period.

Taxes and Benefits

By itself, pay-as-you-go implies that current payroll-tax-payers and current recipients of social insurance benefits have opposing interests. Pay-as-you-go means that current benefits are paid out of current taxes. Any increase to current recipients implies an extra cost to current taxpayers, while taxes can be cut only if benefits are reduced. Taxpayers and benefit recipients should therefore have opposing policy preferences if individuals act selfishly to maximize disposable income. Payers should want lower benefits, to allow their tax payments to fall, while recipi-

ents should want taxes raised, to support increases in their benefits.

The calculation of gains and costs is not so simple when the contributory principle is taken into account. According to the principle, a recipient's current level of benefits depends on the amount he or she paid in taxes when working. The individual-level contributory principle motivates an aggregate policy often described as actuarial soundness (Altmeyer 1966): current tax collections should promote economic growth sufficient to support the taxation that will be needed to pay benefits over the long haul. One way to do this is to use tax collections to generate aggregate public savings, that is, to collect payroll taxes in excess of the amount needed to meet current expenses (Munnell and Blais 1984). If such a system has been working, a policy of collecting higher taxes in the past should have produced higher levels of real income now (see Auerbach and Kotlikoff 1987, 158-161). This increased income can be used to support higher levels of current benefits. Under pay-asyou-go, the benefits will be financed by higher current taxes.

In political terms, pay-as-you-go and the contributory principle are mutually reinforcing. Under payas-you-go, current benefit recipients can recoup past increases in their own taxes (increases in the taxes they paid when they were working) only if the costs to current tax payers increase. But if earlier public savings promoted economic growth, then part of the income currently being taxed was produced by current recipients' previous tax payments. Such a system of effective investment would support the recipients' current claims. Moral rights such as Roosevelt cited might be invoked. The political connotations of such an arrangement should affect current taxpayers' longrun cost calculations: if they deny current recipients' claims, the legitimacy of their own future claims will be undercut. Such contingencies should reduce the net cost of current tax payments. But aggregate public savings are created only if current tax collections exceed current benefit payments. So if taxpayers are currently committed to pay-as-you-go in order to justify their own future claims, then they should also be willing to pay taxes in excess of the amount needed to cover current benefit claims. Indeed (and this is the key point), they should be committed to pay-as-you-go only if they are willing to pay such excess amounts. For if the actuarial-soundness concept holds, the increased income needed to support their own future claims will exist only if such amounts are paid. But if there will be no benefits in the future, then, in a purely selfish calculus, there is no reason to support pay-as-you-go now.

Such a synergy between the short-run policy of pay-as-you-go and the long-run investment policy associated with the contributory principle might go a long way toward dissolving the opposition between taxpayers' and benefit recipients' interests. If successful, a long-run aggregate savings policy can give selfish tax payers the long-run rate of return they need to view their tax payments favorably (see Boskin

and Puffert 1987). Current benefit recipients can be expected to claim the amounts they need to recoup their own earlier payments. But if taxes and benefits are linked in a long-run equilibrium relationship, then recipients should keep their claims low enough that current taxpayers can make the excess payments necessary to bind them to the system's future. Such an equilibrium relationship would exist as an equilibrium path along which taxes and benefits covary (see Auerbach and Kotlikoff 1987; Kamien and Schwartz 1991, 272–76.²

The conditions necessary for a long-run equilibrium to exist suggest restrictions on the forms a short-run policy of pay-as-you-go might take. A longrun equilibrium between taxes and benefits will be robust against stationary short-term shocks, but in general, long-run equilibrium cannot be sustained if either taxes or benefits are affected by arbitrary trends. Several social and economic conditions that affect social welfare tax or benefit levels are clearly nonstationary, however. Two examples are the population-age profile (in particular, the number of elderly and the number of children) and the price level as measured by the Consumer Price Index (Browning 1986). Political factors that affect social welfare policy, such as the party balance in the U.S. House of Representatives also exhibit trends (Berry and Lowery 1987; Browning 1985). A rulelike system of payas-you-go that automatically enforced strong shortrun constraints between tax and benefit levels would magnify the effects of such trends. Long-run equilibrium would then be extremely difficult to maintain or, most likely, impossible.

A discretionary approach to short-run policy might better support long-run goals. The best policy, for this purpose, would be one that prevented uncontrolled trends from having persistent effects on tax or benefit levels. Weak forms of incrementalism might allow this, but a strong form such as Browning (1985) estimates for social welfare expenditures would not. Discretion would mean that the political viability of pay-as-you-go depends entirely on the policy's current political and administrative support.

Short-run Manipulation

Long-run considerations might imply substantial convergence between taxpayers' and benefit recipients' interests. But conflicts between the two groups should nonetheless remain. Even if there were consensus on a core policy of pay-as-you-go and broad agreement on the aggregate amounts of benefits and savings that should be generated, free-rider considerations imply that individuals will not agree about the shares of the totals they are to receive or pay (see Laffont 1988, 112–24). At the margin, each current beneficiary will want to receive more, while each current taxpayer will want to pay less.

In such a situation elected officials should be able to manipulate the system at the margins to serve their immediate electoral needs even if their manipulations will be subject to important constraints. Of course, every incumbent will want to give everyone what they want—more benefits for each recipient and lower taxes for each taxpayer. But if pay-as-you-go is actually in force, this will not be possible. That is, if benefits can be increased only if there is a concurrent increase in the taxes to pay for them or if conversely a decrease in taxes can be obtained only if benefits are concurrently reduced, then officials will be forced to choose. Marginal improvements can go to either taxpayers or benefit recipients but not both. The fiscal constraint will have created constituencies with conflicting interests (see Buchanan 1967; Chappell and Keech 1986; Lowi 1964; Wittman 1983).

The usual assumptions suggest that incumbents will favor those likely to return them the greatest gain in votes. Differences among social welfare program benefit recipients should therefore mean that incumbents treat different groups of recipients differently. The primary difference should be between meanstested and non-means-tested program recipients (see Munnell 1987). By definition, means-tested program recipients are poorer in income. They also typically have less education. Partly for these reasons, meanstested recipients not only are less likely to vote but, more generally, are less likely to use political participation to communicate their needs to public officials (Verba et al. 1993). Recipients from "condition-tested" programs, such as unemployment insurance, are also less likely to vote (Rosenstone 1982; see also Schlozman and Verba 1979, 235-54). So we should expect incumbents to choose against the interests of such recipients, more than against nontested program beneficiaries.

The choice between payroll-tax-payers and social welfare program benefit recipients is therefore better thought of as a choice among taxpayers, tested-program recipients and non-tested-program recipients. Fiscal conflict should exist between payroll-tax-payers and tested-program recipients not only because some tested programs have contributory designs (e.g., unemployment insurance) but also because (formal accounting rules notwithstanding) Social Security has historically been imperfectly separated from the rest of the federal budget (Munnell 1985). So the question which group incumbents will favor most and which least turns on the group members' relative probabilities of participating politically.

It is easy to anticipate that tested-program recipients will be least favored. Payroll-tax-payers are, by definition, working people. Payroll taxes affect the entire middle class. They therefore touch the largest group of active participants. Incumbents will not rank this group behind poor, tested-program recipients who rarely participate. The choice between taxpayers and non-tested-program recipients is not clear. These groups mostly occupy the same kinds of occupational and career ranks, except that members of the recipient group are typically older (usually retirees) and, because of their ages, more wealthy. Such recipients are active participants (Verba et al. 1993). On the other hand, payroll-tax-payers are much more nu-

merous. General considerations such as these do not suggest that incumbents will see a particular priority ordering between the two groups.

We should therefore expect incumbents to seek ways to shift benefits away from the group of tested-program recipients, in order to increase non-tested-program benefits or decrease payroll taxes.

If voters weigh recent gains and losses more heavily than those further in the past and if a stable equilibrium path exists, then these shifts should take the form of regular oscillations around the levels of benefits and taxes specified in the current equilibrium policy. The shifts are prompted in the first place by group members' desires to cheat-in free-rider fashion—on the allocation of shares specified in current policy. By definition, permanent shifts would mean the policy had changed. Free-rider incentives would then exist relative to the new policy. If there is no stable equilibrium, then the free-rider-driven shocks might cause policy to wander to an arbitrary extent, in arbitrary directions. But if there is a stable equilibrium, shifts in one direction must eventually be matched by equally large shifts in the opposite direction. So if there is a stable equilibrium, the shifts among groups should fluctuate. Regular oscillations are expected because of the periodicity of elections. Incumbents will want the shifts in favor of likely voters to occur during election years.3

Such a pattern of manipulation differs from the one described by Tufte (1978, 28-44). Tufte highlights a pattern of election-year increases in social welfare benefits, with payroll tax increases being delayed until nonelection years. On the benefit side, Tufte focuses on nontested programs. He presents informal evidence of election-year increases. Some aspects of these findings have been corroborated by other, more systematic studies, while some aspects have been called into question (Frey and Schneider 1978; Keech and Pak 1989). Tufte uses the idea that incumbents can completely delay the costs for preelection benefit increases until after the election (Tufte 1978, 33). In the interim between the preelection delivery of extra benefits and the postelection collection of the tax bill, Tufte suggests that everyone is enjoying free money—that incumbents have arranged to have no one pay any costs at all.⁵ The ultimate expression of this view is Tufte's kyphosis, according to which extra benefits are supposed to be heaped up in the month or two immediately preceding an election.

My argument differs from Tufte's mainly in that I have tried to characterize more explicitly the fiscal context in which electorally motivated manipulations of social welfare taxes and benefits might be occurring. In my view, that context involves not only contemporaneous trade-offs among fiscally defined constituencies but also long-run fiscal relationships motivated by the contributory principle and the concept of actuarial soundness. Tufte ignores long-run fiscal considerations. In my view, the interaction between long-run and short-run considerations is a central and essential feature of American social welfare. Without the prospect of long-run returns, cur-

rent payroll-tax-payers would have no incentive to contribute. And without a norm of pay-as-you-go, they would have no reason to believe that their collective investments will ultimately be recouped and therefore, again, no reason to participate.

STATISTICAL MODEL SPECIFICATIONS

Because the time series model and the tests defined in terms of the model are complicated, I shall outline the overall design before giving the exact specifications. Part of the model represents the changing social, economic, and political grounds that determine the value of the policy equilibrium at each time. The expected levels of payments and contributions are broken into components based on five socioeconomic variables reported each month. The effects of these variables depend on a collection of variables reported annually that measure current political circumstances. These time-varying effects represent the idea that social welfare tax and benefit levels do not respond to socioeconomic change automatically but instead reflect a continuing history of political conflicts (see Browning 1986, 44-49). The rules by which the current policy equilibrium is derived from socioeconomic conditions are allowed to change with shifts in the political environment. In formal terms, the elasticity⁶ of each of the fiscal variables with respect to each socioeconomic variable is defined to be a function of the political variables.7

The existence of a pay-as-you-go rule is tested in two ways. One test is based on the similarities between the elasticities estimated for taxes and the corresponding estimates for benefits. Greater similarity indicates a stronger rule of pay-as-you-go. The other test is based on an estimate of the dynamic relationship between tax and benefit levels. This dynamic relationship is modeled as a first-order vector autoregression among the logarithms of the fiscal variables. Zero values for the autoregressive coefficients would suggest that pay-as-you-go (or any other short-run constraint) is purely a matter of discretion. One pattern of nonzero values for the coefficients would suggest that strong, virtually automatic pay-as-you-go constraints exist, while another pattern would suggest the existence of an uncontrollable incrementalism.

The specification of the disturbances in the model is designed to test the idea that the expected levels of the fiscal variables are attracted to an equilibrium path. If they are, then the vector of disturbances should be cointegrated (Engle and Granger 1987). The idea here is that random shocks keep the fiscal variables from exactly following the equilibrium path: equilibrium is never perfectly achieved. A cointegrated disturbance process is evidence that efforts are made systematically to compensate for the shocks, to move the fiscal variables back toward the equilibrium path. The second-order vector autoregressive process

specified for the disturbances in the present model is sufficient to test for such cointegration (more precisely, for an appropriate degree of near cointegration).

The disturbance specification also includes an additive outlier feature, to allow for the possibility that a small portion of the observed data might have been affected by gross errors. "Gross error" in this context means that the affected observations were perturbed by processes unlike the one generally in effect in the data. Such processes are always unrelated to the political and socioeconomic conditions that determine the fiscal variables' expected levels. If the levels are connected in an equilibrium relationship, then the outlier-generating processes are also unrelated to the adjustments being made to stay on the equilibrium path. That is, the outlier neither corrects for previous equilibrium-disturbing shocks nor prompts a subsequent correction.

The idea that tax levels are set according to the concept of actuarial soundness implies a tendency for taxes to be collected in excess of the amount needed to support the current equilibrium level of benefits. That is, if savings are to accrue over the long run, then tax collections must generally exceed benefit payments. But if the aggregate effort is to produce savings over the long run in a reliable way, it should not be affected by the kind of socioeconomic and political fluctuations that in the short run affect the levels of the fiscal variables. The test of the actuarial soundness concept is therefore defined as a pattern that should be observed in the disturbance process. In particular, the test specifies a pattern of positive and negative differences between the disturbances and a vector of drift parameters. The differences for transfer payments should be negative, while the differences for the contributions series should be positive. Such a pattern is possible only if the disturbance process is near cointegrated. An aggregate public savings policy motivated by the concept of actuarial soundness can exist only if there is a longrun fiscal equilibrium.

The idea that election-related oscillations affect the levels of taxes and benefits is tested by checking for differences between even-numbered and odd-numbered years in the elasticities of each fiscal variable with respect to each socioeconomic variable. Significant oscillations should produce significant differences in the elasticities. If benefits grow faster in response to worsening socioeconomic conditions in election years than they do in nonelection years while, in response to the same conditions, taxes grow more slowly, then the elasticities for benefits should increase in election years while those for taxes decrease. But such a pattern of changes would tend to violate pay-as-you-go constraints. Strong constraints would force the changes to have the same sign for all the fiscal variables.

Because the aggregate total of transfer payments does not distinguish tested from nontested program benefits, I rely on the meanings of the socioeconomic variables to identify what kinds of changes would benefit or harm which group of recipients.⁸ The

implications of this approach are clearest for a variable used to measure unemployment. Obviously, unemployed individuals often receive unemployment insurance benefits. But people without jobs also often receive benefits from Aid for Families with Dependent Children (AFDC), public assistance, and other means-tested programs. Election-related differences in the elasticity of benefits with respect to unemployment should therefore be a good measure of how tested-program benefit recipients are treated over the course of the election cycle. My argument predicts that these differences will be negative: in an election year, an increase in unemployment will increase transfer payments less than it would in a nonelection year. This reduction should mirror—and possibly be directly connected to-negative differences in the relationship between unemployment and payroll taxes. Any direct connection should reflect pay-as-you-go constraints: the reason for the reduced provision of benefits even as economic circumstances worsen should appear to be a desire to reduce the burden of taxes during such times.

Similar but weaker expectations apply to a variable measuring the price level. If real income, employment, and unemployment are constant, then rising prices represent worsening economic conditions. A pattern of elasticity differences similar to that for unemployment might therefore be expected. On the other hand, the incidence of inflation's harmful effects is not as clearly skewed as those of unemployment (Hibbs 1987, 43-124). But cost-of-living adjustments have historically been more generous for nontested than for tested programs (Munnell 1987). Due to indexing, adjustments for nontested programs such as Social Security have also more often been automatic (Browning 1986, 132-155). Any election-related changes in the elasticity of transfer payments with respect to the price level therefore most likely reflect changes in tested-program benefits.

Tests of Tufte's kyphosis concept are based on dummy variables that allow the level of transfer payments to increase in the month or so preceding an election, regardless of current socioeconomic or political conditions (1978, 31–44). The possibility of such a pattern is tested separately for presidential and midterm election years. There is also a separate test for the 1972 election that Tufte emphasizes. Dummy variables are also used to test the related notion that the increases in taxes needed to pay for the preelection increase are delayed until shortly after the election.

Model Specification Details

The endogenous variables—transfer payments to persons, personal contributions for social insurance, and employer contributions for social insurance—are respectively denoted T_t , PC_t and EC_t , with the subscript indicating time. These series contain aggregate monthly totals for the United States, seasonally adjusted at annual rates. There are n = 480 monthly

observations. Data are described in detail in the

Appendix.

The model is log-linear in the explanatory and lagged dependent variables but has disturbances that are additive on the original rather than the log scale. Using the column vector $\mathbf{y}_t = (y_{it})' = (y_{1t}, y_{2t}, y_{3t})' \equiv (T_t, PC_t, EC_t)'$, the model can be written

$$\mathbf{y}_t = \tilde{\mathbf{y}}_t + \mathbf{u}_t,$$

where \mathbf{u}_i is a stochastic process and, for a 3×3 matrix $\mathbf{A} = [a_{ik}], i, k \in \{1, 2, 3\},$

$$\log y_t = A \log y_{t-1} + \log x_t, \tag{1}$$

logarithms being taken elementwise. The vector $\mathbf{x}_t = (x_{:t})'$ has elements $x_{it} = \gamma_{it} \prod_{j=1}^4 z_{ijt}^{\beta_{ijt}}$, where each variable z_{ijt} measures a socioeconomic condition and each γ_{ir} and β_{ijt} is a function of social and political factors as will be described. Each function β_{ijt} measures the expected elasticity in each year of the fiscal variable y_{it} with respect to the explanatory variable z_{ijt} .

The disturbance vector \mathbf{u}_t is a second-order vector autoregressive process with drift, contaminated by additive outliers and scaled by $\tilde{\mathbf{y}}_t$. That is, let Σ be a 3×3 symmetric positive definite matrix, such that for all t the serially uncorrelated random vector $\mathbf{v}_t = (v_{it})'$ has mean E $\mathbf{v}_t = \mathbf{0}$ and covariance E $\mathbf{v}_t \mathbf{v}_t' = \Sigma$. For a constant drift vector $\boldsymbol{\mu} = (\mu_i)'$ and 3×3 matrices \mathbf{C}_1 and \mathbf{C}_2 , define

$$\tilde{\mathbf{u}}_t = \boldsymbol{\mu} + \mathbf{C}_1 \tilde{\mathbf{u}}_{t-1} + \mathbf{C}_2 \tilde{\mathbf{u}}_{t-2} + \mathbf{v}_t.$$
 (2)

Let $\lambda_t = (\lambda_{it})'$ be a random process in which each λ_{it} takes an arbitrary finite value with probability π_i and takes the value 0 with probability $1 - \pi_i$. Then $\mathbf{u}_t^* = \tilde{\mathbf{u}}_t + \lambda_t$ describes a process in which $\tilde{\mathbf{u}}_t$ is contaminated with additive outliers λ_t (see Martin 1981). Finally, to reflect the fact that $\mathbf{y}_t \geq 0$, use the diagonal matrix $\mathbf{W}_t = \mathrm{diag} \ (\tilde{y}_{it}^{1/2})$ to define $\mathbf{u}_t = \mathbf{W}_t \mathbf{u}_t^*$ (see McCullagh and Nelder 1989, 193). 11

The specification of x_i reflects the idea that aggregate taxing and spending levels are primarily functions of current socioeconomic conditions, with the relation between levels and conditions being determined by the current political environment. Socioeconomic conditions are measured by five variables: P_t , the Consumer Price Index (CPI) in month t; N_t , the total population; R_t , real income (less transfer payments) per capita; UN_{t-1} , the proportion of the population unemployed; and EMN_t , the proportion of the population employed. For all three fiscal variables, $z_{i1t} = P_t$, $z_{i2t} = N_t$, and $z_{i3t} = R_t$. For the transfer payments equation (i.e., in x_{1t}), $z_{14t} = UN_{t-1}$, while for the contributions equations $z_{24t} = z_{34t} = EMN_t$. Unemployment is lagged one month to reflect the administrative delay between becoming unemployed and beginning to receive unemployment compensation (see United States, National Commission on Unemployment Compensation 1980, 44-45). The employment and price level measures directly reflect explicit social welfare legislation. An argument for including UN_{t-1} in x_{1t} can trace back to Titles III and IX of the 1935 Social Security Act, for example, while

the inclusion of EMN_t in x_{2t} and x_{3t} reflects the fact that PC_t and EC_t are mostly payroll taxes (ibid. 9–19). The inclusion of P_t in x_{1t} can be justified by noting the various provisions that have been enacted to adjust benefits for changes in the cost of living. The other variables can be justified intuitively, as measures of either the need for or resources available to support social welfare program activity. For example, a larger population might imply a larger number of potential benefit claimants, while a higher level of R_t might mean a greater supply of funds available to be extracted through taxation.

The political environment is specified as a function of variables that measure the sizes of key groups of potential social welfare recipients, the political parties' control of national government, sectional distinctions, and the national electoral calendar. Specifically,

$$\beta_{ijt} = b_{ij0} + b_{ij1} \log N15_t + b_{ij2} \log N65_t + b_{ij3} H_t$$

$$+ b_{ij4} S_t + b_{ij5} D_t + b_{ij6} D_t H_t + b_{ij7} D_t S_t + b_{ij8} E_t$$

$$\gamma_{1t} = \exp (\beta_{10t} + \zeta_{1t} + d_1 ON72_t + d_2 F77_t$$

$$+ d_3 F82_t + d_4 FM50_t)$$

$$\gamma_{2t} = \exp (\beta_{20t} + \zeta_{2t})$$

$$\gamma_{3t} = \exp (\beta_{30t} + \zeta_{3t})$$

$$\zeta_{it} = c_{i1} CON_t + c_{i2} PON_t + c_{i3} CJF_t + c_{i4} PJF_t.$$

Each coefficient b_{ijl} measures the effect a political variable has on the elasticity β_{iit} .

Variables $N15_t$ and $N65_t$ reflect the priority historically given to the needs of children and the elderly (Douglas 1939; Skidmore 1975; Witte 1962). $N15_t$ and $N65_t$ denote respectively the number of people in the United States population at time t aged 15 or younger and 65 or older. The age thresholds used to define $N15_t$ and $N65_t$ match those in the key age-related provisions of the 1935 Social Security Act: Title IV of the act defines a "dependent child" for benefit purposes to be a child in specified circumstances under the age of 16; and Title II of the act sets 65 as the age at which qualifying individuals are to begin receiving old-age benefits (see Collins 1967).

Variables H_t , S_t , and D_t represent the effects of political party strength in controlling the national government. H_t denotes the difference between the number of Democrats and the number of Republicans in the U.S. House of Representatives a year before t, while S_t denotes this difference computed only for House seats in the South. The one-year lag reflects the idea that a party's presence in the House affects social welfare policy primarily through legislation, with legislation passed in one year taking effect in the next. The separate count for southern seats picks up the sectional difference that has most importantly affected social welfare legislation (Bensel 1984; Quadagno 1988; Sinclair 1978; Sundquist 1968). D_t, which equals 1 in years when the president is a Democrat and 0 otherwise, is intended to measure partisan

control of at least the national level of social welfare program administration (Derthick 1979; Palmer and Sawhill 1984; Welborn and Burkhead 1989; see also Freeman 1988; Lowi 1979, 198–236). The products D_tH_t and D_tS_t are included to allow the effects of party legislative presence to vary depending on which party controls the executive.

The variable E_t , which equals 1 in even-numbered years and 0 otherwise, is used to measure election-year variations. The variables in ξ_{it} are the dummy variables used to test Tufte's kyphosis concept. CON_t and PON_t equal 1 in October and November of midterm and presidential election years, respectively, otherwise 0. CJF_t and PJF_t equal 1 in January and February following midterm and presidential elections, respectively, otherwise 0. $ON72_t$ in γ_{1t} , which is 1 only in October and November of 1972 (otherwise 0), isolates the 1972 election. The variables $F77_t$, $F82_t$ and $FM50_t$ in γ_{1t} pick up quirks in the transfer payments series. ¹³

Tests for Short-Run, Pay-as-You-Go Constraints

One way to implement pay-as-you-go is to establish policies such that payments and contributions independently respond in the same way to changes in socioeconomic conditions. In the present model, such a direct approach would imply $\beta_{1jt} = \beta_{2jt} = \beta_{3jt}$, for each j=1,2,3. In view of the connection between UN_t and EMN_t , we might further expect the variations over time in β_{14t} to be similar to those in $-\beta_{24t}$ and $-\beta_{34t}$. I modify this test to eliminate the possibly confusing effects of election-year manipulations, by computing each β_{ijt} with $E_t=0$ for all t.

Pay-as-you-go might also mean constraints that kick in after levels of benefits and taxes suitable for current socioeconomic and political conditions have been determined. Such constraints might work in either direction. Taxes might be modified as necessary to support the independently determined level of benefits, but benefit payments might also be made contingent on tax collections. Of course, since benefits are partially paid from revenues other than personal and employer contributions, ¹⁵ exact budgetary balance is not to be expected. So far as the broad range of social welfare expenditures is concerned, this form of pay-as-you-go can hold, at most, as a tendency.

The matrix **A** measures such dynamic constraints. If $\mathbf{A} = \mathbf{0}$, there are no constraints; this is the pure discretion case. Two patterns of nonzero values suggest relatively automatic pay-as-you-go constraints. If benefit payments in one month positively affect taxes in the next month, then positive values should be observed for a_{21} and a_{31} , in the first column of **A**. But if benefits are responding positively to prior month taxes, then a_{12} and a_{13} , in **A**'s first row, should be positive.

Because it makes taxes and benefits difficult to control and virtually impossible to coordinate, incrementalism is the short-run dynamic pattern most contrary to pay-as-you-go. In the extreme form of incrementalism, every shock to a fiscal variable has infinitely persistent effects that do not carry through to the other fiscal variables. Such a system is practically uncontrollable. This extreme case would be represented by A = I, where I is the three-by-three identity matrix. More generally, strongly incrementalist dynamic relations are indicated if the diagonal elements of the matrix that measures the variables' ultimate responses to shocks (on the log scale) are much larger than 1, while the off-diagonal elements remain small (see Browning 1985). This matrix is $(I - A)^{-1} = I + \Sigma_{s=1}^{\infty} A^{s}$.

Tests for Long-run Equilibrium and Actuarial Soundness

Cointegration is a model for long-run equilibrium in a multivariate time series (Engle and Granger 1987; Salmon 1982, 628). Adapting the concept to take into account the present heteroscedasticity and outlier specifications leads to the expectation that the underlying residual series \tilde{u}_{it} should be cointegrated. To diagnose near cointegration, I use the decomposition method of Aoki (1990, 197–228). The test refers to the matrix

$$\mathbf{C} = \begin{bmatrix} \mathbf{C}_1 & \mathbf{C}_2 \\ \mathbf{I} & \mathbf{0} \end{bmatrix}.$$

One eigenvalue of C should approximately equal 1. To build long-term savings in accord with actuarial soundness, transfer payments should tend to be reduced below the equilibrium levels expected given current conditions while contributions are, in a strongly coordinated manner, increased. This happens only if the series \tilde{u}_{it} satisfy $\tilde{u}_{1t} < \mu_1$, $\tilde{u}_{2t} > \mu_2$ and $\tilde{u}_{3t} > \mu_3$, which can occur meaningfully only if the \tilde{u}_{it} are cointegrated (see Engle and Granger 1987, 253). The comparisons are between \tilde{u}_{it} and μ_i rather than between \tilde{u}_{it} and 0, because a shift in the mean of \tilde{u}_{it} away from 0 that is due solely to $\mu_i \neq 0$ cannot be related to any compelling concept of a fiscal constraint. To

If $\tilde{\mathbf{u}}_t$ is cointegrated, then the nonzero outliers $\boldsymbol{\lambda}_t$ represent shifts in benefits or in contributions that are not adjustments made to move toward equilibrium. The $\boldsymbol{\lambda}_t$ are, in any case, unrelated to \boldsymbol{x}_t .¹⁸

Tests of Election-related Manipulation

Election-related differences in the elasticities of each fiscal variable with respect to each socioeconomic condition are measured using the coefficients of the election-year dummy variable E_t . The differences with respect to CPI, population, income, and unemployment are respectively measured by

$$\begin{split} \boldsymbol{\Delta}_{P} &\equiv (\hat{b}_{118},\,\hat{b}_{218},\,\hat{b}_{318})'\,,\\ \boldsymbol{\Delta}_{N} &\equiv (\hat{b}_{128},\,\hat{b}_{228},\,\hat{b}_{328})'\,,\\ \boldsymbol{\Delta}_{R} &\equiv (\hat{b}_{138},\,\hat{b}_{238},\,\hat{b}_{338})'\,,\\ \boldsymbol{\Delta}_{UN} &\equiv (\hat{b}_{148} - \hat{a}_{12}\,\hat{b}_{248} - \hat{a}_{13}\,\hat{b}_{348},\,-\,\hat{b}_{248},\,-\,\hat{b}_{348})'\,.^{19} \end{split}$$

The first element in each vector Δ_j measures the difference in the elasticity of transfer payments with respect to socioeconomic variable j, while the second and third elements measure the differences for personal and employer contributions, respectively. The negations of \hat{b}_{248} and \hat{b}_{348} are used in Δ_{UN} to allow all the components of Δ_{UN} to be interpreted as differences in elasticities with respect to unemployment. The elasticity difference for transfer payments is measured by $\hat{b}_{148} - \hat{a}_{12}\,\hat{b}_{248} - \hat{a}_{13}\,\hat{b}_{348}$ because transfer payments are directly affected by unemployment in the preceding month.

Because of the lagged vector \mathbf{y}_{t-1} in equation 1, differences in the elasticities of future values of the fiscal variables with respect to current conditions are also relevant. For each time offset $\tau=1,2,\ldots$, such differences (for $\mathbf{y}_{t+\tau}$ with respect to P_t , etc.) are measured by $\mathbf{\Delta}_j^{\tau} \equiv \mathbf{A}^{\tau} \mathbf{\Delta}_j$, for $j \in \{P, N, R, UN\}$. In the spirit of Tufte's argument, one might expect

such elasticity differences to show that in election years incumbents try to make social welfare policy more responsive to individuals' needs. On the benefits side, this should mean larger increases in transfer payments in response to rising prices, increasing population, falling income, or rising unemployment. On the tax side, greater responsiveness in election years should mean that these same conditions meet with larger reductions. In terms of the elasticitydifference measures Δ_i , these expectations correspond to the sign patterns $\Delta_P = (+, -, -)', \Delta_N = (+, -, -)', \Delta_R = (-, +, +)'$ and $\Delta_{UN} = (+, -, -)'$. Recall that the elements in these patterns refer to the coefficients or combinations of coefficients for transfer payments, personal contributions, and employer contributions, respectively. Similar patterns might be expected for the Δ_i^{τ} values.

Such expectations assume that incumbents can move benefits and taxes in opposite directions at the same time. Strong fiscal constraints would rule out such flexibility. Instead, in response to each socioeconomic condition, benefits and taxes would have to be moved in the same direction. In this case, the values in each Δ_i and in each ${\Delta_i}^{\tau}$ should all have the same sign. Because the distributive implications of these differences are clear only for unemployment and CPI, this prediction is well motivated only for these variables. My argument predicts that benefits will be less responsive to need in election years, in order to allow the rate of increase in taxes to be slowed at such times. Using the interpretations of unemployment and CPI, this prediction becomes the expectation that the signs in Δ_P , Δ_P^{τ} , Δ_{UN} and Δ_{UN}^{τ} will all be negative.

The tests for kyphosis and for the related idea that tax increases are delayed until after the election, refer to the coefficients of CON_t , PON_t , CJF_t , and PJF_t . If the idea is correct, CON_t and PON_t should have positive coefficients in the transfer payments equation, while CJF_t and PJF_t should have positive coefficients in the contributions equations. A positive coefficient for $ON72_t$ would indicate exceptional increases in transfer payments in 1972.

TEST RESULTS

The estimation procedure is described in the Appendix. Estimates for the coefficients in the elasticities β_{ijt} appear in Table A-2 in the Appendix. The present discussion focuses on the tests just described.

Pay-as-you-go Constraints

Inspection of the estimated elasticities $\hat{\beta}_{ijt}$ (computed with $E_t = 0$ for all t) suggests that there is a strong tendency toward contemporaneous pay-as-you-go constraint between transfer payments and payroll taxes, though there is no evidence of a strict budget constraint. The plots of the elasticities for each year, in Figure 1, reveal clear similarities between the transfer payments equation and the equations for contributions but also obvious discrepancies.²² The product-moment correlations between the elasticities for transfer payments and the respective elasticities for contributions summarize the similarities. For personal contributions, the correlations for the elasticities for CPI, population, income and unemployment are .66, .63, .83, and .54. For employer contributions they are .80, .26, .25, and .41. By this measure, the constraints between transfer payments and personal contributions are stronger than those between transfer payments and employer contributions. Because the difference between employer contributions and personal contributions mostly reflects payments made only by employers for state unemployment insurance and workman's compensation, these results suggest that contemporaneous pay-as-you-go exists primarily at the federal level.

The test for dynamic constraints shows weak evidence of automatic pay-as-you-go. In the estimate²³

$$\hat{\mathbf{A}} = \begin{bmatrix} .374 & .152 & - .145 \\ - .068 & .414 & - .330 \\ .037 & - .039 & .099 \end{bmatrix},$$

two values are compatible with pay-as-you-go. The estimate $\hat{a}_{12} = .152$ indicates a moderate tendency for transfer payments to be made contingent on personal contributions in the preceding month. The estimate $\hat{a}_{31} = .037$ indicates a much weaker tendency for employer contributions to be modified to support transfer payment benefits.²⁴ The negative values for \hat{a}_{13} and \hat{a}_{21} are not interpretable from the pay-as-you-go perspective.

Overall, the dynamic relations between payments and contributions are weakly incrementalist. The diagonal elements in

$$(\mathbf{I} - \hat{\mathbf{A}})^{-1} = \begin{bmatrix} 1.528 & .422 & - .401 \\ - .217 & 1.687 & - .585 \\ .071 & - .055 & 1.118 \end{bmatrix}$$

are somewhat, but not dramatically, larger than 1, while the off-diagonal elements are small or moderate

in size. For example, a unit shock in $\log y_{1t}$ ultimately produces a change of 1.528 in $\log y_{1t}$ but changes $\log y_{2t}$ by -.217. These results contradict Browning's (1985) finding of strong incrementalism in social welfare expenditures.

Long-run Equilibrium and Actuarial Soundness

The test for near cointegration shows strong evidence that the fiscal variables are bound to one another in a long-run equilibrium relationship. The estimates for C_1 and C_2 are

$$\hat{\mathbf{C}}_1 = \begin{bmatrix} .783 & - .481 & .694 \\ - .055 & .264 & .433 \\ .007 & - .008 & .807 \end{bmatrix},$$

$$\hat{\mathbf{C}}_2 = \begin{bmatrix} .165 & .004 & - .102 \\ .017 & .292 & .073 \\ - .012 & - .033 & .052 \end{bmatrix}.$$

The eigenvalues of \hat{C} are .991, .774 \pm .091 $\sqrt{-1}$, -.456, -.177, and -.053. As the test requires, one value is approximately 1.²⁵

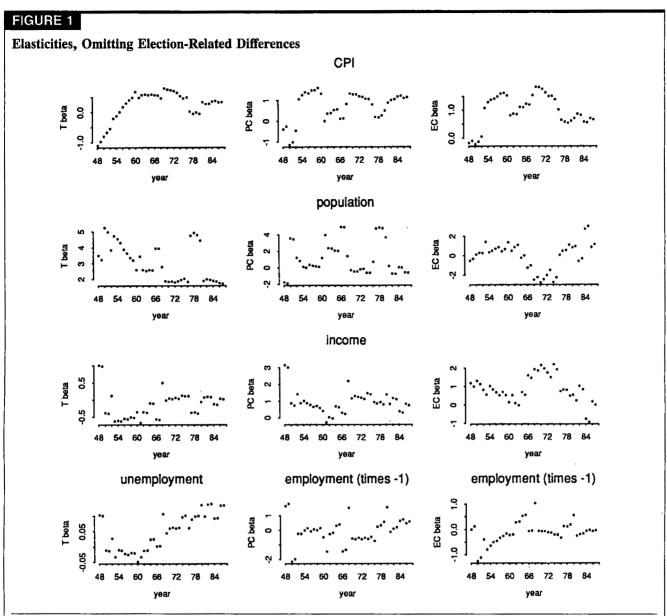
There is also strong evidence that the equilibrating adjustments are designed to build long-term savings. The estimates $\hat{\mu} = (-.37, -.24, .12)'$ compare in the required manner to the residuals $\hat{\mathbf{u}}_i$. Figure 2, which displays the weighted differences $\hat{y}_{it}^{1/2}(\hat{u}_{it}-\hat{\mu}_i)$, in units of 1984 dollars per person, on a monthly basis, ²⁶ illustrates this. The differences for transfer payments are negative, those for contributions, positive.

The savings-building adjustments involve large amounts of money. This point can be seen in Figure 2. Over the entire 1948–87 period, adjustments in transfer payments average to a reduction of \$52.74 per person in each month. This corresponds to an annual, national aggregate reduction averaging \$127.2 billion per year. The adjustments in contributions are smaller but still substantial. In per capita terms, the combined adjustments in personal and employer contributions average to an increase of \$3.74 per person in each month. This corresponds to an annual, national aggregate increase averaging \$9.0 billion per year.

Election-related Manipulation

The estimates of the election-related differences in the elasticities do not suggest that incumbents make social welfare policy more responsive to everyone's needs in election years. Figure 3 plots the differences in elasticity with respect to CPI, population, income, and unemployment, for each fiscal variable. That is, the figure shows the values of Δ_j (at delay = 0), and the values of Δ_j^{τ} for $\tau = 1, \ldots, 7.27$ None of the Δ_j or Δ_j^{τ} vectors have the sign patterns predicted by the idea that election-year policy is more responsive.

But there is support for the idea that fiscal constraints force incumbents to trade the interests of payroll-tax-payers against those of tested-program

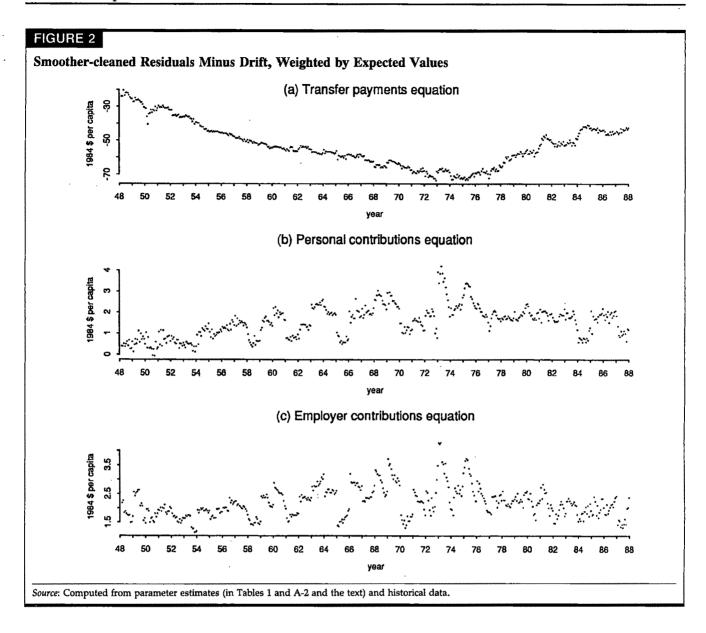


Source: Computed from parameter estimates in Table A-2 and historical data.

Note: The elasticities (beta) of transfer payments (T), personal contributions (PC) and employer contributions (EC) with respect to each socioeconomic variable are computed with the election-year dummy variable set equal to 0 for all years. The elasticity for each year is the percentage change in the expected value of each fiscal variable induced by a percentage change in the socioeconomic variable in that year, other things equal.

benefit recipients. The patterns expected for unemployment and CPI if benefits are being reduced to support reductions in taxes are clearly in evidence for transfer payments and personal contributions. The differences are negative at all time offsets for both CPI and unemployment. (For personal contributions, this means employment \times -1.) The contemporaneous differences are all statistically significant, and the one-period-lagged differences are significant for transfer payments.²⁸ The differences for employer contributions do not follow the expected pattern but are statistically insignificant.

The elasticity differences for transfer payments are related to the differences for personal contributions in a way that strongly suggests the operation of a pay-as-you-go constraint. Indeed, the value $\hat{b}_{148} - \hat{a}_{12}\hat{b}_{248} - \hat{a}_{13}\hat{b}_{348}$ for transfer payments in Δ_{UN} is negative only because of the positive estimate for the coefficient \hat{a}_{12} that links transfer payments in the current month to personal contributions in the previous month. When employment decreases (say because of a rise in unemployment), personal contributions decrease more rapidly in an election year than they do in a non-election year (i.e., $\hat{b}_{248} > 0$). If the link between transfer payments and contributions is ignored, an increase in unemployment appears to make transfer payments increase more rapidly in an election year ($\hat{b}_{148} > 0$). But in fact, due to the link, transfer payments increase less rapidly (i.e., $\hat{a}_{12}\hat{b}_{248} > \hat{b}_{148}$). For CPI, the link between transfer payments

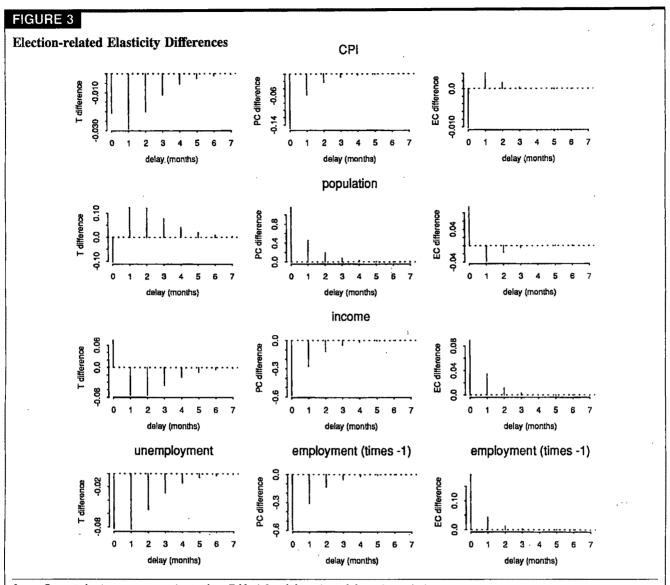


and contributions manifests itself in the fact that the first lagged elasticity difference for transfer payments is larger (more negative) than the contempo-

The election-related differences in the elasticity of transfer payments with respect to unemployment are nearly as large as the variations over time in the remainder of the elasticities for unemployment. This can be seen by comparing Figure 3 to the plot for unemployment in Figure 1. The non-election-related part of the elasticities, shown in Figure 1, ranges from a low value of -.05 in 1960 to a high value of .14 in 1983. This range is just more than double the magnitude of the value of -.08 observed for the initial and one-period-lagged election-related differences. This comparison suggests the substantive significance of the election-related oscillations in transfer payments' response to unemployment.

The results of these election-related differences are nonetheless sufficiently small that the changes can be viewed as manipulations at the margins. Simulations based on the model estimates show the changes inducing an average per capita reduction of \$.74 per month in transfer payments during the study years, which can be compared to the average per capita transfer payment of \$102.86 per month (both amounts in 1984 dollars). The per capita change in personal contributions averages to a reduction of \$.58 per month, versus an average per capita contribution of \$29.11.²⁹

The tests for kyphosis give no indication that transfer payments are usually increased in the month before the election, though there is strong evidence of such manipulation in 1972. The estimates reported in Table 1 for CON_t and PON_t in the transfer payments equation are both smaller than their standard errors. But the coefficient for $ON72_t$ suggests an October–November increase of approximately 4.2% above what would otherwise have been expected, given the conditions and policies in effect at that time. ³⁰ Since



Source: Computed using parameter estimates from Table A-2 and the estimated dynamic matrix A.

Note: Figures show how the elasticities of transfer payments (T), personal contributions (PC) and employer contributions (EC) with respect to each socioeconomic variable differ in even-numbered, as compared to odd-numbered, years. Differences are shown for the elasticities both of the current value (delay = 0) and of future values (delay > 0) of each fiscal variable with respect to each socioeconomic variable.

the expected amount of transfer payments in each of those months was approximately \$10.96 billion (in 1972 dollars), this corresponds to an increase of more than \$450 million in each month.³¹

On the tax side, the estimates give no evidence of special increases in contributions following midterm elections but do point to such increases following presidential elections. The coefficient estimates for CJF_t are smaller than their standard errors, but the estimates for PJF_t are clearly positive. The presidential results should not be read as supporting the tax-shifting idea, however. For the estimate for PJF_t in the transfer payments equation shows a tendency for transfer payments also to increase following presidential elections. Indeed, in dollar terms the estimated .85% increase in transfer payments is just slightly larger than the estimated increases of 1.31%

and .77% in personal and employer contributions, respectively.³² So the idea that payroll tax increases are postponed until after the election is supported only with an important qualification: there is also a delayed increase of comparable magnitude in transfer payment benefits.

The impression that 1972 was exceptional is reinforced by a pattern appearing in the estimated outliers. High-density plots of the outlier estimates $\hat{\lambda}_{it}$ appear in Figure 4. Most of the outlier values are zero, and most of the nonzero values do not occur in any readily interpretable pattern. The chief exception is a sequence of increasingly negative values for employer contributions during 1972. In 1972 dollars, the values correspond to monthly reductions, from July through December, of \$37 million, \$56 million, \$110 million, \$216 million, \$317 million, and \$406

VARIABLE	TRANSFER PAYMENTS EQUATION	PERSONAL CONTRIBUTIONS EQUATION	EMPLOYER CONTRIBUTIONS EQUATION
October-November			
Midterm year	.0013 (.0017)	.0038 (.0022)	.0038 (.0018)
Presidential year	0017 (.0019)	0049 (.0024)	.0043 (.0019)
January-February	, ,		
Midterm year	−.0033 (.0018)	0017 (.0022)	.0010 (.0018)
Presidential year	.0085 (.0019)	.0131 (.0024)	.0077 (.0020)
October-November, 1972	.0417 (.0059)		_
Fiscal year adjustment			
I (F77)	0020 (.0002)	- Andrews	_
II (F82)	.0017 (.0002)	endered.	-
February-March, 1950	.2720 (.0139)		_

million.³³ These reductions are followed by increases in April, May, and July 1973 amounting to \$51 million, \$17 million, and \$10 million, respectively. The outliers for personal contributions also indicate reductions during 1972, in August and September, amounting to \$83 million and \$80 million. In total, the preelection reductions in contributions during 1972 are roughly the same size as the October-November increases in transfer payments—the July-December reductions total approximately \$1.24 billion, as compared to the October-November increase of approximately \$900 million.³⁴ Such results qualify Tufte's conclusion that the 1972 acceleration he documents in real disposable income was due to "an autumn increase in transfer payments" (1978, 52). Cuts in payroll taxes were at least as important.

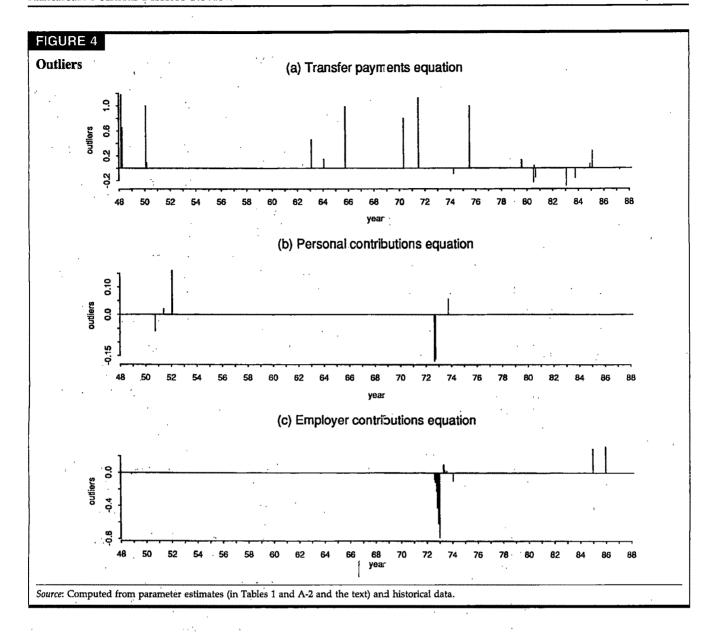
CONCLUSION

The statistical tests strongly support the idea that interaction between long-run and short-run fiscal considerations is a central feature of American social welfare. The near-cointegration test provides direct evidence that transfer payments and payroll taxes are bound together in a long-run equilibrium relationship. That relationship appears to be structured in part by the concept of actuarial soundness. Regarding the short run, there is evidence that the principle of pay-as-you-go constrains how transfer payments and

payroll taxes respond to changing social and economic conditions. Finally, there is evidence that these constraints lead to reductions in the benefits provided through means-tested programs in election years, so as to support small election-year reductions in payroll taxes.

The dynamic relations estimated among the fiscal variables suggest that pay-as-you-go is a norm that must be continually sustained through discretionary adjustments based on political will, rather than an administrative rule that will usually be satisfied in due course automatically. Incrementalist dynamics exist, exacerbating the disruptive effects of exogenous trends. But these incrementalist tendencies are sufficiently weak that discretionary actions are capable of maintaining a long-run balance between taxes and benefits. Discretionary management along these lines is well documented for Social Security (Bernstein and Bernstein 1988). But for the rest of the social welfare system one is hard pressed to cite historical precedents or institutional grounds for such coherence. It is possible that the present evidence of long-run fiscal constraints stems solely from the fiscal arrangements in Social Security.

It is important to recognize limitations in the evidence regarding long-run constraints. The near-cointegration test does not precisely indicate the targets to which equilibration is being directed. The test of the concept of actuarial soundness does not trace the exact balance being maintained between



taxes and benefits over time but rather only tests whether the equilibrating adjustments tend to move taxes and benefits in the directions long-term actuarial soundness would generally require. The tests are adequate for the present theoretical concerns, but are not intended to be and should not be interpreted as substitutes for careful accounting (see, e.g., Munnell and Blais 1984).

It is worthwhile to revisit the issue of the size of the election-year manipulations. The estimate reported of an average per capita reduction in transfer payments of \$.75 per month (in 1984 dollars) was computed relative to the entire United States population. But the idea that the reductions affect only tested-program benefit recipients suggests that this procedure seriously underestimates the reductions' impact on those whose benefits are cut. It would be better to compute the reductions relative to the size of the tested-program recipient group. If this size is measured by the number of persons unemployed in each month, then the estimated average reduction per

person rises to a much more substantial value of \$45 per month. For an intuitive feel for the importance of this amount, consider that seven months of such a reduction would exceed the Census estimate of median monthly gross rent in the United States in 1980, which was \$304 (in 1984 dollars; United States, Bureau of the Census 1983, 23). Seven months of this reduction would also exceed the average monthly benefit paid in 1988 through AFDC, which was \$288 (in 1984 dollars; Peterson and Rom 1990, 167). Clearly, a reduction of \$45 per month would be a significant cost for a low-income person already in dire straits due to job loss. Admittedly, this estimate probably overstates the typical individual impact of the election-year reductions, because the reductions in transfer payments are probably not entirely concentrated on the unemployed. But it is closer to the mark than the original estimate. Even reductions half as big would mean significant fluctuations from year to year in the financial assistance being given to affected poor people.

APPENDIX

Data Descriptions and Sources

Definitions of variables are given in Table A-1.

TABLE A	-1	
Variable D	efinitions	
SYMBOL	UNITS	DESCRIPTION
Monthly		
T	\$ billion	Transfer payments ^a
PC	\$ billion	Personal contributions for social insurance ^a
EC	\$ billion	Employer contributions for social insurance ^a
<i>P</i>	index	CPI, all items (1982-84 = 100)
N	1,000s	Population, total (including armed forces overseas) ^b
1	\$ billion	Personal income less transfer payments ^a
U	1,000s	Labor force, civilian, unemployed, seasonally adjusted
EM	1,000s	Labor force, civilian, employed, seasonally adjusted
Annual		
N15	millions	Population aged 15 or younger, 1 July
N65	millions	Population aged 65 or older, 1 July
Н	count	No. Democrats minus Republicans in House, previous year
S	count	No. Southern ^o Democrats minus Southern Republicans in House, previous year
Special		
['] F77		0 before 1977, annual July—June trend (July = 1, , June = 12) otherwise
F82		0 before 1982, annual July—June trend (July = 1,,
FM50		June = 12) otherwise 1 if February or March of 1950, 0 otherwise

Sources: Tt, PCt, It: Bureau of Economic Analysis, Business Statistics 1979 (1948-60) and 1986 (1961-86) and Survey of Current Business, February and August 1988 (1987). EC, unpublished data from Bureau of Economic Analysis, Government Division. P_t , U_t , EM_t : same as T_t , except 1987, from Business Conditions Digest, August 1988. N_t : the Bureau of the Census's Current Population Reports, ser. P-25, nos. 27, 35, 45 (1948–49), 1022 (1950-87), 229 (Alaska and Hawaii, 1 July 1950-59). N15, N65; Bureau of the Census, Historical Statistics of the United States: Colonial Times to 1970 (1948-1959); Statistical Abstract of the U.S., 1987 (1970, 1979); and Current Population Reports, ser. P-25, nos. 519 (1960-69), 721 (1971-75), 800 (1976-78), 1022 (1980-87). H_t, S_t: Joint Committee on Printing, Congressional Directory (80th Cong.); Statistical Abstract of the U.S., 1954 (81st-83d), 1957 (84th-85th), 1963 (86th-88th), 1967 (90th), 1975 (92d, 1981-804), 1967 (81st-83d), 1957 (84th-85th), 1968 (87th-86th), 1969 (90th), 1975 (92d, 1981-804), 1960 (90th), 1975 (92d, 1981-804), 1960 (90th), 1975 (92d, 1981 94th), 1980 (89th, 91st, 93d), 1984 (95th-96th), 1988 (97th-100th). Note: United States aggregate totals, 1948-87, excluding Alaska and

Hawaii before 1960.

"Monthly totals seasonally adjusted at annual rates. Interpolation used to exclude Alaska and Hawaii for 1950-59.

Southern states = AL, AR, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV.

Estimation Procedure

The estimation procedure is a variation of the generalized method of moments studied by Gallant (1987, 442-46, 566-83). Apart from a smoother-cleaner operation used to make the estimates robust (see step 3), the procedure implements the estimator specified by Gallant (ibid., 446). Estimation is iterative. ³⁵ Before the first iteration, assign $y_t^{\#} := y_t$. Then each iteration has five steps. In order, do

1. For each $i: \hat{x}_{it} \leftarrow \hat{a}_{ik}, \hat{b}_{ijl}, \hat{c}_{im}, \hat{d}_m \leftarrow MQLE (y_{it}^{\#}).$ 2. $\log \hat{\mathbf{y}}_t := \hat{\mathbf{A}} \log y_{t-1}^{\#} + \log \hat{\mathbf{x}}_t, \hat{\mathbf{W}}_t := \operatorname{diag} (\hat{y}_{it}^{1/2}), \hat{\mathbf{u}}_t^{\#} := \hat{\mathbf{W}}_t^{-1} (\mathbf{y}_t - \hat{\mathbf{y}}_t).$ 3. For each $i: \hat{u}_{it} \leftarrow SMO(\hat{u}_{it}^{\#}).$ 4. $\hat{\mathbf{C}}_{1,t} \hat{\mathbf{C}}_{2,t}, \hat{\mathbf{y}}_t, \hat{\mathbf{\Sigma}} \leftarrow VAR(\hat{\mathbf{u}}_t)$ 5. $\hat{\mathbf{v}}_t^{\#} := (\hat{\mathbf{\Sigma}}^{-1/2})' \hat{\mathbf{v}}_t, \mathbf{y}_t^{\#} := \hat{\mathbf{y}}_t + \hat{\mathbf{W}}_t [\operatorname{diag} (\hat{\sigma}_{it}^{-1/2})] \hat{\mathbf{v}}_t^{\#}.$

At step 1, the coefficient parameters are estimated separately for each equation, by maximizing the quasi-likelihood function defined by assuming the mean $E y_{it}^{\#} = \hat{y}_{it}$ and covariance matrix $s_i^2 \hat{\mathbf{H}}$, where $s_i^2 > 0$ and $\hat{\mathbf{H}} = \text{diag}(\hat{y}_{i1}, \ldots, \hat{y}_{in})$ (see McCullagh and Nelder 1989, 324–28). Notice that these assumptions imply independent observations. Obviously, when $\mathbf{y}_{t}^{\#} = \mathbf{y}_{t}$, this contradicts the specification of $\mathbf{\tilde{u}}_{t}$ as autoregressive (ignoring the trivial case where $C_1 = C_2 = 0$), the allowance of nonzero values for μ , and the specification of arbitrary nonzero values for λ_t . The formula used to compute y_t^* at the end of each iteration can be thought of as purging these features, so that ultimately y_t^* does satisfy the independence assumption.

In step 2, estimated values are assigned for $\log \tilde{y}_t$, $\hat{\mathbf{W}}_t$ and $\hat{\mathbf{u}}_t^{\#}$. Then step 3 uses a robust smootherclearer algorithm (Martin and Thomson 1982) to estimate the uncontaminated residual process. Each series $\hat{u}_{it}^{\#}$ is smoother-cleaned separately, based on separately computed bounded influence estimates of its univariate autoregressive parameters (Martin 1980).

At step 4, Whittle's recursion is used to estimate the vector autoregressive parameter matrices C_1 and C_2 , the covariance matrix Σ , and the innovations \mathbf{y}_{t} (Whittle 1953; Whittle 1963, 101–2). In the recursion, each component series \hat{u}_{it} is centered using the sample mean

$$n^{-1}\sum_{t=1}^n \hat{\bar{u}}_{it}.$$

Notice that the estimates \hat{C}_1 and \hat{C}_2 are consistent even if the distribution of \mathbf{v}_t has "heavy tails" as compared with the Gaussian case (Hannan 1970, 329;

see Phillips 1987; Phillips and Durlauf 1986). At step 5, the Cholesky decomposition $\hat{\Sigma}^{1/2}$ of $\hat{\Sigma}$ is used to compute approximately uncorrelated pseudoused to compute approximately uncorrelated pseudo-innovations $\hat{\mathbf{y}}_t^\#$. That is, at least for the converged values, plim $\hat{\mathbf{v}}_t^\#\hat{\mathbf{v}}_t^\#' = \text{plim} (\hat{\mathbf{\Sigma}}^{-1/2})'$, $\hat{\mathbf{v}}_t\hat{\mathbf{v}}_t'$ $\hat{\mathbf{\Sigma}}^{-1/2} = (\mathbf{\Sigma}^{-1/2})'\mathbf{\Sigma}\mathbf{\Sigma}^{-1/2} = \mathbf{I}$. Then the $\hat{\mathbf{y}}_t^\#$ are weighted, using $\hat{\mathbf{W}}_t$ and the diagonal elements $\hat{\sigma}_{ii}$ of $\hat{\mathbf{\Sigma}}$, and added to $\hat{\mathbf{y}}_t^\#$ to produce the value of $\mathbf{y}_t^\#$ to be used in the next iteration. By this procedure, we should have, at convergence, $s_i^2 \approx \hat{\sigma}_{ii}$ (see McCullagh and Nelder 1989,

Upon convergence of the iterative procedure, μ is

estimated by applying the least squares estimator described by Aoki to the smoothed residuals $\hat{\mathbf{u}}_t$ (1990, 213–21)

213–21). The innovations $\hat{\mathbf{v}}_i$ lack skew but do have heavier-than-Gaussian tails. The covariance matrix is

	.02566	00180	.00045	
$\hat{\Sigma} =$	00180	.00303	.00110	
	.00045	.00110	.00250	

Table A-2 reports estimates for the coefficients in β_{ijt} .

	TRANSI PAYME		PERSONAL CONTRIBUTIONS		EMPLOYER CONTRIBUTIONS	
VARIABLE	ESTIMATE	S.E.	ESTIMATE	S.E.	ESTIMATE	S.E.
PI						
Constant = 1	-15.413	1.307	-14.873	2.755	-9.878	2.28
Pop. aged ≤ 15 (log)	3.904	.306	2.731	.584	4.461	.48
Pop. aged ≥ 65 (log)	019	.264	1.380	.461	-2.072	.37
House party balance	.0008	.0004	006	.001	001	.00
House balance, South	.001	.002	.017	.004	007	.00
Democratic president	448	.159	.864	.437	.364	.36
Dem. pres. × balance	001	.001	001	.002	.001	.00
Dem. pres. × bal. South	.002	.002	019	.004	012	.00
	021	.002	149	.045	010	.03
Election year	021	.007	143	.040	.010	.00
opulation	20.220	5.122	10.460	12.449	-18.534	10.38
Constant = 1	32.338			2.693	-18.354 -8.251	2.26
Pop. aged ≤ 15 (log)	-6.538	1.558	-2.168			
Pop. aged ≥ 65 (log)	-1.303	1.585	-1.039	2.359	14.017	1.92
House party balance	005	.003	.011	.007	008	.00
House balance, South	.011	.013	.009	.021	.134	.01
Democratic president	1.711	1.570	1.115	2.491	1.512	2.04
Dem. Pres. × balance	.022	.003	.027	.010	001	.00
Dem. Pres. × bal. South	031	.014	015	.020	021	.0
Election year	103	.082	1.153	.244	.094	.19
come						
Constant = 1	1.929	3.237	16.019	5.876	19.733	4.90
Pop. aged ≤ 15 (log)	.333	.653	773	1.161	2.634	.99
Pop. aged ≥ 65 (log)	826	.519	-3.200	.800	-8.095	.6
House party balance	.003	.001	.003	.003	.005	.00
	016	.005	034	.008	073	.0
House balance, South	1.205	.480	2.874	.769	800	.63
Democratic president	011	.002	011	.004	.005	.0
Dem. Pres. × balance			017 018	.007	.006	.0.
Dem. Pres. × bal. South	001	.004		.107	.089	.0:
Election year	.073	.039	583	.107	.009	.0.
Jn)emp. ^a					45.000	0.4
Constant = 1	.524	.541	16.693	9.835	15.622	8.1
Pop. aged ≤ 15 (log)	140	.102	.342	1.936	-2.720	1.6
Pop. aged ≥ 65 (log)	.077	.080	-5.236	1.377	-1.311	1.0
House party balance	0001	.0003	.012	.005	.003	.0
House balance, South	0018	.0008	039	.014	009	.0
Democratic president	.115	.082	6.486	1.380	-2.544	1.1
Dem. Pres. × balance	0005	.0003	.015	.007	.007	.0
Dem. Pres. × bal. South	0004	.0008	.050	.013	.015	.0
Election year	.035	.008	.592	.168	–.190	.1
constant = e						
Constant = 1	-313.967	89.995	90.866	187.207	456.010	156.3
Pop. aged ≤ 15 (log)	67.556	24.529	9.776	39.992	106.575	33.8
Pop. aged \geq 65 (log)	8.905	22.435	-25.853	32.328	-237.873	26.3
	.090	.047	074	.099	.154	.0
House party balance	288	.198	533	.305	-2.293	.2
House balance, South	266 -7.503	22.262	3.813	34.029	-29.371	28.0
Democratic president			419	.139	.057	.1
Dem. Pres. × balance	375	.063	.140	.285	.370	.2
Dem. Pres. × bal. South Election year	.367 2.153	.202 1.349	-18.418	3.665	453	2.9

Notes

I thank Charles Uthus, Bureau of Economic Analysis, for data on employer contributions for social insurance. Thanks also go to Ben Ginsberg, Bill Keech, Elizabeth Sanders, Martin Shefter, and Sherri Wallace for helpful comments on an earlier draft.

1. For example, suppose current recipients require current returns at rate r > 0 on their earlier tax payments p. That is, they require current benefits valued at rp. Under pay-as-yougo, current taxes to supply such benefits would increase with

2. The equilibrium concept is not the static idea of a point at which taxes and benefits remain constant at particular values. The long-run equilibrium tested by the cointegration model I shall use represents a particular kind of equilibrium

- 3. The pattern of regular oscillations around the long-run equilibrium path distinguishes my predictions from those of Director's Law (Stigler 1970). Director's Law suggests a permanent bias in public expenditures in favor of the middleincome classes, with taxes shifted to fall disproportionately on the poor and rich, due in part to differences in voter participation among income classes. Oscillations such as I predict would neither prove nor disprove the existence of such a permanent bias in American social welfare policy.
- 4. Alt and Chrystal review other problems with Tufte's theory and evidence (1983, 118–22). Nordhaus (1989) reviews recent theoretical developments.
- 5. The possibility that extra payments are funded by debt complicates matters, but in any case, debt should not be analyzed as if it were "free money." The complications are many (e.g., Buchanan 1967, 256-66). I raise only the following question: If debt is a source of "free money" before the election, why only then? Empirical evidence regarding election-related variations in debt is mixed (Laney and Willett 1983).
- 6. The elasticity is the percentage change in a fiscal variable expected, given a percentage change in a socioeconomic variable, other things equal (see Gujarati 1988, 145; Tufte 1974, 113-15).
- 7. The selection of economic and political variables is similar to those used by Browning (1985) and Berry and Lowery (1987), though they were not trying to represent time-varying elasticities.
- 8. Plainly, it would be preferable to analyze benefits from tested and non-tested programs as separate series. Unfortunately, the necessary program-level data are not readily available at the frequency needed to implement the statistical
- 9. "Transfer payments to persons" includes "income payments to persons, generally in monetary form, for which they do not render current services" (United States, Department of Commerce, Bureau of Economic Analysis 1987, 3, n. 1). "Personal contributions for social insurance" includes "payments by employees, self-employed, and other individuals who participate in the following programs: Federal old-age, survivors, disability, and hospital insurance; supplementary medical insurance; State unemployment insurance; railroad retirement; government retirement; veterans life insurance; and temporary disability insurance" (ibid., 1, n. 3). "Employer contributions" includes matching, supplemental and other

payments made for such programs by employers.

10. Each β_{ijt} is defined to vary annually. Exactly, $\beta_{ijt} = \frac{1}{2} \left(\frac{1}{2} \left(\frac{1}{2} \right) \right)$ $(\partial \tilde{y}_{ii}/\partial z_{iji})/(\tilde{y}_{ii}/z_{ijt})$. But as the number of observations in each year grows large, $\beta_{ijt} = \text{plim} (\partial y_{ii}/\partial z_{ijt})/(y_{ii}/z_{ijt})$ for each year.

- 11. Checks using several choices from the power family of variance functions in some simpler, univariate models supported the variance specification used in the text (McCullagh and Nelder 1989, 400). The alternative, gammalike model, in particular, produced inferior results (ibid., 285).
- 12. In terms of the variables defined in Table A-1, R_t =
- $I_t/(P_tN_t)$, $UN_{t-1}=U_{t-1}/N_t$ and $EMN_t=EM_t/N_t$.

 13. F77, and F82, (see Table A-1 for definitions) bracket a

period when the shift in the federal government fiscal year from July-June to October-September apparently induced a seasonal fluctuation in transfer payments not removed by the government's seasonal adjustment procedures. FM50, picks up a large, temporary increase observed in T, in February and March of 1950.

14. See n. 20.

15. Since 1955, the ratio of transfer payments to contributions has varied between approximately 1.4 and 1.7. During this period, approximately 5% of transfer payments were nongovernmental.

16. C is the dynamic matrix in the state space representation of equation 2 given by $\mathbf{z}_t = \mathbf{C} \mathbf{z}_{t-1} + \mathbf{e}_t$, $\mathbf{\tilde{u}}_t = [\mathbf{I} \ \mathbf{0}] \mathbf{z}_t$, where $\mathbf{z}_t = (\bar{\mathbf{u}}_t', \bar{\mathbf{u}}_{t-1}')'$ is the state vector and $\mathbf{e}_t = (\boldsymbol{\mu}' + \mathbf{v}_t', \mathbf{0}')'$. The case with C having a single near-unit eigenvalue corresponds to ū, being (near) cointegrated of degree 1 (Aoki 1990, 17, 123, 222).

17. Nonzero μ_i values represent permanent biases—upward or downward-in the levels of transfer payments and contributions. They do not represent an equilibrium constraint.

18. The sense in which λ_t is unrelated to both $\tilde{\mathbf{u}}_t$ and \mathbf{x}_t is somewhat heuristic. For the precise meaning in the context of a completely linear model, see Martin and Thompson 1982.

19. For $j \in \{P, N, R\}$, the *i*th element of Δ_j is $\Delta_{ij} = E_t \partial [(\partial x_{it}/\partial j_t)/(x_{it}/j_t)]/\partial E_t$.

20. This is based on the conceptual substitution $EMN_t =$ $(CLF_t - U_t)/N_t$, where CLF_t is the size of the civilian labor force at t. The point is to think of changes in EMN, as being

produced by changes in U_t/N_t , holding CLF_t/N_t constant. 21. Using $EMN_t = (CLF_t - U_t)/N_t$, the fact that x_{1t} is directly affected by the lagged value UN_{t-1} implies that for February— December in each even-numbered year, E_i $\partial [(\partial x_{1i}/\partial UN_{i-1})]/\partial E_t \approx b_{148} - a_{12}b_{248} - a_{13}b_{348}$. The elasticity difference equals b_{148} only for January.

22. The similarities in Figure 1 reflect similarities among the coefficient estimates. See Table A-2.

23. Every \hat{a}_{ik} is more than three times bigger than its estimated standard error, except \hat{a}_{31} (s.e. .017) and \hat{a}_{32} (s.e. .022).

24. In an alternative specification in which y_t is defined with $y_{1t} = T_t$, $y_{2t} = 2PC_t$ and $y_{3t} = EC_t - PC_t$ (throughout the present data, $EC_t \ge PC_t$), the estimate for **A** is

$$\hat{\mathbf{A}} = \begin{bmatrix} .5978 & .0301 & -.0009 \\ -.0614 & .1427 & -.0005 \\ .5017 & -.6080 & .4623 \end{bmatrix}$$

In view of the principal difference between employer and personal contributions (see the text), these estimates suggest that the dependence of employer contributions on payments reflects constraints in supplemental state programs for unemployment insurance and workman's compensation, while the dependence of payments on personal contributions reflects constraints in Social Security.

25. So far as can be tested with the present data, the observed near cointegration is not due solely to the component that PC, and EC, should share because of the matched payments employees and employers make for Social Security (see, e.g., Brittain 1972). That is, near cointegration also appears if the model is estimated using the alternative specification described in n. 24.

26. The plotted "adjustments" are $\hat{y}_{it}^{1/2}$ $(\hat{u}_{it} - \hat{\mu}_i)P_{1984:1}$ 106/(N, P, 12).

27. For all j, Δ_j^{τ} remains approximately zero for all $\tau > 7$. 28. The standard error (first-order Taylor approximation) of the contemporaneous elasticity difference $\hat{b}_{148} - \hat{a}_{12}\hat{b}_{248} - \hat{a}_{12}\hat{b}_{248}$ $\hat{a}_{13}\hat{b}_{348} = -.082$ for unemployment in the transfer payments equation is .037. The standard error of the one-period-lagged difference for unemployment in the transfer payments equation (=-.083) is .045. The standard errors of b_{248} , b_{348} , b_{118} , b_{218} , and b_{318} can be found in Table A-2.

29. For employer contributions, the average per capita change is an increase of \$.14, versus an average per capita

contribution of \$31.09.

30. For an explanation of the interpretation in terms of

percent change, see Tufte (1974, 125).

31. For October and November of 1972, I compute \hat{y}_1 , equal to \$130.04 billion and \$133.95 billion, respectively. The monthly dollar figure used in the text is $10.96 = (130.04 + 133.95)/(2 \times 12)$.

32. Since transfer payments typically total approximately 1.5 times the total of personal and employer contributions (recall n. 15), the point is that $.0085 \times 1.5 = .01275$ is just smaller than the estimate of .0131 for PJF, for personal contributions equation but larger than the estimate of .0077 33. The monthly dollar values are computed using $\hat{\lambda}_{it}$ $\hat{y}_{it}^{1/2}/12$.

34. The coefficient estimates for PON, in Table 1 imply an October-November decrease in presidential election years of approximately .49% in personal contributions, and a corresponding increase of approximately .43% in employer contributions. Since these effects are small, I ignore them.

35. The S-PLUS® (Statistical Sciences 1991) programs used to compute the estimates are available on request.

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THE REVERSE COATTAILS EFFECT: LOCAL PARTY ORGANIZATION IN THE 1989 BRAZILIAN PRESIDENTIAL ELECTION

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Increasingly, it is said that the main determinants of electoral outcomes are class, ethnicity, and religion and that local political organizations occupy only marginal roles in national elections. I assess the effects of local party organizations in the presidential election of 1989 in Brazil. Given the long hiatus in competitive politics, the absence of any parties linked to the country's previous democratic experience, and the weakness of citizen identification with political parties, Brazil should be a textbook example of the collapse of local political organizations. The presidential candidates, however, acted as if party endorsements mattered, and in the context of Brazilian politics, it was rational for municipal mayors to trade blocs of votes for future local benefits. Applying a series of increasingly complex models to the vote shares of the leading candidates, I show that all candidates did significantly better in municipalities where the mayor represented their party. I also show that spatial factors affect the tactics of local politicians, and I distinguish charismatic from purely organizational components of support.

s democratization sweeps over formerly authoritarian regimes in the Third World and in Lithe former Soviet bloc, elections move to the center of the political stage. For students of politics in these new regimes, the primacy of elections presents an intellectual challenge. What will be the main factors determining electoral outcomes? Will class, ethnicity, and religion—the usual determinants of electoral results in advanced industrial countrieshave the same weight in the newly competitive nations? If the experience of many industrial and industrializing nations, especially in Latin America, foreshadows the political futures of the nations in transition, local political organization will join class, ethnicity and religion as determinants of national-level electoral contests. By the term *local political* organization I refer not to affective ties linking citizens with their communities but, instead, to the efforts of a community's political leaders to deliver blocs of votes to candidates the leadership prefers. In Colombia, for example, two dominant parties have long relied on an extensive clientelist network to link local political machines to the national state (Buitrago and Dávila 1990). In Mexico and Venezuela, local bosses "mobilize their clienteles to support a political party in rallies, demonstrations, and elections in exchange for favors from higher government and party officials" (Coppedge 1993, 262). In Italy, "the key to Christian Democratic power in the major southern cities has been . . . control of the municipio" (Chubb 1982, 78). Local politicians in Japan gather votes for Diet candidates by persuading constituents that good relations with their Diet members are a prerequisite for the continuation of subsidies and grants-in-aid from the central government. Success at generating support for the Diet candidate yields financial aid for the local politician's own election campaign (Curtis 1971, 44-45).

How can we evaluate the potential of local political ·

organizations in the electoral processes of formerly authoritarian regimes? First, if local organizations are truly significant actors in national politics, they ought to affect electoral results in *supra*local contests, for example, in presidential elections. Second, if local organizations exist apart from longstanding voter loyalties, the effects of the machine ought to be clear even where party identification is weak. Finally, if local organizations have staying power in competitive polities, they should matter even when politicians use modern marketing techniques to reach voters directly.

I shall focus on just such a case, the Brazilian presidential election of 1989. Consider the background of the election. The 1989 contest was the first national decision since 1960, and less than one-third of the electorate had ever voted in a national election.² None of the political parties contesting the 1989 election had been around in 1960, and most of the contending parties were less than two years old.3 The Brazilian Democratic Movement party (PMDB),* the nation's largest party, had ridden the initial success of its emergency economic program to overwhelming victories in the 1986 congressional and gubernatorial races, but the program collapsed soon after, and the popularity of the PMDB fell so drastically that half its congressional delegation deserted to form new parties or join old ones. In public opinion surveys, the percentage of the population agreeing that in its own vote choice, "the candidate's party is the decisive factor" had declined from 43% in 1982 to 24% in 1986 and to 18% in 1988 (Muszynski and Mendes 1990, 64).

As the election unfolded, the weakness of the parties seemed pathetic. Officially, the race included 22 candidates. Almost from the beginning of the campaign, the leader was Fernando Collor de Mello, a little-known governor from the politically and eco-

^{*} All party acronyms are listed in the Abbreviations.

nomically marginal state of Alagoas. Backed by the National Reconstruction party (PRN)—a party created solely for the election—Collor's campaign focused on his alleged achievements in rooting out Alagoas's marajás, the vastly overpaid "maharajas" of the civil service. His campaign team, for the first time in a Brazilian presidential election, made extensive and effective use of television.

The election took place in two rounds. In the first round Collor and the runner-up, Luís Inácio Lula da Silva of the Workers' party (PT), jointly received 48% of the vote. The parties backing these two candidates held less than 5% of all seats in the Chamber of Deputies, while two other candidates, whose parties held 61% of the seats, received only 5% of the vote. Though the losing parties of the first round had little sense of the true preferences of their partisans, they could not avoid endorsing one of the two finalists. After a campaign bitter enough to embarrass an American marketing consultant, Collor defeated Lula 53% to 47%. The new president remained in office until his impeachment and conviction on corruption charges in 1992.

Brazilian political analysts interpret the 1989 election as essentially plebiscitary: parties, in the conventional wisdom, failed in their attempts to mobilize or sway voters.4 The presidential candidates disagreed with this interpretation, however, at least insofar as their campaign behavior reveals an underlying sense of the electoral process. In the months before the election every candidate worked at winning the endorsements of governors, senators, federal deputies, and local mayors. One of the country's leading newspapers, the Folha de São Paulo, provided frequent updates of its adesómetro (endorsement meter), a thermometer recording pledges of support announced by various national and local politicians. Newspapers in every state updated who-endorsedwhom stories. Candidates, in other words, thought politicians and their organizations still had the power to influence voters.

Were the presidential candidates simply fighting the last election? Were they failing to realize that the old politics of local bosses and local organizations no longer mattered? Or were the candidates right? Did local parties matter even when television had the power to catapult unknowns to national prominence?

In the context of Brazilian politics, the endorsement-seeking tactics of the presidential candidates, at least insofar as they focus on local officials, are perfectly rational, because the candidates know that municipal leaders have every reason to remain in the good graces of the future president. Municipalities (the basic unit of Brazilian government below the state level) desperately need the transfer payments that the central government makes available. In sanitation, school construction, social assistance, and roads, central government transfers are absolutely indispensable. Poor and rural municipalities are the most dependent, but even big cities need federal help. A very large share of these transfers—perhaps as much as 50%—is discretionary, and the central government relies heavily on political criteria in signing the expenditure transfer agreements known as convênios. In 1990–91, for example, municipalities in the home states of President Collor (Alagoas), the education minister (Rio Grande do Sul), and the health minister (Paraná) all received a significantly greater number of transfer agreements than municipalities in other states. Thus any sensible local leader, needing central government money and knowing that political criteria traditionally determine the distribution of such monies, would try to deliver votes to some candidate.

Endorsement seeking in Brazil has a second rationale: politicians know that strong local machines have the tools to influence the choices of voters. Naturally a machine leans on its patronage employees for support. Government employees constitute between 5% and 10% of the economically active population in the whole country (far more in some municipalities), and voters whose livelihoods depend on local government will surely support their leaders.7 Local leaders also influence voters well beyond the narrow circle of friends and dependents. The local machine might disrupt the rallies and meetings of the opposition. It can hinder the attempts of the opposition to distribute electoral advertisements or gifts to voters. The machine can influence local newspapers and radio and television stations to slant news so as to benefit its favorites. Indeed, radio and television licenses were themselves often granted as political rewards to influential local politicians. These activities constitute not vote fraud (which, by 1989, had become quite unlikely) but simply a continuation of ordinary local government political activity.

If the presidential candidates behaved rationally in seeking the endorsements of state and local leaders, the question remains: Was the outcome of the 1989 election "politics as usual"? In other words, did organization matter?

No research has been undertaken directly on this topic, principally, I believe, because appropriate data bases have been unavailable.8 I have created such a data base, and in this essay I construct and evaluate a model assessing the ability of local party organizations to mobilize voters for the presidential election. The analysis suggests that party endorsements and the mobilizational commitments they represent did indeed matter. In fact, they had a powerful impact on the fortunes of all the major candidates in the Brazilian presidential election. Although Fernando Collor de Mello probably would have won even if local machines had remained truly neutral, the Collor phenomenon was almost certainly exceptional, and in a normal election local organization will remain critical. Rumors of the death of parties, in other words, have been greatly exaggerated.

I first present some background necessary to understand the election itself, then develop and test a series of models linking local organizational backing to candidate support in the two rounds of the election. These models successively increase in complexity and

realism. Finally, I extend the modeling exercise and consider the overall significance of the results.

THE PRESIDENTIAL ELECTION OF 1989

Though the 1989 election was Brazil's first popular presidential competition since the military seized power in 1964, the outgoing president was a civilian, not a general. In 1984 Tancredo Neves had beaten the military's preferred candidate in a contest conducted wholly within an electoral college, a college composed of various elected and appointed officials. When Neves died soon after the election, his vicepresident, conservative northeasterner José Sarney, assumed the presidency. Though Sarney had his ups and downs, by the end of 1988 he was widely unpopular, and all contenders in the 1989 campaign attacked his presidency. Sarney's ministers endorsed various candidates, but the president backed no one, and his administration held to a position of official neutrality.

The Politics of Structure and Geography

Brazil is truly a federal system. States have been important actors in Brazilian politics since independence. Governors of economically significant states immediately become major players in national politics, and most congressional deputies reflect state and local interests. Such legislative localism is largely a product of Brazil's electoral system, its unique version of open-list proportional representation. In all open-list proportional representation systems, voters cast ballots for individual candidates. The number of seats a party receives is basically determined by a simple ratio: the sum of all votes cast for the party's candidates divided by the sum of all votes cast for candidates of all parties. The identity of the holders of the seats is then determined by ranking the candidates by the number of individual votes received. The Brazilian version of open-list proportional representation adds some features not found elsewhere: whole states serve as at-large congressional districts; state-level conventions select candidates; and, by law, parties cannot refuse to renominate incumbent deputies.

The results of open-list proportional representation in Brazil are predictable: a proliferation of weak, ideologically incoherent parties; a focus on personalized politics; and a president usually bereft of solid legislative backing. Desperate to attract congressional support, Brazil's presidents parcel out cabinet positions to powerful state leaders in the hope that the latter will influence their states' delegations. Not surprisingly, these ministers manipulate projects and patronage jobs to advance their own political futures. President Sarney, for example, appointed Antonio Carlos Magalhães, former (and current) governor of Bahia, to be minister of communications. Magalhães funneled radio and television licenses to backers of

the president (many of them deputies) and to supporters of his own political ambitions.

The central government distributes pork primarily to municipalities and secondarily to states. As a tool for attracting *legislative* support, pork is rather blunt, because real legislative districts (i.e., the actual areas in which deputies campaign) are usually neither whole states nor single municipalities. In other words, deputies care little about statewide pork, because they would have to share credit with the rest of their delegation. And though most deputies focus their campaigning on small groups of municipalities, few of the resulting bailiwicks are so secure that they resemble U.S.-style single-member districts (Ames 1992). As a result, the president has to persuade mayors to persuade their deputies to repay presidential generosity. Sometimes it works, sometimes not, but the overall price, in terms of efficient allocations of government spending, must rise. 10

Region is also crucial to the Brazilian equation. The Northeast, composed of nine very poor states, has long been dominated by conservative politicians and political bosses. Although in certain states clientelistic politics has noticeably declined, traditional styles of politics maintain their grip in much of the region, and large portions of the rural Northeast survive only as a result of federal transfers. Here politics is a business, sometimes the only prosperous business. Generally supportive of Brazil's political Right, the Northeast (along with the North and Center–West) benefits from the malapportionment of the national legislature. 11

The South and Southeast regions, by contrast, include the bulk of Brazil's industry and most of its Left and Left-Center electoral strongholds. Deputies in the South and Southeast are no less pork-oriented than northeasterners—but their vote bases are competitive, rather than monolithic, and they are substantially more accountable to their electorates.

Parties and Candidates

The results of the first round of balloting are shown in Table 1. My empirical analysis focuses on five of the first seven finishers: Fernando Collor de Mello, Luís Inácio Lula da Silva, Leonel Brizola, Paulo Maluf, and Ulysses Guimarães. ¹² I have eliminated Mário Covas, who finished fourth, because his Brazilian Social Democratic Party (PSDB) controlled very few municipalities. Afif Domingos of the Liberal party (PL) had a few more votes than Ulysses, but Afif was almost never an important player in the campaign, and his party was minuscule compared to the PMDB. Aureliano Chaves represented the Liberal Front party (PFL), a major party, but his campaign never got off the ground, and most PFL mayors abandoned his candidacy well before the first round of voting. ¹³

In order to understand how local party organizations affected the 1989 election, readers unfamiliar with the Brazilian political scene need a brief introduction to the chief parties and their candidates.

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ACSUITS OF ROUND OF	16 01 1707 1168	duchtial Election
CANDIDATE AND PARTY	VOTES	PERCENTAGE
Collor, PRN	20,611,011	28.52
Lula, PT	11,622,673	16.08
Brizola, PDT	11,168,228	15.45
Mario Covas, PSDB	7,790,392	10.78
Maluf, PDS	5,986,575	8.28
Afif, PL	3,272,462	4.53
Ulysses, PMDB	3,204,932	4.43
Freire, PCB	769,123	1.06
Aureliano, PFL	600,838	.83
Caiado, PSD	488,846	.68
Camargo, PTB	<u>379,286</u>	52
Total	72,280,909	91.16

Results of Round One of 1989 Presidential Election

Source: Tribunal Superior Eleitoral: Coordenação Geral de Informática. Brasília, DF: Brasíl.

Note: Eleven candidates, each with less than .52% of the vote, have been eliminated from this table. All the electoral data in this and subsequent tables come from the tribunal. Throughout the analysis, the percentage of votes cast for the candidate is the fraction of valid votes (i.e., votes for candidates minus blank and null votes).

Ulysses Guimarães and the PMDB. The Brazilian Democratic Movement party had originated as the Brazilian Democratic movement (MDB), the officially tolerated opposition party during the military regime. Increasingly independent and critical of the dictatorship, its legislative candidates began to mount a serious challenge in the election of 1974, especially in the more developed South and Southeast. In the 1970s and 1980s, the PMDB grew enormously, spreading from its original base to the whole country. With the legalization, in the 1980s, of multiparty politics, competitors on the left and right flanked the PMDB, and it lost most of its ideological coloration. 14 In 1984, the electoral college chose a PMDBista president, but Vice-President José Sarney, who assumed the presidency upon Tancredo Neves's death, was a northeasterner representing the much more conservative PFL. In 1989, the PMDB selected Ulysses Guimarães as its candidate for the presidency. As the official leader of the PMDB, Guimarães commanded wide respect, even reverence, for his struggle to reestablish Brazilian democracy. A 73-year-old Paulista, Guimarães was a skilled legislator, a moderate social democrat, and a lackluster television orator.

Leonel Brizola and the Democratic Labor Party. Leonel Brizola's political career is the stuff of legends. ¹⁵ A founder of the Brazilian Labor Party (PTB) in 1945, in the 1950s he had been federal deputy and governor of Rio Grande do Sul, a dynamic state in the South. Moving in 1962 to the state of Rio de Janeiro (then called Guanabara), Brizola became an active figure in an already well established PTB and gained a seat as federal deputy once again. In 1964, the military regime, angered by Brizola's radical activities and powerful populist speeches, forced him to leave the

country. He returned in 1979 as part of a general amnesty, and in 1982 Rio's voters elected him governor, this time on the ticket of the Democratic Labor party (PDT), a party he had formed when the electoral courts refused him the old PTB label. Brizola's national ambitions have always foundered on one huge obstacle: though he commands enormous popular support in Rio Grande do Sul and Rio de Janeiro, he has never been able to penetrate the state of São Paulo. In Rio de Janeiro and Rio Grande do Sul Brizola received 50% and 61% of the votes, respectively, in the first round of the 1989 election. In São Paulo, a state with more than one-fifth of the Brazilian electorate, he received less than 1.5%. ¹⁶

Labeling Brizola ideologically is difficult. His socialism is certainly less doctrinaire than Lula's, but Brizola's thinking—often labeled populist—contains both xenophobic and statist elements.

Luís Inácio Lula da Silva and the Workers' Party. Even in the cacophony of Brazilian politics, the PT stands apart (see Keck 1992; Sader and Silverstein 1991). As the largest ideologically militant party in Brazil, it represents traditional socialism. Riven by internal disputes, the party is nonetheless highly disciplined in the federal Chamber of Deputies. Not only does a significant fraction of PT deputies come from working-class organizations, but Lula, the party's head, began as an industrial worker and union organizer. Though supported by intellectuals, especially university professors, its working-class electoral base is substantial, particularly in industrial states like São Paulo. In her study of the 1989 election in the paulista municipality of Presidente Prudente, Kinzo found Lula to be the only candidate with a largely classbased appeal (1992, 59). As Keck points out, Lula's campaign called for a socialist transformation of Brazilian society, while stressing the need for national reconciliation and the formation of a national popular government (1992, 159).

Paulo Maluf and the Democratic Social Party. Its name notwithstanding, the Democratic Social party (PDS) is a right-wing party. Recall that the military regime had permitted the creation of an official opposition party, the MDB, which became the PMDB. The military also created a progovernment party, the National Renovating Alliance—ARENA. As the democratic transition unfolded, ARENA became the PDS. Its presidential candidate, Paulo Maluf, had been appointed governor of São Paulo by the military regime in 1979. A politician of legendary memory and deal-making skill, Maluf enjoyed considerable popularity in the city of São Paulo (which elected him mayor in 1992), and his PDS controlled hundreds of municipalities across the nation.

Fernando Collor de Mello and the National Reconstruction Party. The National Reconstruction party (PRN) was nothing more than a vehicle for Collor's ambitions. Raised in a traditional political family (his father had been governor and an uncle had been a federal

minister in the 1930s), Collor hailed from Alagoas, an economically and politically marginal state with a reputation for exceptionally corrupt politics (Mello 1992). The military appointed Collor mayor of Maceió, Alagoas's capital, and he subsequently won election as federal deputy and governor. Lula derisively referred to Collor as the "lap dog" of the military, but Collor ran for president as a populist reformer, criticizing the Sarney administration and trumpeting his own record in Alagoas as a corruption fighter. Collor was aided by his youth (he was barely forty at the time of the campaign) and his good looks. The PRN was his fifth political party. His career had already passed through ARENA, the PDS, the PMDB, and the Party of Youth. The PRN was not much of a party, however: at the time of the campaign, it controlled outright only three municipalities and had only 14 federal deputies.

The Course of the Campaign

At the end of 1988 and the beginning of 1989, Lula and Brizola led all opinion polls, and the election shaped up as a battle between these two leftists. But around the end of March, Brazilians began to witness the fenômeno Collor—a surge in the polls that carried Fernando Collor de Mello past the field and ultimately to about 40% of the electorate, more than the other candidates combined. 17 Although Collor finally slipped to a bit below 30%, no one seriously challenged him, and in its last six months, the campaign was really about second-place. Lula finally squeezed in, victorious over Brizola by less than five hundred thousand votes.

With only a month between the first and second rounds of the election, the struggle for the endorsements of the losing parties soon reached fever pitch. A group of PMDB deputies quickly endorsed Lula, but Orestes Quércia, the governor of São Paulo and a fierce opponent of Lula, demanded that the party remain neutral. Other PMDB governors fell in behind Quércia, and the PT itself proclaimed that no politician associated with the discredited Sarney administration (read PMDB) could appear with its candidates. So the PMDB endorsed no one. The PSDB, by contrast, agonized for awhile, then granted "qualified" support (apoio crítico) to Lula. Lula also picked up some unqualified support: Brizola threw his PDT enthusiastically behind the man who had so narrowly kept him from the final round.

On the right, alliances were simpler. The PFL and PTB strongly endorsed Collor. Various minor rightwing parties soon followed suit. Maluf's PDS joined the Collor brigade, although a few PDSsistas, especially in Brizola's stronghold of Rio Grande do Sul, went over to Lula. After two nationally televised debates and intense television advertising (the lowpoint coming during a Collor television program when Lula's ex-wife denounced him as a bad father and a racist), Collor defeated his PT rival, 53% to 47%.

Mayors and Local Machines

Brazil's mayors had all assumed office in 1988. Previously, the military regime had imposed officials on large cities and on certain other municipalities it classed as "strategically important," so the 1988 election was the first since the military takeover in which all municipalities had the right to elect their own mayors and councils. Nineteen different parties elected mayors in 4,000-plus municipalities. The major parties (PT, PDT, PMDB, PSDB, PDS, PTB, and PFL) elected a total of 3,164 mayors. In 1,787 cases, the victor had no formal support from other parties; in 1,377 cases, a formal coalition of two or more parties supported the winner.18

Did mayors use their influence to boost the candidate of their party, or did they desert to someone else? Overwhelmingly, mayors remained loyal. 19 In the first round, mayors from the PT (Lula), PDT (Brizola), and PDS (Maluf) almost never deserted. Even though Ulysses Guimarães' candidacy was in desperate trouble months before the actual vote, few PMDB mayors abandoned him, at least publicly. Most mayoral desertions came from the PTB and PFL. No one, including the candidate himself, took the PTB's campaign seriously, and PFLista Aureliano Chaves's candidacy foundered from the very beginning. Of those PTB and PFL mayors who abandoned their candidates, the largest number publicly en-

dorsed Fernando Collor de Mello.

The ability of a mayor to put the machine's muscle behind a candidate depended, in part, on the mayor's own freedom of action. If a coalition, rather than a single party, had backed the winning mayor, the members of the supporting parties would likely object to the mayor's helping a presidential candidate they did not support. Moreover, successful coalitions usually elected members of the municipal council as well, so that the mayor was likely to face council opposition to electioneering efforts. As a result, presidential candidates are likely to gain less in municipalities where a coalition supported the mayor, as compared to municipalities where the mayor won without allies.

MODELS OF PARTY ORGANIZATION AND PRESIDENTIAL SUPPORT

Let us begin with a "stupid" model, that is, a model regressing the candidates' vote shares on dummy variables representing the party (or parties) in control of the municipality.²⁰ These dummies are scored 1 if the mayor or coalition came from a given party and 0 if the mayor or coalition came from some other party (including parties with no serious presidential candidate). The results, presented in Table 2, can be interpreted as the number of percentage points a candidate gains in municipalities with mayors or mayoral coalitions of each party.

The simple model is encouraging. Lula does well in PT municipalities and badly elsewhere (except in the

PARAMETER	LULA (PT)	COLLOR (PRN)	BRIZOLA (PDT)	MALUF (PDS)	ULYSSES (PMDB)	LULA (2 (PT)
Intercept	17.66**	30.95**	27.11**	4.88**	4.06**	54.38**
PT	12.39**	-8.89**	-23.00**	12.16**	-1.93**	-2.78
PDT	5.82**	11.39**	13.98**	3.24**	23	2.85*
PMDB	91	5.60**	-11.61**	2.88**	4.63**	-11.96**
PSDB	9.52**	-2.42	-17.77**	5.02**	26	-3.97
PDS	-3.61**	7.60**	-10.20**	6.03**	2.37**	-14.84**
PTB	-1.95	13.91**	-21.31**	6.91**	.32	-20.40**
PFL	.71	15.33**	-19.07**	2.41**	1.93**	-18.43**
Coalitions						
PT	-2.78**	-14.21**	-16.76**	16.55**	-2.83**	-6.63**
PDT	26	-1.06	-2.15	2.45**	.54	-1.80
PMDB	4.68**	7.69**	15.49**	.74	3.44**	-9.57**
PSDB	15.95**	-13.03**	-18.55**	-1.80	56	11.81**
PDS	-2.57**	4.21**	-11.39**	6.02**	1.69**	-12.17**
PTB	3.10**	2.83*	-13.14**	5.34**	.12	-6.92*
PFL	5.87**	10.68**	-17.54**	.74	1.13*	-11.46**
R ²	.14	.23	.17	.26	.11	.14
F	43.24	76.97	52.38	90.35	31.07	43.16
р	.0001	.0001	.0001	.0001	.0001	.000

few cases where the mayor was a PSDBista). Brizola gains only where his PDT ruled by itself, and Ulysses Guimarães gains the most in PMDB municipalities. In the second round, Lula loses badly everywhere except in the municipalities of the Left, but only in PDT municipalities does he really do well. Brizola, in other words, delivers votes to Lula in even larger numbers than the PT's own "machine." The only real anomaly is Maluf, who seems to do well everywhere, regardless of party. Maluf's results may well stem from his popularity in urban centers of the South, but we shall have to await elaboration of the model.

This model is "stupid," of course, because it leaves out socioeconomic and demographic factors. Even in a polity where few parties occupy well-defined ideological positions, candidates appeal to different social groups. Since these groups distribute themselves unevenly across municipalities, municipal-level results should differ even if party machines are nonexistent.

With their populist and statist appeals, Lula and Brizola clearly appealed to big-city voters, especially to working- and lower-class groups and to government employees (who concentrate in big cities). Collor's strength was greatest in small towns and rural areas, and, being a candidate with no party, he did well among newly arrived residents, immigrants who lack ties to old political machines. Maluf was a conservative candidate with considerable appeal in the capital city of São Paulo, while Ulysses Guimarães' candidacy lacked any clear, group-oriented content.

The only comprehensive source of Brazilian socio-

economic and demographic data is the census of 1980.²¹ By 1989, the country had changed, especially through the shifting of population from rural to urban areas. I therefore substituted the size of the registered electorate, a datum collected in 1988, for the census measure of the percentage of the population living in urban areas.²² Given the spatial distribution of Brazilian population and the absence of a distinction between cities and counties, measures of urbanization correlate closely with measures of total population, and the more recent population figures minimize any overweighting of rural areas.²³

With electoral results aggregated at the municipal level, any inferences about the voting behavior of particular cohorts within these municipalities risk the dreaded "ecological fallacy." Here, however, the ecological fallacy is less troublesome, since I seek merely to account for the cross-municipality variance in voting patterns. My interest lies in political machines: to measure their effects, the social and demographic factors affecting municipal-level outcomes are held constant.

Table 3 presents the results of the "less stupid" model. The socioeconomic and demographic variables produce the expected effects. Lula and Brizola do better in larger municipalities. Lula, Brizola, and Maluf gain votes in areas with more industry. Collor does well in areas with more agriculture and more migrants. And leftists Lula and Brizola are more successful than "corruption fighter" Collor where government employees are more numerous.

With the inclusion of socioeconomic and demographic variables, the effects of party machines

PARAMETER	LULA (PT)	COLLOR (PRN)	BRIZOLA (PDT)	MALUF (PDS)	ULYSSES (PMDB)	LULA (2) (PT)
Intercept	31.28**	44.94**	11.17**	-7.39**	9.25**	49.37**
PT .	14.83**	-2.69*	-16.31**	05	1.76**	22
PDT	-4.89**	-14.52**	21.85**	1.47**	.13	10.03**
PMDB	-1.00	-3.29**	-1.64	1.10**	3.18**	82
PSDB	6.97**	-5.80*	-4.75	58	1.81	2.98
PDS	-2.22*	-1.95	-3.04	5.32**	07	-3.62*
PTB	06	5.60**	-9.56**	1.98*	81	7.02**
PFL	.59	3.88**	-7.62**	1.60**	42	-5.10**
Coalitions						
PT	1.97	-11.38**	16.52**	-6.43**	1.33*	11.37**
PDT	-1.03	-5.42**	7.66**	95	.63	5.53**
PMDB	3.44**	47	-6.84**	.75	1.92**	76
PSDB	15.90**	-4.74**	-29.02**	41	1.61*	.79
PDS	-2.20**	-2.69**	-1.00	2.76**	.92*	1.89*
PTB	2.00*	-1.62	-3.34*	1.28*	.26	.81
PFL	5.06**	1.45	-8.98**	.96	82	-1.50
Voters (millions)	4.04**	-7.86**	30.50**	-16.10**	.57	23.40**
Voters ²	54**	1.57**	-5.80**	2.78**	09	4.30**
% in manufacturing	7.85	-68.45**	48.32**	31.85**	-9.44**	49.82**
% government employees	19.06	-80.39**	121.50**	1.64	-45.14**	100.89**
% in agriculture	04**	.02**	.01**	.02**	.00**	05**
% migrants	.00	.04**	05**	.02**	−.03**	04**
Per capita income (\$1,000s)	-2.51**	-1.62**	−.53 *	2.86**	−.51**	98**
R ²	.26	.48	.35	.55	.26	.47
F	59.25	159.7	90.2	207.2	59.38	151.2
р	.0001	.0001	.0001	.0001	.0001	.0001

sharpen. Lula continues to do well in PT municipalities, and he no longer loses support when the PT enters into a coalition.²⁴ Collor now gains only in municipalities controlled by the PTB or PFL—the two parties whose candidates were so weak that mayors abandoned them in droves. Brizola, as before, gains only in PDT municipalities, but his gain is almost 50% higher with the socioeconomic and demographic variables included. Maluf's support changes dramatically: in the more complex model, he gains major support only on the right, with by far the biggest gains coming from his own PDS-controlled municipalities. Ulysses Guimarães' party support also sharpens: only in PMDB and PT municipalities does he gain, and the PMDB contribution is twice that of the PT. Finally, in the second round, Brizola's PDT contributes heavily to Lula, Lula's own PT helps not at all, and the right-wing parties consolidate around Collor.

What does this model lack? The answer, as any "Michigan" scholar knows, is partisan tendency. Suppose we operationalize partisan tendency purely as party identification. The PT, for example, commands loyalties rivaling many long-established European parties. Overall, however, few parties competing in 1989 had been around very long, and none of

the major parties had ever participated in a presidential election. As a result, party identification is not measurable in terms of loyalties spanning multiple elections. Still, despite Brazilian parties' short histories, weak organizational structures, and ideological incoherence (at least as compared to European parties), voters may develop habitual attachments either to a particular party or to a general ideological direction.

Socioeconomic conditions and political organizations, both historic and current, create habitual attachments. At the moment of casting a ballot, the voter finds the party whose program is closest to his or her own preferences. Closeness, of course, is subjectively perceived, and both the past and present power of a municipality's reigning political organizations mediate the voter's perceptions. Can socioeconomic and organizational effects be separated? First, we should seek a general left-right ideological direction, rather than identification with a particular party, as a measure of partisan tendency. By including various parties, the capture of a general left or right tendency minimizes the effects of loyalties to any given organization or candidate. And even if the indicator is a bit broad, its inclusion can only reduce the effects of current party organization-in other

words, the estimator of party organization ultimately generated will be the minimum possible. Second, we should base our estimate of partisan tendency on municipalities where no single organization dominates, that is, on municipalities with considerable interparty fragmentation. In such competitive municipalities, voters are less subject to the coercion of a dominant machine.²⁵

Measuring Partisan Tendency

In the absence of survey responses, any measure of partisan tendency must rely on actual electoral outcomes—in this case on the congressional elections of 1978, 1982, and 1986. Voters cast their ballots for individual candidates (under the rules of open-list proportional representation). I aggregated, for each election, the votes of all the candidates from each party in each municipality. In other words, the analysis began with the total ARENA vote in each municipality for 1978, the total PMDB vote in each municipality for 1986, and so on. Then, selecting in each state just those municipalities with interparty fragmentation greater than the state's median, I regressed these party vote totals on the socioeconomic and demographic variables.²⁶ The coefficients from these equations then generated predicted party totals for all municipalities, fragmented and unfragmented, by state and by year. In effect, this technique assumes that in more fragmented, competitive municipalities dominant machines have less influence over voters' partisan tendencies. It also assumes that the socioeconomic and demographic variables would create the same partisan tendencies in dominated municipalities if a more competitive political environment let voters express themselves.

These predicted party votes were then aggregated into left and right tendencies. In 1978 and 1982 (elections in which the military regime allowed only two parties), the Left is the MDB and the PMDB, respectively, and the Right is ARENA and the PDS. In 1986, the Left is the PT plus the PDT and the Right is the PFL plus the PDS. Of course, some fuzziness remains: Is the MDB really on the left? Can the PMDB be called a left-wing party? Lacking a perfect resolution of these ambiguities, I chose to establish a range of estimates: a minimum left-maximum right estimator on one side and a maximum left-minimum right estimator on the other. The minimum left estimator utilized the smaller of the predicted 1978 MDB and 1982 PMDB votes; maximum left utilized the larger.²⁷ The maximum right estimator was the largest of the three predictions: 1978 ARENA, 1982 PDS, and 1986 PFL-PDS. Minimum right equalled the smallest of the three.

Given this range of partisan tendencies, Table 4 presents estimates of the model with the indicator of the *largest* possible Left, while Table 5 includes the indicator of the *smallest* possible Left. ²⁸ The results show that partisan tendency matters just where it ought to matter. For Lula, Brizola, and Maluf—three candidates whom voters could locate ideologically—

the partisan-tendency coefficients are mostly in the expected directions, with high significance levels. Partisan tendency makes no difference either for Collor, who seemed all over the left-right space, or for Guimarães, whose position was essentially centrist. Although our partisan-tendency measures had consistently strong effects on the ideological candidates, some signs are inconsistent. With partisan left tendency estimated at its maximum, increases in left tendency led to more votes for Lula and Brizola in round 1 and for Lula in round 2-just what we expect. But with left tendency estimated at the minimum of its plausible range, increases led to big gains for Lula in round 1, a lower vote for Brizola, and—the real surprise—a sharply lower vote for Lula in round 2. Do these results make sense?

The key lies in Brizola's vote in round 1 and in Brizola's ability to deliver votes to Lula in round 2. Unlike Lula supporters, Brizolistas are mostly not strong left partisans. So when left tendency is estimated conservatively, increases help Lula but hurt Brizola. With left tendency estimated generously, further increases help both candidates, but the bigger gain goes to Lula. In the second round, Brizola delivers his nonideological voters to Lula, so Lula appears to be hurt by increases over the minimum left estimate.

If this explanation is correct, the model ought to be better behaved when reestimated without Rio Grande do Sul and Rio de Janeiro, Brizola's two strongholds. This is precisely what happens. With these two states removed, the effect of increasing left partisanship is now more consistent: increases in partisan left tendency from its maximum produce gains of about four points for Lula; increases from minimum partisan left tendency neither help nor hurt.

With underlying partisan tendency now included in the model, what happens to our party variables? The results confirm the overall importance of local organization. In the first round, every candidate, from the strongest to the weakest, received a big boost in municipalities headed by a mayor from the candidate's party. Collor, the candidate with no party, clearly managed to pick up support from PTB and PFL mayors; the failure of their standard-bearer had put PTB and PFL mayors in a desperate position. Maluf, too, got some support from PTB and PFL organizations, but Maluf relied primarily on his own PDS machine. Remarkably, Ulysses Guimarães kept a portion of his local PMDB support, even though his candidacy foundered six months before the election. Guimarães' supporters may have done little to help him (the PMDB coefficient is the smallest of any candidate's own party), but at least they rarely deserted to other candidates.29

Party organization affects the results in the second round as well, but here the situation becomes a bit more complicated. The PDS, PTB, and PFL all went massively for Collor. Brizola made good on his endorsement of Lula, delivering more votes in PDT (and PDT-in-coalition) municipalities than any other

TABLE 4

Presidential Support with Socioeconomic and Demographic Variables and Partisan Tendency: Left at Maximum of Plausible Range

PARAMETER	LULA (PT)	COLLOR (PRN)	BRIZOLA (PDT)	MALUF (PDS)	ULYSSES (PMDB)	LULA (2) (PT)
Intercept	29.94**	45.49**	10.24**	-6.55**	9.14**	48.09**
PT	14.86**	-2.71	-16.30**	07	1.78**	19
PDT	-4.98**	-14.49**	21.77**	-1.41*	.13	9.94**
PMDB	-1.09	-3.25	-1.72	1.17**	3.19**	91
PSDB	6.76**	-5.71 *	-4.92	44	1.81	2.77
PDS -	-2.47	-1.85	-3.24	5.48**	07	-3.86*
PTB	24	5.67**	-9.72**	2.11**	81	-7.19**
PFL	.47	3.93**	-7.73**	1.68**	41	-5.22**
Coalitions		•				
PT	2.47*	-11.58**	16.87**	-6.74**	1.37	11.84**
PDT	-1.83*	-5.09**	7.08**	44	.59	4.77**
PMDB	3.37**	44	-6.92	.80	1.93**	83
PSDB	15.76**	-4.68**	-29.13**	31	1.61*	.65
PDS	-2.26**	-2.66**	-1.06	2.80**	.93*	-1.95*
PTB	2.03*	-1.63	-3.33*	1.26*	.28	.85
PFL	5.00**	1.46	-9.05**	1.00	78	-1.55
Voters (millions)	3.54**	−7.66**	30.19**	15.74**	.53	22.89**
Voters ²	49**	1.55**	-5.79**	2.74**	08	-4.24**
% in manufacturing	4.85	-67.21**	46.18**	33.76**	9.68**	46.94**
% government employees	-15.07	-66.31*	97.08*	23.31	-47.82**	68.19*
% in agriculture	04 * *	.02**	.02**	.02**	.01**	04 **
% migrants	00	.05**	05**	.03**	−. 03**	05**
Per capita income (\$1,000s)	-2.50**	-1.62**	-5.26*	2.86**	-5.11**	98**
Maximum partisan left tendency	5.11**	-2.10	3.64*	-3.24**	.38	4.88**
R ²	.27	.48	.35	.55	.26	.47
F	58.84	152.7	86.51	201.1	56.68	145.9
р	.0001	.0001	.0001	.0001	.0001	.0001

Note: N = 3,611.

 $p \le .05$. $p \le .01$.

party delivered. The PT, however, seems to have been unable to contribute any positive increment to its own candidate. Why does the PT appear so ineffective? Perhaps its weakness is not so surprising. The PT mayoralities were all big cities. Nowhere did the party have the kind of dominant organization typical of a small town or rural machine. Moreover, the PT's rise to power in local government was very recent. Indeed, until 1988, there were no PT mayors. The PT was bereft of the ties to newspapers and radio and television stations that other machines profited from financially and manipulated politically. Finally, the PT's experience in governing its cities had been rocky. Its ascendancy to power raised expectations that it could not meet, especially given the hostility of other national power holders. Overall, local PT organizations lacked the dominance, the mechanisms of control, and the interparty alliances typical of other

Still, before writing off the PT as a possible mobilizer of local voters, let us return to the issue of Brizola, his concentrated vote, and his pro-Lula mobilization. In the reestimated model, with Rio Grande do Sul and Rio de Janeiro removed, Lula gains about four percentage points in PT municipalities. He *loses*

about four-and-one-half percentage points in PDT municipalities. So outside the highly politicized, PDT-dominated states of Rio de Janeiro and Rio Grande do Sul, the PT could deliver a few votes, after all. Moreover, Brizola's influence fell off sharply outside his traditional bases.

Did organizational power affect the overall outcome? At one level, this query is unanswerable: if local organization had not existed, candidates would have adopted alternative strategies. But it is possible to calculate the overall gains and losses each party produced for its candidate.30 Without the party machines, Brizola would have lost to Lula by a bit more than the actual result. In the second round, Collor added to his advantage with the strength of his right-wing allies, but he would have edged Lula anyway. Thus one can say that in this particular election organizational effects did not change the outcome. But if local machines had not influenced blocs of voters, then preelection polls, especially in the second round, would have been closer. In that case, campaign funds might flow differently, candidates might adopt different tactics, and the ultimate results could look quite different.

TABLE 5 Presidential Support with Socioeconomic and Demographic Variables and Partisan Tendency: Left at Minimum of Plausible Range

PARAMETER	LULA (PT)	COLLOR (PRN)	BRIZOLA (PDT)	MALUF (PDS)	ULYSSES (PMDB)	LULA (2) (PT)
Intercept	29.16**	45.00**	15.32**	-8.43**	9.56**	50.64**
PT	13.40**	-2.66	-13.54**	75	2.00**	.63
PDT	-7.06**	-14.47**	26.06**	-2.52**	.48	11.33**
PMDB	-2.75**	-3.24**	1.74	.26	3.47**	.21
PSDB	5.74**	5.77*	-2.37	-1.18	2.01	3.71
PDS	-3.27*	-1.92	-1.02	4.81**	.10	-2.99
PTB	-1.93	5.65**	-5.95**	1.08	51	-5.90**
PFL	75	3.91**	-5.01**	.95	19	-4.29**
Coalitions						
PT	48	-11.31**	21.29**	-7.62**	1.72*	12.84**
PDT	-1.84*	-5.40**	9.21**	-1.34**	.77	6.01**
PMDB	1.49*	42	-3.08*	- .18	2.23**	.39
PSDB	10.76**	-4.60**	-19.03**	-2.91**	2.41**	3.86*
PDS	-3.44**	-2.66**	1.39	2.16**	1.13**	-1.14
PTB	.92	-1.59	-1.28	.77	.44	1.45
PFL	3.56**	1.48	−6.11**	.23	54	60
Voters (millions)	5.32**	-7.90**	28.10**	15.44**	.38	22.60**
Voters ²	−.64 **	1.57**	-5.60**	2.73**	07	-4.20**
% in manufacturing	.34	-68.24**	62.94**	28.18**	-8.29**	54.31**
% government employees	15.59	-80.27**	128.33**	08	-44.67**	102.99**
% in agriculture	03**	.02**	.01	.02**	.00**	−.05**
% migrants	一.01**	.05**	00	.01**	03**	03**
Per capita income (\$1,000s)	-2.60**	-1.62**	-3.53	2.82**	−.49**	93 * *
Minimum partisan left tendency	13.79**	36	-26.79**	6.71**	-2.11	-8.23**
R ²	.29	.48	.38	.56	.26	.47
F	66.08	152.4	98.76	205.7	57.27	146.9
р	.0001	.0001	.0001	.0001	.0001	.0001

Note: N = 3,611.

 $p \le .05$. $p \le .01$.

EXTENSIONS OF THE BASIC ARGUMENT

I shall present two refinements of the final model, each strengthening the conclusion that party organization matters.31 The first estimates the model for a subset of municipalities identified according to the size and preferences of their neighbors. This refinement demonstrates that the institutional basis of Brazilian electoral politics, especially the trajectory of political careers at the state level, affects the strategies of local politicians. The second refinement explores the organizational and affective bases of support of a single, unique politician, Leonel Brizola.

What Happens When Neighbors Disagree?

Remember that Brazil elects members of its state assemblies and its federal Chamber of Deputies through open-list proportional representation. At both levels, whole states are districts.³² The most common entry into the federal legislature is service as a state deputy, and state deputies often begin in local politics, for example, as municipal council members or mayors.³³ Suppose a mayor or council member wants to run for a legislative seat, and suppose the potential candidate can count on heavy support from the citizens of the municipality. Still, few individual municipalities are so big that their votes alone can assure victory, so aspiring statewide candidates from small communities need outside allies. Logically, they seek these allies in nearby municipalities. This search gives politicians from large communities broader influence, influence beyond their particular municipalities. The smaller the community from which a potential candidate hails, the more dangerous it is to alienate big neighbors.³⁴ If politicians from larger communities back a different presidential candidate, a small-town mayor may decide not to disrupt opponents' rallies, council members may choose to ignore (rather than follow) the mayor's political directives, and community leaders may reach "understandings" with outside political forces.

The political danger of disagreeing with large neighbors ought to weaken endorsement effects. How can this hypothesis be tested? First, ascertain, for each municipality, which contiguous neighbors were governed by parties backing opposing candidates.35 Next, calculate each municipality's opposition neighbors, that is, the total contiguous popula-

	nine Sui	rrounde	a by La	irge Nei	gnbors	Backing	Uther	Candida	tes			
		JLA PT)		LOR RN)		ZOLA DT)		LUF DS)		SSES IDB)		LA (2) PT)
PARAMETER	ALL	S.O. ^a	ALL	S.O. ^a	ALL	S.O. ^a	ALL	S.O.ª	ALL	S.O.ª	ALL	S.O. ^a
Intercept	29.94	31.39	45.49	42.50	10.24	17.45	-6.55	-10.50	9.1	9.81	48.0	53.96
PT	14.86	8.75	-2.71	13	-16.30	-9.98	07	-2.29	1.7	1.80	1	-3.19
PDT	4.98	-3.71	-14.49	-15.85	21.77	24.79	-1.41	-1.13	.1	.47	9.9	12.9
PMDB	-1.09	-2.66	-3.25	62	-1.72	74	1.17	1.22	3.1	1.87	9	-2.8
PSDB	6.76	5.06	-5.71	-4.20	-4.92	-3.57	44	-1.01	1.8	1.48	2.7	1.5
PDS	2.47	-2.07	-1.85	-1.94	-3.24	-1.67	5.48	4.45	0	.19	-3.8	2.5
PTB	24	78	5.67	3.91	-9.72	-6.87	2.11	1.69	8	51	-7.1	-5.5
PFL	.47	80	3.93	.90	-7.73	-4.06	1.68	2.25	4	.02	-5.2	-3.5
Coalitions				-								
PT	2.47	-1.17	-11.58	-5.13	16.87	33.17	-6.74	-15.82	1.3	2.95	11.8	12.9
PDT	-1.83	-2.05	-5.09	38	7.08	43	44	.97	.5	1.30	4.7	-1.2
PMDB	3.37	-2.11	44	-1.51	6.92	2.06	.80	.88	1.9	.18	8	.2
PSDB	15.76	16.54	-4.68	6.19	-29.13	-32.23	31	-5.80	1.6	3.29	.6	-5.9
PDS	2.26	-2.96	-2.66	-3.61	-1.06	03	2.80	3.62	.9	1.09	-1.9	3
PTB	2.03	82	-1.63	-2.00	-3.33	1.87	1.26	.24	.2	.42	.8	1.3
PFL	5.00	5.59	1.46	2.76	-9.05	-8.91	1.00	.16	7	05	-1.5	-1.7
Voters (millions)	3.54	1.51		-21.50	30.19	37.04	15.74	-9.41	.5	-2.24	22.8	31.8
Voters ²	49	21	1.55	5.37	-5.79	-6.80	2.74	.78	0	.63	-4.2	-6.5
% manufacturing % government	4.85	7.89	-67.21	-45.05	46.18	16.10	33.76	37.76	-9.6	-13.26	46.9	21.7
employees	-15.07	15.02	66.31	37.06	97.08	-47.94	23.31	30.44	-47.8	-56.74	68.1	-31.8
% agriculture	04	04	.02	.04	.02	01	.02	.03	.0	.00	0	- .C
% migrants	00	.00	.05	.06	05	06	.03	.03	0	03	0	0
Per capita income									-			
(\$1,000s)	-2.50	-2.83	-1.62	-1.75	-5.26	98	2.86	3.43	-5.1	59	-9.7	-1.3
Maximum partisan								- · · · ·			= *	
left tendency	5.11	5.87	2.10	-4.74	3.64	3.95	-3.24	-3.18	.3	1.50	4.8	7.1
H ²	.27	.31	.48	.53	.35	.45	.55	.47	.26	.24	.47	.5
F	58.84	3677	152.7	90.70	86.51	66.8	201.1	72.28	56.68	25.14	145.9	11
p p	.0001	.0001	.0001	.0001	.0001	.0001	.0001	.0001	.0001	.0001	.0001	.0

Note: Left is at maximum of plausible range. N = 3,611 for "all," 1,807 for small outliers. "Small outliers.

tion whose mayors backed opposing candidates minus the total contiguous population whose mayors supported the same candidate. Then find the ratio of opposition neighbors to the local population. Finally, estimate the endorsement model on what I call *small outliers*, that is, municipalities with neighboring opponent-to-local population ratios above the national mean. Table 6 compares these results to the overall model.

The estimates in Table 6 support the argument. In small-outlier PTB and PFL municipalities, Collor's first-round gains decline (though remaining statistically significant). Maluf's PDS support and Guimarães' PMDB backing also fall. In the second round, small-outlier PT municipalities give Lula less support; similar municipalities of the opposing PDS, PTB, and PFL provide Lula more backing than he gets in the country as a whole. Only one party runs against the trend: the PDT offers strong support to Brizola in round 1 and to Lula in round 2 even when big neighbors are hostile. Actually, this exception strengthens the case. As we saw earlier, the PDT, especially in Rio de

Janeiro and Rio Grande do Sul, is not merely a local machine. In these areas of PDT domination, the party has strong state-level leadership, and it serves as an electoral vehicle for a charismatic candidate commanding loyal grassroots support. Politicians may adjust their behavior to future political prospects, but ordinary voters do not.³⁶ Thus, the small-outlier hypothesis applies to machine-based candidates, not those with durable grassroots support. Politicians in strong local machines seek to ingratiate themselves with presidential hopefuls, but they also look to their immediate political futures, futures that, in Brazil, are inevitably determined at the state level.

Organization Versus Charisma in Support for Brizola

Charisma is a term social scientists and journalists often toss around without careful definition, but even by the most demanding criteria, Leonel Brizola qualifies as charismatic. Still, how can a charismatic

presidential candidate receive 1.5% of the vote in the state of São Paulo and 50.5% in the state of Rio de Janeiro? Of course, Rio and São Paulo differ, but both are industrial, heavily urban, and located in the highly developed Southeast. Brizola had no difficulty, moreover, reaching Paulista voters: his television access in the two states was exactly equal. And he had already managed, without losing his original base, to transfer his appeal from his first home in Rio Grande do Sul to Rio de Janeiro. Why was São Paulo so hard to crack?

Perhaps the problem lay purely in organization. The PTB, Brizola's party before the military dictatorship, had tried for almost 20 years, with little success, to gain a foothold in São Paulo.³⁷ The modern PDT itself had few Paulista deputies and controlled few municipalities. But weak local organization can be no more than a partial explanation, because in Rio Grande do Sul and Rio de Janeiro Brizola did well even where *opposition* parties controlled local municipalities.

Suppose we approach the Brizola riddle by examining his support spatially, moving from the centers of strong Brizola states to municipalities near the borders of these states, then to municipalities on each side of the border, then to municipalities near the borders of opposition states, and finally to the centers of opposition states. If Brizolismo is purely an organizational phenomenon, the drop-off at the border of his core states should be quite abrupt. A gradual decline, on the other hand, would suggest support built on social networks and on affective ties between Brizola and voters, because such networks and ties cut across state boundaries.

What is the appropriate measure for assessing Brizola's support? Given that the municipalities in each spatial group have different expected (predicted) levels of Brizola vote, levels that depend on each group's socioeconomic and demographic conditions, partisan tendencies, and local political control, actual Brizola vote percentage would be inappropriate. Rather, the test should be the size of the difference between Brizola's actual percentage and the model's predicted percentage. ³⁸ In other words, an initial way to assess the argument is to compare these residual values for Rio de Janeiro and São Paulo, Rio de Janeiro and Minas Gerais, and Rio Grande do Sul and Santa Catarina.

Let us begin with Rio de Janeiro and its neighbors. Excluding the contiguous border municipalities of the two states, the differences between Rio and São Paulo average 40 percentage points. Brizola's vote in Rio municipalities bordering São Paulo falls off, thus suggesting a gradual decline in Brizola support, but this is misleading. The shape of the two states is such that very few municipalities actually touch, and a large number of former Paulistas live inside the state of Rio in these few border municipalities. In fact, Brizola's support drops off very rapidly upon crossing the border. A comparison of Rio de Janeiro and Minas Gerais leads to a similar conclusion. The contiguous municipalities of the two states differ by 20

percentage points, and the municipalities one row from the border are 43 points apart. So far, then, the story seems to be one of organizational strength and weakness.

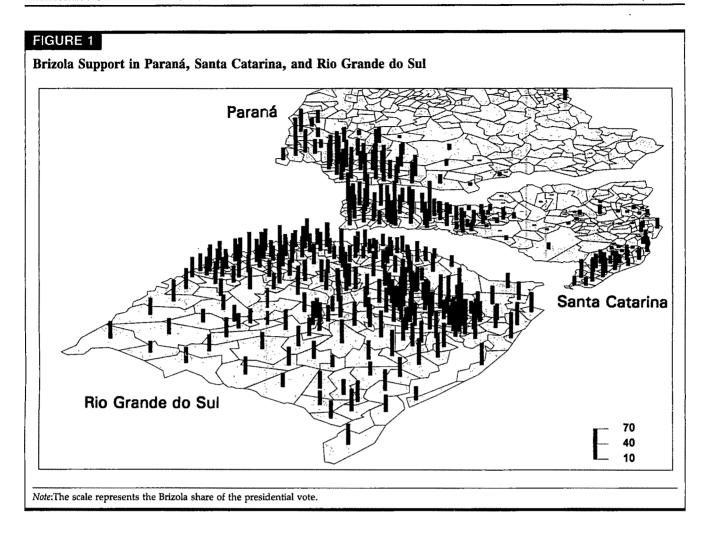
Tracing Brizola's vote from Rio Grande do Sul to Santa Catarina reveals a different picture, one of more gradual decline. Brizola's vote drops off very little from the center of Rio Grande do Sul to its border with Santa Catarina. On the Catarinense side of the border, his support is a bit lower but still substantial, and he holds that level in municipalities one row back from the border.

Figures 1 and 2 illuminate the difference by showing the actual municipalities involved in each case. Figure 1 demonstrates that Brizola's strength in Santa Catarina is not distributed evenly along the entire border with Rio Grande do Sul; rather, it concentrates in the west and along the east coast.39 And observing that Brizola's strength continues past western Santa Catarina into southwestern Paraná, a pattern of migration emerges. Over the last 40 years, one stream of migrants from Rio Grande do Sul went up the eastern edge of Santa Catarina toward São Paulo. Another, numerically much more important, went across western Santa Catarina into Paraná, then into Mato Grosso do Sul, Mato Grosso, and into the Amazon. Some migrants stopped in Santa Catarina; others trekked as far as Paraná. Because their social ties remain with Rio Grande do Sul, they are fertile ground for PDT organizational efforts. Figure 2, by contrast, shows what happens in the absence of a long-lasting population exodus. Because people flow back and forth across the border, Brizola shows some strength in the nearest municipalities. But beyond these neighbors, his support quickly drops to very low levels. 40

Obviously this is just a preliminary test of a complex hypothesis. Still, the analysis highlights the limits of charisma. Brizola is undeniably a charismatic politician, and the rules of the Brazilian electoral campaign gave him television and radio access to voters everywhere. But he scored well only where his political organization had been in place long before the 1989 campaign. In such areas, the PDT's ties with voters bound them to Brizola even after they migrated. But where Brizola's organization had historically foundered, his considerable personal magnetism failed to attract even minimal support.

CONCLUSION -

Most scholars and journalists, I suspect, wrote about Brazil's presidential election from the perspective of their own places of residence, that is, from very large cities. In such cities, electronic media are a principal means of political communication, and political organizations are far less dominant than their counterparts in small cities or rural areas. But all over Brazil, local political organizations are alive and well. They survive because politicians, faced with the institutional structure of Brazilian politics, find it necessary



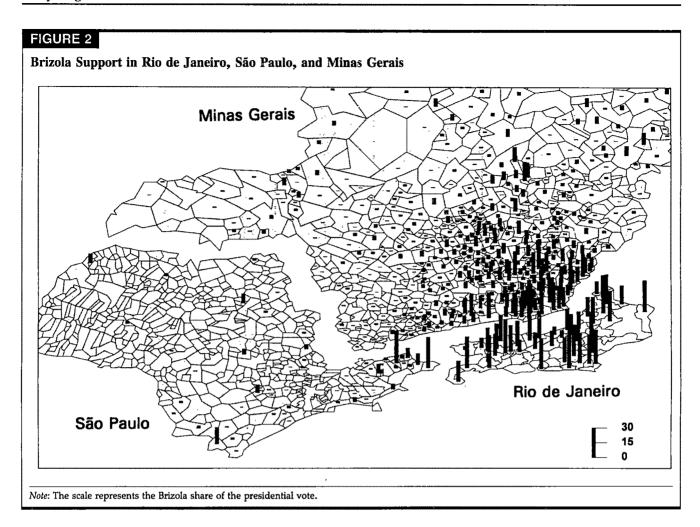
and feasible to trade blocs of votes for pork barrel and patronage.

True, the candidates with the strongest organizations failed to advance to the second round in the Brazilian election. But they would have been enormously weaker without the support organization provided. The issue is not whether Brizola would have edged Lula or whether Lula could have beaten Collor. This election was clearly an anomaly: Collor was the first presidential candidate to utilize the electronic media extensively. In the future, all serious candidates will be "telegenic," and all will make sophisticated use of radio and television. No one, as a result, will have Collor's overwhelming advantages. In consequence, party organization is likely to become crucial just because it creates advantages that candidates cannot overcome during the short life of a campaign itself. Brizola's utter failure to penetrate São Paulo demonstrates, even for charismatic candidates, the centrality of organization.

More than 40 years ago V. O. Key (1949) explored the spatial nature of local, state, and national political organization in the American South. Though data-processing techniques have advanced enormously since Key's pioneering work, few scholars have followed his lead, either in the United States or elsewhere. What contribution did the big-city machines

of the United States make to presidential vote totals in the early part of the century? How does Mexico's Revolutionary Institutional party channel votes to the "official" candidate? Did local organization matter in pre-1973 Chile, where open-list proportional representation coexisted with ideologically coherent parties?

The failure of scholars and journalists to recognize the importance of local organization in Brazil's presidential election also has a methodological aspect, one that raises warning flags for analysts of elections in other newly competitive polities. With the rise of the political marketing industry, the price of survey research has declined, and survey techniques in Brazil and other Third World countries have advanced. Scholars would be wise, nevertheless, to temper their attraction to survey data. Although public opinion research tells us a great deal about the motivations of individual voters, surveys sample from broad geographic areas. As a result, survey data inevitably mask such locality-specific effects as party organization. Because political context matters, analysis of aggregate data must remain an important part of public opinion research. Brazil can hardly be the only country where the political landscape shelters both the electronic media and local political organizations.



ABBREVIATIONS

ARENA National Renovating Alliance

MDB	Brazilian Democratic movement
PCB	Brazilian Communist party (Roberto
	Friere)
PDS	Democratic Social party (Paulo Maluf)
PDT	Democratic Labor party (Leonel Brizola)
PFL	Liberal Front party (Aureliano Chaves)
PL	Liberal party (Afif Domingos)
PMDB	Brazilian Democratic Movement party
	(Ulysses Guimarães)
PRN	National Reconstruction party (Fernando
	Collor de Mello)
PSD	Social Democratic party (Ronaldo Caiado)
PSDB	Brazilian Social Democratic party (Mário
	Covas)
PT	Workers' party (Luís Inácio Lula da Silva)
PTB	Brazilian Labor party (Affonso Camargo)
	* · · ·

Notes

This paper is part of a larger project on Brazilian elections and legislative behavior. It has been supported by Washington University and by the National Science Foundation (SES-8921805). I am especially indebted to the many Brazilians and others who facilitated my research and understanding of

Brazilian politics, including Antonio Medeiros, Benedito dos Santos Gonçalves, Carmen Perez, Celina Souza, David Fleischer, Gei Espinhara, Jardelino de Lucena Filho, José de Ribamar C. Caldeira, Maria de Alonso Andrade, Maria D'Alva Gil Kinzo, Pedro Celso Cavalcanti, Sandy Lubezky, and Valentina Rocha Lima.

- 1. For a provocative discussion of the importance of spatial attachments in American politics and of the role of political institutions in fostering such attachments, see Salisbury 1993.
- 2. Increases in participation do not necessarily increase party volatility. But in Brazil even experienced voters have low levels of party loyalty, so new voters enter the electoral process in a context of weaker socialization into parties. On party volatility in Latin America, see Coppedge 1992.
- 3. The PTB did contest both elections, but the 1989 version of the party was similar to the 1960 version in name only.
- 4. Šee, e.g., Geddes and Neto 1992; Kinzo 1992; Lamounier 1989, 1990; Lima 1993; Marchal, Théry, and Waniez 1992; Nêumanne 1989; Silva 1993; Singer 1990; A. Souza 1992; Straubhaar, Olsen, and Nunes 1993. Singer's view is typical: "Collor's election [placed him] outside the parties, with a rhetoric of confrontation against the elites and with a strong base of support among the masses whose availability always characterizes Latin American populism. . . [There was] direct linkage between leader and mass, without any necessary party channel" (1991, 18). Singer based the analysis on surveys taken in the poor areas of the city of São Paulo. Amaury de Souza refers to "plebiscitary renewal" and to the "volatility of the electoral support of parties and public" (1992, 181). The weekly news magazine Veja said that the electorate demonstrated that "it votes for who it wants, when it wants, without the opinion of any political boss having any

influence over its choice" ("O voto no raio x," Veja, 29 November 1989, p. 62). Lamounier (1989) points to four aspects, in addition to television, of the Collor appeal: Collor's anti-Sarney campaign, his campaign against politicians in general, his ability to use his small party efficiently while internal disputes wracked the big parties, and his support from business as the "lesser evil" as compared to Lula or Brizola. Lima (1993) focusing specifically on the role of television in the campaign, discusses the soap operas airing on Brazil's biggest network, TV Globo. These novelas. "either directly or indirectly portrayed Brazil as a kingdom of political corruption run by professional politicians and politics as a contaminated social space" (p. 105).

5. In addition, administrations can refuse to transfer

- 5. In addition, administrations can refuse to transfer money committed by previous agreements, even international agreements. President Sarney, for example, blocked an emergency loan that the World Bank had slated to help Rio de Janeiro deal with the effects of a flood.
- 6. I estimated a logistic regression in which the dependent variable was the existence of an education or health transfer agreement with the central government between April 1990 and July 1991—roughly, the first 15 months of the Collor administration. Transfers were more likely to go to municipalities that were larger in population, more urban, and poorer. But for an Alagoas municipality, the probability of receiving a transfer jumped an additional 123%; for a Bahian municipality with a PFL mayor, the probability jumped 105%; for a Paraná municipality, the probability jumped 84%; and for a municipality in Rio Grande do Sul, the probability jumped 160%. The qualification "with a PFL mayor" reflects the ability of Bahia's powerful governor, Antonio Carlos Magalhães, to channel central government money to municipalities of his party.
- 7. In his study of Presidente Prudente, a cattle-raising town in western São Paulo state, Lamounier argues that in an undiversified economic structure of this type, "employment opportunities will frequently depend on favors and personal loyalties. . . . Such a structure would be very unreceptive to oppositionist appeals such as those in contemporary Brazilian party disputes" (1978, 4).
- 8. Brazilianists have given a great deal of attention, however, to parties, elections, clientelism, regionalism, populism, and related topics. The classic treatment of local-level clientelism is Leal 1949. Empirical works on parties and elections include Fleischer 1981, vol. 1; Lamounier 1989; Lamounier and Meneguello 1986; Mainwaring 1992/93, Santos 1979; Sarles 1982; Soares 1973; A. Souza 1992; and M. Souza 1976. On regionalism, see Lavareda 1978; Schwartzman 1975; and A. Souza 1985. On populism, see Weffort 1978. Research focusing on the 1989 election includes Singer's (1990) study of Collor support in São Paulo city, Kinzo's (1992) analysis of Presidente Prudente (a municipality in São Paulo state), and Fleischer's (1989) work on the effects of the 1988 municipal election.
- 9. Chile (pre-1973) and Finland are other examples of open-list proportional representation, but in both cases, national party leaders select candidates. Chile's congressional districts, in addition, were arbitrary constructs: they corresponded to no unit of government, and they cut across province lines. See Ames 1992.
- 10. When the water supply in one São Paulo municipality failed, President Sarney promised to release central government funds if the deputy based in that area voted to give the president a term of five (rather than four) years. The deputy refused, and the president held up the money. But when Sarney left the country for a foreign visit, the interim president, Paulista Ulysses Guimāraes, immediately signed the papers. The money flowed.
- 11. During the Constitutional Assembly of 1987–88, a large billboard greeted deputies and senators (who jointly constituted the Assembly) leaving the airport in Brasília. The sign read simply, "CO + NO + NE = 259"; that is, the delegations from the Center-West, North and Northeast together held 259 votes, a majority of the Assembly. Purely by population,

- they would merit less than 30%. Most of the advantage falls to states in the Center-West and North.
- 12. I shall refer to the candidates in a modified form of common Brazilian usage. Sometimes common usage implies a family name, sometimes a given name, sometimes a nom de guerre. The names in Table 1 are exactly as they appear in official electoral results.
- 13. Aureliano Chaves's PFL was a splinter of the PDS and was formed to support Tancredo Neves's successful run for the presidency against the official candidate in the electoral college vote in late 1984. Concentrated mainly in the Northeast, the PFL is ideologically indistinguishable from the PDS (which in 1993 merged with the Christian Democratic party to form the Renovating Progressive party), though its hundreds of mayors are perhaps a bit more oriented to pork-barrel politics. Chaves had been vice-president under the last military president, João Figueiredo, and minister of mines and energy in the Sarney government. Tests of the various models on Chaves's vote percentage show small but statistically significant gains in municipalities with PFL mayors.
- 14. In work in progress, I have implemented a factor analysis of votes in the 1987–88 Constitutional Assembly. Both the right-wing and left-wing parties fall out as well-organized factions, but the PMDB rarely votes as a bloc.
- 15. For a good treatment of Brizola's career, see Bandeira 1979. On the old PTB, the best work is Benevides 1989. Amaury de Souza (1985) analyzes Brizola's victorious 1982 campaign for the governorship of the State of Rio.
- 16. Brizola would have overtaken Lula and gone to the second round if he had picked up another half-million votes, that is, if his São Paulo total had represented just 4.2% of the state's votes.
- 17. The figures are percentages of those who actually voted minus about .6% who voted null or blank. About 12% of those eligible to vote failed to appear at the polls.
- 18. The major parties also participated in coalitions successfully backing a member of a minor party. In fewer than two hundred cases, a minor party won with no coalitional support. Some successful parties, such as the Municipal party, ran no candidates above the local level. They will be ignored.
- 19. I searched for mayoral endorsements in at least one newspaper from the capital city of every state in Brazil during the last four months before the election. These desertions became the basis of a "true preference" variable, which is the party of the mayor, adjusted to subtract desertions to, and add desertions from, other parties. The whole empirical analysis was then redone with this new variable. As expected, coefficients of the party endorsement variables are a bit stronger. In the interests of simplicity, I present only the "pure party" version of the analysis. This version is preferable, because the reporting of desertions by the newspapers may be systematically biased toward, for example, larger communities.
- 20. The models presented here become more complex, but they never include dummy variables for individual states. Though such dummies were tried, they seemed generally inappropriate even though they raised R-squareds. First, Brazil's political institutions ought to have the same effects everywhere, so the model should attain reasonable levels of explanatory success without dummies. Second, I could find no theoretically consistent explanation for the differences in the dummies' coefficients. Finally, I tried some state-by-state regressions, but the differences seemed more likely the results of the differing numbers of municipalities than differences in state politics.
- 21. The Instituto Brasileiro de Geografia e Estatística (IBGE) implemented the census. Officials at Prodasen, the data-processing agency of the Federal Senate, made the data available in machine-readable form. The IBGE did not undertake the 1990 census until 1991–92; few results are currently available.
- 22. The registered electorate is a good proxy for the adult population, because voting in Brazil is obligatory, with fines for those who fail to vote and cannot demonstrate an imped-

iment such as illness. Registration is required for all literates over 18 and less than 70; it is optional for illiterates, those between 16 and 18, and those over 70.

- 23. The models include both the number of voters and its square, because for some candidates a curvilinear relationship seemed likely. In other words, up to a certain point their vote shares would increase with population, but above that population level vote shares would level off or decline.
- 24. Lula still does well in the eight municipalities controlled by the PSDB. Most were located outside São Paulo, the home state of PSDB candidate Covas.
- 25. Although the analogy is imperfect, the coercive effects of a dominant machine may resemble those of censorship under a dictatorship. For an analysis of Brazilian public opinion under military-imposed censorship, see Geddes and Zaller 1989.
- 26. These regressions were implemented state by state; that is, a regression was run separately in each state on all municipalities above the state's median interparty fragmentation. The variables included measures of total population, educational attainment, size of tertiary economy, agricultural population, migration, income distribution, and per capita income. The R-squareds varied from 2% to 77%, with the differences mostly a function of the number of municipalities in the state-by-state regressions. Using F-tests, all are significant except 1982 for São Paulo and 1986 for Minas Gerais. Interparty fragmentation equalled 1 minus the sum of the squares of the total vote garnered by each party. Levels of interparty fragmentation were substantially higher in the states of the South and Southeast.
- 27. The two Left estimators exclude the 1986 election, because in most municipalities the PT and PDT simply had no organization or candidates.
- 28. The choice of mayor is really endogenous; that is, it is affected by partisan tendency, and the model as specified ignores that relationship. Correctly specifying the relationship would be very difficult, and it is unlikely that the results would differ.
- 29. This result confirms my personal observation during the campaign. Ulysses Guimarães commanded enormous loyalty and respect for his role in restoring Brazilian democracy. Some of his followers held out hope that his standing in the polls would recover, but even those who knew the game was lost refused to declare for other candidates. His accidental death in November 1992 brought forth an outpouring of genuine mourning rare among politicians.
- 30. For continuous variables, take the mean value of the variable times the coefficient; for the dummy variables, multiply the number of municipalities with a particular party or coalition times the coefficient.
- 31. I also estimated the model for a regional partition of municipalities. If parties in the Northeast (a region known for its poverty, greater clientelism, and machine dominance) contribute significantly more to their candidates than parties elsewhere, the results presented earlier may simply mistake a regional phenomenon for a national one. If, on the other hand, estimates of party effects in the Northeast are similar to those in the country as a whole, then it is more likely that we have identified a strategic response of both local politicians and local voters to the incentives they face.

In fact, regression results show that party organizations in the Northeast performed, with predictable deviations, much like those in the rest of the country. Lula's PT, Brizola's PDT, and Maluf's PDS all helped their candidates less in the Northeast than in the country as a whole. In the first round, Collor did about twice as well in northeastern PTB or PFL municipalities as in all of Brazil, and Ulysses Guimarães almost doubled his overall PMDB total in the Northeast. In the second round, Lula improved his vote percentage in PT municipalities of the Northeast, but the PT controlled so few municipalities that the additions are unimportant. In northeastern PDT municipalities, Lula saw his second-round gains drop to less than half those in PDT municipalities nationwide. And Lula lost heavily in PDS, PTB, and PFL municipalities located in the Northeast.

Why are these results plausible? Consider Lula, Brizola, and Maluf. The increments in the vote of Lula and Brizola in PT and PDT municipalities of the South and Southeast reflect both organizational strength and fierce, long-standing partisanship. Outside the South and Southeast, these parties lack the cumulative power of working-class and industrial contexts and their traditional militancy. Thus the Northeast is pure organization, almost without grassroots militancy. Maluf's relative decline in the Northeast is actually not statistically significant, but his identification as a Paulista candidate might have led to weakness. Maluf's national percentage in round 1 was 8.28%; in São Paulo he received 22.56%. So Maluf loses not just in the Northeast but everywhere outside his home. The gains in the Northeast by Collor and Ulysses Guimarães (and Lula's Iosses to Collor in the second round) reflect mostly organizational strength, though Collor's status as a northeasterner probably helped. Though Ulysses Guimarães was a Paulista, his party endorsement coefficient was much worse in São Paulo than in the Northeast.

In general, then, parties relying on grassroots support faltered in the "backward" Northeast; and parties relying on machine domination did better. In no case, however, does removal of the Northeast states from the estimation erase the significance of the party coefficients in the South and Southeast. Thus local party machines support candidates at about the same rate in the "modern" areas of the country as in the supposedly tradition-bound, "clientelistic" regions.

32. To those unfamiliar with Brazilian politics, this may seem bizarre, so it is worth putting another way. State assemblies really have no districts at all: everyone is elected at large.

- 33. In the last four legislatures, 32% of federal deputies had been state deputies immediately before moving to Brasília. Another 13% had just concluded terms as mayors or municipal council members, while 21% held state-level bureaucratic positions. Career paths vary widely, however, across states. In states like Bahia, where bureaucratic control over federal largesse has long been a route to political power, purely local politicians are rare. In the South and Southeast, especially in states like Minas Gerais, politicians tend to have strong local roots, so the most common career trajectory follows a single sequence: council, mayor, state deputy, federal deputy. See Ames 1992.
- 34. For an extensive treatment of these issues, see Ames 1992.
- 35. This is, of course, no small task for a country in which many states have hundreds of municipalities. I have developed computerized municipal-level maps of all Brazilian states. These maps, coupled with programs developed by the statistics department of the University of Washington, Seattle, generated a 1–0 nearest-neighbor matrix for each state. With these matrices, a program could be created to produce a data set in which the voting population of each municipality's contiguous neighbors was output to the same observation.
- 36. The same argument could be made for Lula and the PT; that is, his personal appeal should prevent declines in small-outlier PT municipalities. The actual decline that did occur is probably a statistical artifact: since most PT municipalities were large cities, very few fit into the small-outlier category.
- 37. The Paulista PTB, Benevides points out, had always been "electorally weak, politically disorganized, and ideologically inconsequential," and it served the interests of the PTB leaderships in Rio de Janeiro and Rio Grande do Sul that it should remain so (1989, 9; see also 18–22).
- 38. The discussion uses the minimum left partisan tendency, but the results are essentially the same with partisan tendency at maximum left level.
- 39. In the municipalities along Santa Catarina's western border with Rio Grande do Sul, Brizola averaged 47.4%, and in coastal municipalities, he reached 37.5%. On the central part of the border, however, he averaged only 20%.
- 40. Note that both São Paulo and Minas Gerais extend much further away from the border than the municipalities

shown in the figure. Brizola's strength in these distant regions is extremely low.

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THE PRESTHUS DEBACLE: INTRAPARTY POLITICS AND BARGAINING FAILURE IN NORWAY

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Bargaining failure has been neglected in the study of interparty coalitions. I examine the unsuccessful attempt by three Norwegian nonsocialist parties to defeat a Labor party minority government in June 1987 when the majority opposition twice failed to agree on a simple vote to that end. This failure of coalition bargaining was facilitated by incomplete information, intraparty constraints and misdelegation of authority, and by anticipation of the impending parliamentary recess. Despite these complexities, simple noncooperative games like the war of attrition can shed considerable light on this and other unsuccessful negotiations. The "Presthus debacle" demonstrates that political parties bring many different objectives to the bargaining table and that the trade-offs between these objectives vary according to organizational and institutional conditions.

n Monday, 8 June 1987, Rolf Presthus was widely expected to become Norway's next prime minister within a few days. Presthus' Conservative party (H) had just agreed to join forces with the other two major nonsocialist parties, the Christian People's party (KRF) and the Center party (SP), to bring down Gro Harlem Brundtland's Labor party (A) minority government on the farm bill that it had brought before the Storting (the Norwegian parliament). The nonsocialist motion of no confidence (censure), attached to a farm bill amendment, needed only the additional support of the right-wing Progress party (FRP), which had repeatedly called for the government's ouster. Well-informed observers estimated the likelihood of a change of government at 80% or 90% (Rommetvedt 1991, 198). Yet on 12 June, the Storting rejected two separate no-confidence motions against the government. The nonsocialist majority of four parties, each explicitly committed to dislodging Brundtland's government, twice failed to agree on a simple vote to that end. Presthus' dreams of becoming prime minister never materialized, and the nonsocialist parties were humiliated.

The causes of this "Presthus debacle" are interesting for a number of reasons. First, the outcome was one that none of the principal players most preferred and few anticipated. Four parties, all explicitly committed to a change of government and collectively a legislative majority, twice failed to oust a minority government. Moreover, two of these parties, the Center party and the Conservatives, set the agenda in ways that left them distinctly worse off. The Presthus debacle underscores our limited understanding of bargaining failure in coalition politics. It demonstrates constraints on the ability of political parties to make enforceable agreements, as well as the importance of information and sequence in interparty negotiations. Finally, the events of June 1987 show that parties differ in the objectives they bring to coalition bargaining. To account for such differences in party objectives, we have to consider the constraints of politics within parties, especially the consequences of different intraparty *delegation regimes*.

In this analysis, I first discuss the causes of bargaining failure, as well as the implications for coalition theory, then describe in greater detail the events of June 1987. I construct an initial game-theoretic representation of these events, derive various solutions, and test their adequacy. In order to solve the remaining puzzles, I then introduce a more complex account of party behavior, taking into account information uncertainties and intraparty constraints. I apply this framework to the various phases of the Presthus debacle in order to illuminate the behavior of the critical players. In the conclusion, I summarize the lessons of the Presthus debacle for the study of government coalitions.

BARGAINING FAILURE AND COALITION THEORY

The making and breaking of interparty coalitions are dramatic and decisive events in parliamentary democracies. And although the study of coalition behavior has long been one of systematic, rigorous, and often formalized comparative inquiry, there are still significant lacunae in our knowledge. As students of coalition politics have pointed out, we know much less about coalition termination than we do about formation, and there is next to no systematic knowledge of what happens in between, that is, about the process of coalition governing (Laver and Shepsle 1990a).

Bargaining Failure and Selection Bias

Even the study of coalition formation is curiously incomplete. Empirical studies almost exclusively focus on coalitions that have actually formed, that is, on *successful* coalition bargaining. The sordid tales of bargaining failure are rarely told. But if our interest is

in coalition bargaining itself, then this practice results in selection bias, which may distort our understanding insofar as unsuccessful bargaining situations systematically differ from successful ones (Geddes 1990). By restricting our observations to cases of successful bargaining, we obtain samples that systematically misrepresent the likelihood of securing such agreements. If our interest is in coalition stability, for example, the effect of neglecting bargaining failures may be systematically to underrepresent the least favorable conditions for durable coalescence.

Information and Bargaining Failure

The principal reason that coalition bargaining fails may be information uncertainty, which indeed has been a key concern in coalition theory since its inception. In his seminal work, Riker (1962) argued that information uncertainty could lead to deviations from his size principle, such as the formation of oversized coalitions, a thesis later elaborated by Dodd (1976). Luebbert took issue with this argument, claiming that "rationalist theory greatly overstates the role of information uncertainty" (1983, 242). In the face of bargaining failures like the Presthus debacle, Luebbert's dismissal seems too categorical, though he may have been right in suggesting that coalition theorists have traditionally emphasized the wrong kinds of information uncertainty. Riker and Dodd, for example, focused on uncertainty as to the weights (i.e., the number of parliamentary votes) and prior moves of other players (e.g., Dodd 1976, 40-47), whereas incomplete information about payoffs or feasible strategy sets may be the more critical uncertainties. In other words, the players in parliamentary bargaining are more likely to be uninformed about each other's options and objectives than about agreements and bids that already exist. In the Presthus debacle, as we shall see, the Conservatives and the Center party knew perfectly well how many seats the Progress party commanded. They were much less sure about how the latter weighed the costs and benefits of supporting their no-confidence motion.

Constraints and Intraparty Politics

Information uncertainty in coalition bargaining may in large part be a function of intraparty politics. Party leaders may know each other's personal preferences and yet be less than fully informed about the discretion each enjoys vis-à-vis his respective party. This problem goes to the heart of the assumption that parties bargain as unitary actors—a prevalent, though increasingly questioned, stylization in studies of cabinet coalitions (Laver and Schofield 1990; Laver and Shepsle 1990b; Luebbert 1986). It is often more fruitful to think of party leaders as agents of their respective parliamentary or extraparliamentary parties (their principals) in coalition bargaining. Because of variation in delegation regimes, party leaders may be constrained in different ways in these negotiations. Such constraints may strongly affect outcomes, and when bargaining fails, it may be because of how the hands of the agents are tied.

Institutions and Bargaining Models

Political institutions may also play an important part in bargaining failure. Coalition theory has traditionally paid little attention to the rules under which bargaining takes place (but see Pridham 1986; Strøm, Budge, and Laver n.d.). Such rules may regulate the mandate of the formateur or informateur, the sequence of negotiations, or the set of parties with which this person is permitted to negotiate. Another important factor that has rarely been systematically considered is time constraints on coalition bargaining. Situations of extreme time pressure may be particularly likely to cause information uncertainty and to occasion delegation problems that impede effective party oversight. Indeed, the time pressures associated with the impending parliamentary recess significantly impinged on the negotiations resulting in the Presthus

The complex set of factors that might facilitate bargaining failure is not adequately captured in the traditional study of government coalitions, which theoretically has relied mainly on cooperative coalition theory, whose predicted outcomes are entirely preference-induced, or in Riker's (1980) words "equilibria of tastes." The object has been to characterize a set of equilibria, generically large and unstable, without recourse to ad hoc institutional assumptions, which are indeed difficult to incorporate into this body of theory. Such considerations as information uncertainty, constraints, and sequential moves can much more adequately be addressed within the theory of noncooperative games. A disadvantage of noncooperative game theory is that its potential complexity may jeopardize parsimony of explanation. Yet in exploring the Presthus debacle, we shall see that even very simple noncooperative models can generate significant insights. These considerations suggest that the emerging literature applying noncooperative games can make fruitful contributions to the study of multiparty coalition bargaining (see, e.g., Austen-Smith and Banks 1988; Baron 1991, 1993).

PRESTHUS AND THE EVENTS OF JUNE 1987

The Presthus debacle ended a lengthy effort by the Norwegian Conservative party and its sometime allies to resurrect the nonsocialist coalition that had governed Norway from 1965 to 1971 and again between 1983 and 1986 (see Strøm 1990b; Strøm and Leipart 1993). In May 1986, Kåre Willoch's coalition, which had been reduced to a minority government in the September 1985 election, resigned after losing a bill to increase gasoline taxes to offset the precipitous decline in government oil revenues. Gro Harlem Brundtland formed a precarious Labor minority gov-

ernment facing a nonsocialist legislative majority. Upon his resignation as prime minister, Willoch also passed the chairmanship of the Conservative party to his hand-picked successor, Rolf Presthus, who had previously served as Willoch's finance minister.

Presthus' chief concern was to reconstruct the bourgeois coalition.² As finance minister, he had distinguished himself as an effective facilitator of interparty compromise, and largely for this reason, the Conservatives had elected him as their leader. Since the Conservative party, with 50 seats in the 157-member Storting, was almost twice as large as its two potential partners combined, Presthus was also the designated prime ministerial candidate of the three "coalitional" parties: the Center party (with 12 seats), the KRF (with 16 seats), and the Conservatives. His first opportunity to gain office arose in October 1986 during the debate on the budget presented by finance minister Gunnar Berge. The failure of this so-called "autumn hunting season" became apparent on 28 October, when the three coalitional parties abandoned their attempt to reach a budget accord. The Labor government survived, and the breakdown of these negotiations immediately led to mutual blaming and name calling by Conservative and Center party representatives.

The next chance to defeat Brundtland arrived during the following spring session, when there were two legislative opportunities for joint nonsocialist action: the revised national budget (which contained an agenda for long-term economic policies, such as taxation) and the government's farm bill. During April and May 1987, the three parties began to coordinate their policies in these two areas, amid extensive media speculation about the prospects for a change of government. The leaders of all three parties encouraged such speculations by issuing optimistic, though duly ambiguous, public declarations.

As negotiations reached a definitive phase in early June, the different designs of the three parties became more evident. The Center party and the Conservatives in particular had distinctly different preferences over the different potential censure motions that were discussed. One possibility was a general motion of no confidence unrelated to any particular issue. The second and third options were no-confidence motions attached to a farm bill amendment (to increase agricultural subsidies) or to a minority report on the revised national budget (to provide tax relief). The Center party, with its distinctly rural and agricultural constituency, strongly favored the farm bill option, whereas the Conservatives equally clearly preferred the third (the revised national budget). The position of the KRF was less clear. On 5 June, party leader Kjell Magne Bondevik signaled a weak preference for the option favored by the Conservatives. However, the KRF leader seemed most anxious to maximize the likelihood of passage. Bondevik was concerned that since the three coalitional parties collectively controlled only 78 of the 157 seats in the Storting, they needed the two votes of the pivotal right-wing

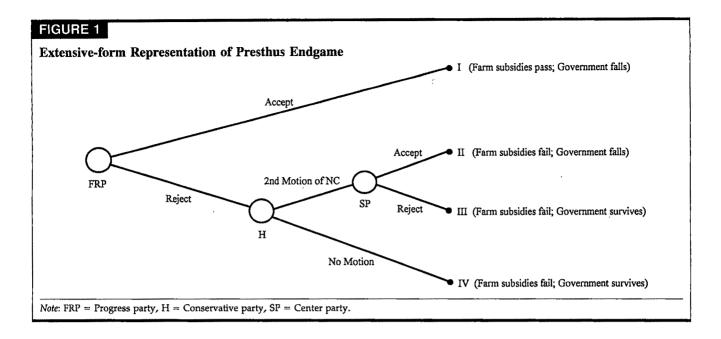
Progress party. The six Left Socialist party members had to be counted upon to support Labor's 71 representatives in any showdown with the bourgeois opposition.

Although the three coalitional parties needed the Progressive vote, they were not prepared to enter any binding agreement with a party many considered irresponsible and extremist. The question, therefore, was what sort of no-confidence motion Carl I. Hagen, the Progress party leader, would support without a quid pro quo. Hagen had explicitly promised to support a general no-confidence motion. He could also be counted upon to sympathize with a proposal to lower taxes, since tax relief was his party's principal policy concern. Higher farm subsidies, on the other hand, were hardly an issue for which the Progress party would muster any enthusiasm. On a no-confidence motion attached to this bill, Hagen would therefore be forced to choose between his preferences for a nonsocialist government and lower farm spending.

Negotiations between the three coalitional parties increased their pace through 8 June, as the end of the parliamentary session was rapidly approaching. After 12 June, the Storting would be in recess for four months, with local and regional elections in September. It eventually became clear that the Center party would insist on attaching the no-confidence motion to a farm bill amendment. The Center party's insistence, which left no room for concessions, set the agenda for the battle over the Labor government's life. The KRF, with few stakes in farm policy or tax relief, quickly accepted the Center party's policy demands. With recess less than a week away, the national executive committee of the Conservative party eventually caved in on 8 June and accepted farm subsidies as the key to a nonsocialist government. This agreement was cemented on 10 June, when the three parties' members of the Agriculture committee drafted a joint no-confidence motion calling for higher agricultural appropriations.

The pivotal Progress party decision came on 11 June, the penultimate day of the Storting's spring session and the day before the parliamentary vote. Precisely at 7:30 P.M. Progress party leader Carl I. Hagen gave a press conference, which was broadcast live at the top of the prime-time television news hour. Hagen's speech, in which he disclosed that the Progress party would not topple Brundtland at the cost of budget-busting farm subsidies, was an immense publicity hit. In chastising the other bourgeois parties for their irresponsibility and opportunism, Hagen managed to project fiscal and ethical integrity. And while he rejected the farm bill amendment, Hagen explicitly committed his party to a general no-confidence vote against Brundtland.

The next day, the final act unfolded. Arne Alsåker Spilde, a Conservative member of the Agriculture Committee, formally introduced the no-confidence motion. At about 9:30 P.M., the Storting defeated Spilde's motion by a vote of 80 to 77, as the Progress party and the Socialist Left cast their lots with Labor.³



Approximately one hour later, the legislators faced a second no-confidence motion, pressed by Conservative parliamentary leader Jan P. Syse and expressly built on the joint minority report of the three coalitional parties on the revised national budget. This motion was supported also by the KRF and allegedly drafted in large party by Kjell Magne Bondevik (Dagbladet, 17 June 1987). After a bitter debate, Syse's motion failed by a larger margin (89 to 68) than Spilde's (Stortingstidende 1986/87:4082-4214). In the end, the Center party voted against Syse's measure even though it was based on the party's own financial program. These defeats left the nonsocialist parties embarrassed and bitterly divided, while Socialist Left parliamentary leader Hanna Kvanmo gloated over 'this historic moment: the burial of non-socialist cooperation" (Rommetvedt 1991, 208).4 Kvanmo was temporarily correct, since the nonsocialist parties would make no further coalition efforts until after the September 1989 election.

The Presthus debacle also took its human toll. When the Storting finally took its summer recess at 2:30 A.M., Center party deputy leader Anne Enger Lahnstein remained seated in the empty chamber, quietly weeping over the humiliations her party had suffered during the acrimonious debate. Yet the hardest blows fell on Rolf Presthus himself. More than anybody else, he had been the architect cf nonsocialist cooperation, and as the bourgeois prime ministerial candidate, he would also have been its main beneficiary. The failure of Presthus' efforts quickly led to calls for his resignation as Conservative leader. In September, the Conservatives suffered a staggering defeat in the local and regional elections, and Presthus immediately tendered his resignation. Four months later, he was dead at the age of 51, felled by a massive heart attack.

GAME REPRESENTATION

This complex and puzzling "Presthus debacle" involved a variety of players, as summarized in the Appendix, I shall test the explanatory power of simple noncooperative game models against these puzzles and complexities, first by exploring a very simple set of assumptions and then by enriching these models as the task at hand requires. The interaction between these parties can be divided into two subgames: an unstructured bargaining game up through 10 June, which set the agenda for a subsequent, structured endgame over the following two days. My strategy will be to analyze these games in reverse order, beginning with the endgame and then using its solutions to understand the prior bargaining game. Figure 1 represents the endgame in extensive form, with each of the critical parties as a player. The game tree contains three decision nodes, illustrated by circles at which the player is identified by the first letter in that party's Norwegian name. These decision nodes represent choices faced by the three critical parties and the sequence in which they were confronted. Note that this sequence corresponds to the order in which the parties had to make their actual decisions, not to the formal voting procedure in the Storting. The branches lead from the starting node to four different end nodes (outcomes), identified by roman numerals. End node III represents the actual outcome.

For simplicity, Figure 1 excludes parties whose moves were taken for granted by all players. Thus, it was common knowledge that Labor and the Socialist Left would vote against any no-confidence motion, that the KRF would support any feasible censure motion, and that the Progressives would join any tax-related or general motion pressed by the Conser-

vatives. The parties in question had, in essence, precommitted themselves to these strategies.

The agenda reconstructed in Figure 1 had been set by the previous bargaining between the three coalitional parties. The sequence of moves is as follows. The Progress party first decides whether to accept the farm bill amendment. If it does, the government is defeated (outcome I). If, on the other hand, the Progress party rejects this no-confidence motion, the Brundtland government's fate depends on the further moves by the other bourgeois parties. The Conservatives then have to decide, presumably after consultations with the KRF, whether or not to press their own motion of no confidence, based on the national budget. If they do not, the game ends at outcome IV. If they do press a second no-confidence motion (and assuming the support of the KRF and the Progressives), the Center party has a second opportunity to dislodge Brundtland (outcome II). If the Center party rejects the second (Syse) censure motion, however, the outcome is III.

Figure 1 represents this game in its simplest form by assuming perfect and certain information. The expectations concerning the outcome of this endgame are derived from simple noncooperative game models. The equilibrium concept is subgame perfect Nash, which implies that in equilibrium, no player has any incentive unilaterally to deviate from his or her strategy and that this condition applies in every subgame, as well (see Fudenberg and Tirole 1991; Rasmusen 1989). In the extensive-form representation in Figure 1, solutions are found through backward induction from the end nodes (outcomes I–IV).

Party Objectives

In order to solve this game, we need to stipulate how each party evaluates each of the outcomes to which each of its choices may lead. It is clearly desirable to derive these preferences from general assumptions about the motives ("objective functions") of the players (here, the relevant Norwegian party leaders). For this purpose, I rely on three common and parsimonious models of competitive party behavior, derived from the literatures on electoral competition and coalition formation: the office-seeking party (O), the policy-seeking party (P) and the vote-seeking party (V) (Strøm 1990a). The office-seeking party seeks to maximize its control of public office and the spoils it derives from such control (see Leiserson 1968; Riker 1962). The policy-seeking party instead maximizes its impact on public policy (Axelrod 1970; Budge and Laver 1986; De Swaan 1973). Finally, the vote-seeking party seeks to maximize its support in future electoral contests (Cox 1987; Downs 1957). We can give these general models concrete meaning in this Norwegian case by specifying the aspects of the various outcomes that are relevant to each party under each behavioral assumption. This specification is obviously a tall order, and I again simplify as much as possible.

Policy Preferences

If parties were pure policy seekers, what would be the critical aspect of the game outcome? As shown in Table 1, the only issue clearly riding on the June 1987 negotiations was agricultural policy. On 6 June, Center party leader Johan J. Jakobsen had taken pains to stress that a potential nonsocialist government should not be expected to pursue economic policies significantly different from those of its predecessor (Rommetvedt 1991, 196). Hence, I assume that policyseeking parties would vote according to their preferences on the farm bill and that they would be indifferent between outcomes that are equivalent as far as farm policy is concerned.8 Other issues enter their calculations only secondarily, in the sense that if forced to vote on such issues (e.g., the national budget), parties (strictly) prefer voting in accordance with their program to voting against it.9

Assumption P1. The Center party prefers any outcome in which its farm bill amendment passes to any outcome in which it fails. Thus, it prefers outcome I to all others. Secondarily, if forced to vote on its national budget policy, the party prefers supporting this motion to opposing it. Hence, the Center party prefers outcome II to outcome III.

Assumption P2. The Conservative party and the Progress party prefer any outcome in which the farm bill amendment fails to any outcome in which it passes. Thus, they prefer outcomes II—IV to outcome I.

Office Preferences

The implications of an office-seeking model are more straightforward. I assume that Brundtland's resignation would lead to the formation of a three-party bourgeois cabinet and that she would not resign voluntarily. Under these assumptions, the three potential coalition partners prefer office to opposition. The Progress party, which would not gain office under any circumstances, is indifferent.

Assumption O1. The Center party and the Conservative party prefer any outcome in which the Brundtland government resigns to any outcome in which it does not. Thus, they prefer outcomes I and II to all others.

Assumption O2. The Progress party is indifferent between outcomes in which the Brundtland government resigns and outcomes in which it does not. Thus, this party is indifferent between all outcomes.

Electoral Preferences

Finally, assume that parties are motivated strictly by future electoral success. If so, we need to look for explanations of their behavior in the anticipated electoral consequences of different strategies. Specifically, different legislative strategies may entail individual or collective electoral costs. Individual costs result from policy inconsistency, that is, from voting against party program or previous commitments.

Collective electoral costs are a more complex matter. The Norwegian electorate apparently contains a significant number of sophisticated nonsocialists whose first preference is a minor party, such as the Progress party or the Liberals. Yet these voters may be induced to vote strategically for one of the three "coalitional" parties if and only if these parties jointly present a credible alternative to a Labor government. Otherwise, these individuals vote sincerely for the smaller party closest to their policy preferences. 10 In other words, these voters can be persuaded to cast a governmental, rather than a partisan, vote if they believe that a governmental choice exists. The three coalitional parties therefore have a collective electoral incentive to enhance their credibility as a potential government. Hence, they should avoid any behavior-such as overt policy disagreements-that detracts from this credibility. The Progress party, on the other hand, should for electoral purposes seek to provoke precisely such conflict.¹¹ Hence, the following assumptions go with the vote-seeking model.

Assumption V1. The Center party prefers any outcome in which it does not vote against its previous policy to any outcome in which it does. It also prefers voting consistently with the other coalition parties to voting contrary to one or both of them. Consequently, the Center party prefers all other outcomes to outcome III.

Assumption V2. The Progress party prefers any outcome in which any subset of the Center party, the KRF, and the Conservative party vote inconsistently with each other to any outcome in which they all vote consistently. Hence, it prefers outcome III to all others.

Assumption V3. The Conservative party prefers voting consistently with the Center party and the KRF to voting inconsistently with one or both of them. Hence, the Conservative party prefers all other outcomes to outcome III.

Table 1 summarizes the critical information about each possible outcome of the endgame. Table 2 identifies the equilibria, with the first three rows representing the assumptions of the respective models. Three general observations can be made: (1) under two of three models, there are multiple equilibria, (though in neither case more than two), which reflect assumptions of indifference; (2) different behavioral models yield different equilibria, only one outcome (II) being in equilibrium under all behavioral assumptions; and (3), most tellingly, no model predicts the actual outcome (III).

Since these simple assumptions obviously do not adequately explain the Presthus debacle, we need to introduce richer and more plausible stipulations. To avoid the multiple equilibria caused by indifference, let parties be motivated by not just one but two—or all three—of the objectives we have discussed. To retain as much simplicity as possible, I assume lexicographic preferences, wherein some objectives (e.g., office) dominate others (e.g., policy). In this example, policy preferences would only be decisive for outcomes between which a party is indifferent on office grounds. Stipulations 4–7 in Table 2 represent four

TABLE 1										
Properties of Different Outcomes of Endgame										
		OUT	COME	}						
PROPERTY	I	II	III	IV						
Government falls? Farm subsidies pass? Coalition parties agree? Center party policy-consistent?	Y Y Y	YNYY	N N N N	N N Y						

different models of lexicographic preferences. In each row, the party objectives, in abbreviated form, have been listed in declining order. With lexicographic preferences, the solution sets are reduced in most cases to a unique equilibrium (outcome II).

Persistent Puzzles

The problem, of course, is that outcome II failed to materialize on 12 June, 1987. The Center party did not vote for Jan P. Syse's motion of no confidence. And if the other parties could have anticipated this move, the whole solution unravels. Three puzzles thus persist: (1) Why did the Progress party oppose the farm bill amendment even though it turned out to be the only feasible way to give Norway the nonsocialist government the party explicitly favored? (2) Why did the Conservatives support higher farm subsidies, even though they ran counter to their program and failed to win the party either office or votes? and (3) Why did the Center party, after its farm bill amendment had been defeated, vote against a measure that would have put the party in government and prevented an acrimonious conflict with the other nonsocialists? I shall examine these questions in reverse order, exploring some additional considerations that can be incorporated in an extensive-form model. These considerations include interparty differences in objectives, incomplete information, and the effects of organizational arrangements involving party leaders in several simultaneous and interrelated games.

Equilibria Accord	ling to Dif	ferent Pa	rty Objec	tives
STIPULATED PARTY		OUT	COME	
OBJECTIVES	I	II	III	IV
1. Office (O)	X	X		
2. Policy (P)		Х	********	Х
3. Votes (V)		Х		Х
4. O > P		Х		****
5. O > V	X	Х		********
6. P > V		Χ	******	
7. $O > P > V$		Χ		***************************************

 $\it Note$: $\it X$ denotes a subgame perfect Nash equilibrium. Stipulations 4–7 assume lexicographic preferences.

SYMBOL	MEANING	ASSUMPTIONS
G	the value of a change of government	G_H , G_S , $G_F > 0$
F	the value of an amended farm bill	$F_{S} > 0; F_{H}, F_{F} < 0$
С	the electoral value of policy inconsistency	$C_i < 0$ for all i
K	the electoral value of nonsocialist conflict	$K_{\rm S}$, $K_{\rm H}$ < 0; $K_{\rm F}$ >
V	the expected value of the endgame if a farm bill amendment is rejected	_
W	the expected value of the endgame if a compromise no- confidence motion is rejected	_
р _{н,s}	the estimated probability of adoption of a no- confidence motion based on a farm bill amendment	$0 \le p_{H,S} \le 1$
q _{H,S}	the estimated probability of adoption of a no- confidence motion based on a policy compromise between the Center party and the Conservatives	$0 \le q_{H,S} \le 1$ $q_{H,S} \ge p_{H,S}$
r _F	the estimated probability of adoption of a no- confidence motion based on a policy issue chosen by the Conservatives	0 ≤ r _F ≤ 1

COMPLEX PARTY OBJECTIVES

Initially, I assumed that all parties had identical goals: policy, office, or votes. I later relaxed that assumption so as to permit lexicographic preferences. However, I still assumed that all parties had the same objective functions. In order to grasp the June 1987 events, we need to move beyond these simplifications and to consider (1) how different goals may be traded off against each other and (2) how parties differ in the weight they give to different objectives. Consider the case in which political parties place positive value on all three "goods"-votes, office, and policy (see Strøm 1990a)—and in which trade-offs may have to be made between different goods. Table 3 defines the parameters of the endgame under the assumption that parties have objective functions that include office, policy, and votes. These parameters simply

TABLE 4				
Payoff Matrix	for Presthus En	dgame	!	
	O	JTCON	ИE	
PARTY	I	II	III	IV
Progress Conservative Center	$G_F + F_F + C_F$ $G_H + F_H$ $G_S + F_S$	G _F G _H G _S	K _F K _H C _S + K _S	0 0 0

formalize objectives we have already discussed. A change of government in itself has a positive value G for both the Conservatives and the Center party. Passage of the farm bill amendment has a policy value *F* that is positive for the Center party but negative for the Conservatives. There is an individual electoral cost C of policy inconsistency and a collective cost K, to the coalitional parties, of policy disagreement. The payoff to each party is a function of these four parameters. Table 3 also includes the Progress party, which (as before) values nonsocialist conflict positively and the farm bill amendment negatively. I make one modification in stipulating an instrumental value to the Progress party of a change of government (a positive G). Although a nonsocialist government would yield no direct office benefits for the Progressives, it would in all probability pursue policies more favored by the party. The remaining parameters in Table 3 refer to the prior bargaining game, which I shall discuss later. With this notation, we can represent the payoff structure of the endgame, as in Table 4.

Intraparty Aggregation and Constraints

In order to understand the apparent irrationalities in party behavior, we now have to focus on intraparty constraints. So far, I have discussed parties as if they were unitary actors, a common simplification in the analysis of coalition politics (see Laver and Schofield 1990). In reality, parties are complex organizations of individuals with different preferences over policy, office, and votes. Organizational rules determine which of these preferences get reflected in party policy. Coalition bargaining decisions have the analytical advantage of directly involving only a small elite of leaders, since parties typically delegate such decisions to their leaders because of shortages of time and information. In such coalition negotiations, party leaders act as agents for their respective parliamentary or extraparliamentary parties (their principals). The trade-offs that these leaders make between different goods (and thus the coalition agreements they are willing to accept) depend on three factors: (1) the feasible outcomes and payoffs, (2) the "contracts" between party leaders and their respective principals (parliamentary parties or executive committees), and (3) the private rewards to the leaders from the various outcomes.

Delegation Regimes and Party Organization

Variation in intraparty delegation regimes is a particularly interesting determinant of bargaining behavior. To prevent their leaders from strictly pursuing their private rewards, parties hold them accountable through contracts and performance standards, where the ultimate sanction for poor performance is loss of office. As Luebbert observed, "Party leaders are motivated above all by the desire to remain party leaders" (1986, 46), and intraparty incentives powerfully constrain their behavior in coalition bargaining. Yet designing an effective contract for their leaders is no small matter, and parties sometimes unintentionally give their leaders incentives to behave contrary to the party interest. In the Presthus debacle, the delegation regimes of both the Center party and (particularly) the Conservatives ultimately backfired.

Most Norwegian parties share a similar organizational structure (see Strøm 1993). The annual (Conservatives, Progressives) or biennial (Center party) national congress (landsmøtet), which normally meets in the spring, is the highest formal authority within the party. Between congresses, leadership is officially exercised by two party committees elected by the congress: the national council (landsstyret) and the executive committee (sentralstyret). Norwegian parties have two top leadership positions: a chair, elected by the party congress, and a parliamentary leader, elected by the party's parliamentary caucus (stortingsgruppe). In many parties, it is customary for a single person to hold both offices concurrently.

Despite their general similarities, the Center party, the Conservatives, and the Progress party presented somewhat different organizational features in 1987. The Conservative party traditionally had a strong parliamentary party accustomed to delegating critical decisions to its leader, whereas, particularly in the Progress party, the influence of the extraparliamentary leadership was stronger. More important, however, were the characteristics of the 1987 leaderships. Whereas the Center and Progress leaders were both long-term party chairs, as well as parliamentary leaders, Presthus had to share power with parliamentary leader Jan P. Syse. And whereas Jakobsen and Hagen were entrenched leaders with almost 10 years' tenure each, Presthus was still new and inexperienced and faced much more visible competition within his party. Internal constraints varied between the Center and Progress parties, as well. Hagen dominated his parliamentary delegation of two (himself and a military officer used to taking orders) much more effectively than Jakobsen could control his.

THE ENDGAME: MISDELEGATION

Let us now return to the decisions of the three critical parties in the endgame, with an eye toward the constraints each leader faced within his own party.

The Center Party: Throwing the Steering Wheel out the Window

The most striking puzzle in the Presthus debacle is the behavior of the Center party at the last decision node, where its choices were clear. If Jakobsen rejected the censure motion from the Conservatives and the KRF, his party would be left in opposition and susceptible to electoral repercussions (compare its payoffs under outcomes II and III in Table 4). The farm bill amendment had already been lost. In rejecting costless office benefits and embracing electoral liabilities, the party seems deliberately to have made itself worse off.

Yet this behavior was no accident. Immediately after Hagen's press conference, the Center party reiterated its opposition to any censure motion based on the revised national budget. The party's executive committee issued a similar proclamation the day before Hagen's press conference (Rommetvedt 1991, 206). At that time, Center party representatives explained their apparently uncooperative behavior as a way to force the Progress party to accept higher farm subsidies. If Hagen believed that the Center party would support a second no-confidence motion, they argued, he would have no incentive to accept higher farm spending. Game-theoretically, however, the Center party's posturing should not have been a credible threat. If ultimately forced to choose between outcomes II and III, the party would still have office as well as electoral incentives to vote for the noconfidence motion. Hence, Hagen could confidently call the Center party's bluff. Yet even before the final vote, neither Hagen nor the Conservative leaders apparently harbored much hope that Syse's no-confidence motion would pass. Even the Labor party taunted the Center party during the debate, essentially daring the party to bring down the government.

The key to the Center party's intransigence surely lies in intraparty constraints. Jakobsen's insistence on the farm bill amendment reflected the deep skepticism toward any Conservative coalition harbored by two members of parliament, Ragnhild Q. Haarstad and Lars Velsand. Haarstad and Velsand represented the so-called "Hedmark guerrilla," a vocal left-wing faction of the party. 12 Jakobsen, who himself favored a change of government, appears to have precommitted himself to this group in internal party negotiations. In the parliamentary caucus on 5 June, Velsand voted to accept the Labor party's farm bill. His eventual support for a censure motion was expressly conditional on the farm bill amendment. A censure motion on any other basis was unacceptable (Thomassen 1991, 33–34). ¹³ Haarstad was even more intransigent and proceeded to vote against her party's own (technically, Spilde's) no-confidence motion, a highly unusual occurrence in traditionally cohesive Norwegian parties.

Could Jakobsen have reneged on his commitment and thrown his weight behind the Syse no-confidence motion anyway? Jakobsen might well have faced sanctions from his parliamentary group or national executive committee in such an event. More importantly, however, a humiliating scenario might have ensued had he supported a second no-confidence motion. The bourgeois majority in the Storting was so slender (80 to 77) that it could tolerate only one defection, such as Haarstad's on the first censure vote. Had both Haarstad and Velsand opposed Syse's no-confidence motion, it would have failed even with Progress party support, and no one would have looked more ineffectual than Jakobsen.

Thus, the "Hedmark guerrilla" was, in fact, pivotal and Jakobsen may therefore have preferred the seemingly irrational strategy leading to outcome III for internal party reasons. The nested games in which the Center party leader was involved may account for his anomalous behavior (Tsebelis 1990). His impotence was caused by the pivotal parliamentary position of the "Hedmark guerrilla." This pair of parliamentarians were able to impose strict ex ante controls on their leader's negotiations with the Conservatives. By allowing Jakobsen virtually no discretion in bargaining, the Center party had essentially "thrown the steering wheel out the window." Such brinkmanship may have helped the party in bargaining with the Conservatives, but it came back to haunt the agrarians in the parliamentary debate on 12 June.

One additional consideration may have been involved in the Center party's decision. Quite possibly, Jakobsen did not think of himself as playing a one-shot game. If, indeed, he foresaw a series of interparty games, his concern may have been reputation (Calvert 1987; Kreps 1990). According to Rommetvedt one Center party parliamentarian remarked in retrospect that he would have preferred a different strategy (1991, 207). But once the party had insisted on the farm bill amendment, it had to stick with it: "The party had tied itself so tightly to the mast that it could not vote for the general motion of no confidence without losing credibility at later junctures" (p. 210). Jakobsen thus became a victim of his own precommitment (Elster 1984).

The Conservative Party: Incompatible Incentives

As much as the Progress party won the June 1987 showdown, the Conservative party was the loser. The party failed to dislodge Brundtland, poisoned its relationship with the Center party, and suffered badly in the polls. After 12 June, the political career of Rolf Presthus was effectively ruined. One could hardly imagine a less desirable outcome. Both critical Conservative decisions seem foolhardy in retrospect: acquiescence in the farm bill amendment, as well as the subsequent decision to press a second no-confidence vote. Why on earth did the Conservatives go down this path to disaster?

The answer surely involves incomplete information concerning the Progress party. Presthus' dilemma also directs our attention to the internal politics of the Conservative party. The Conservatives clearly realized the risk that if the Progress party balked, their acquiescence on the farm subsidy issue would be for

naught. Ideally, Presthus therefore wanted some guarantee of cooperation from either the Progress party or the Center party (concerning a potential, second censure motion) before accepting this agenda. Unfortunately, any explicit agreement with the pariah Progress party was ruled off-limits by his two would-be coalition partners. Hence, the Conservatives repeatedly attempted to extract assurances from the Center party that the latter would remain committed to bringing down Brundtland in a potential second round. ¹⁴ Conservative parliamentary leader (later prime minister) Jan P. Syse raised this issue on 7 June. But Jakobsen steadfastly refused to issue any such guarantee.

Under this uncertainty, the prospects seem to have been estimated differently by the various Conservative leaders. Former prime minister Kåre Willoch and Storting president Jo Benkow both opposed Presthus' course of action (Bakke 1990, 187; Rommetvedt 1991, 196; Willoch 1990, 404). And though he supported Presthus, Jan P. Syse seems to have pressed more insistently for a guarantee from the Center party. Did these experienced parliamentarians more correctly estimate how unlikely it was that the Progress party and the Center party would make the choices necessary to propel Presthus into the prime ministership?

Was Presthus a victim of wishful thinking? Perhaps. But according to Thomassen both Presthus and Syse recognized that the farm bill amendment was a huge gamble (1991, 35). Presthus apparently chose to accept big risks and to "swallow camels" in order to gain the prime ministership. He was willing to accept quite unpalatable policies and take substantial electoral risks in the hope of dislodging Brundtland. Rather than any personal idiosyncracy, however, this behavior reflected Presthus' "contract" with the Conservative party and, more specifically, the incompatibility between his incentives as an individual and the interests of the party.

After only about a year as Conservative leader, Presthus' star had already begun to fade. Though he had been elected in large part because of his perceived skills in promoting nonsocialist cooperation, he had failed to deliver the bourgeois government for which many Conservatives were hoping. A consummate insider, Presthus also had not proven to be an inspirational opposition leader. The media hounded him for his failure in the "autumn hunting season," as well as for his awkward style. The party congress in Tromsø, in early May 1987, made a nonsocialist government before the end of the spring session an explicit party goal, thus adding to the pressure on Presthus (Norvik 1990, 156; Thomassen 1991, 30–31; Willoch 1990, 403–4). Time was already running out for Presthus, and he knew it. The party had defined recapturing executive office as his overriding concern. Any outcome short of this goal would bode ill for Presthus' political future. On the other hand, the party had not properly defined the highest acceptable price of office.

Thus, in considering the second censure motion, Presthus may not have attached much value to the added electoral liability (K_H) of outcome III, as com-

pared to outcome IV. Either way, the party would remain in opposition and be electorally damaged. Presthus' leadership was also likely to come to an end. But if Presthus was thus essentially indifferent between outcomes III and IV, then any infinitesimal chance that the Center party would "tremble" and support the Syse censure motion would lead Presthus down this path (Selten 1975). In fact, the negotiators from the Conservative party and the KRF took care to maximize the costs to the Center party of rejecting Syse's proposal (Rommetvedt 1991, 208).

The Conservative executive committee members may not have shared Presthus' preferences, but with customary deference they allowed him the rope by which he would come to his political demise. Presthus' gamble was approved by the party's executive committee, as well as by its parliamentary caucus. In fact, in the parliamentary caucus, only three out of 50 members were opposed (Bakke 1990, 188; Norvik 1990, 158). 16 By tradition, the Conservative party's control of its leaders has taken place mainly through ex post accountability. In the Norwegian Conservative party (as among tories elsewhere), nothing fails like failure, particularly in the electoral arena. Predictably, after his gamble failed, Presthus' leadership of the Conservative party was doomed. While his complicity in the design to increase farm subsidies might have been forgiven, it was much more difficult to overlook the electoral damage he had inflicted. In other words, while Presthus might have been able to trade off policy goals for office with impunity, he could not get away with sacrificing the party's electorate.

Both Presthus and Jakobsen were thus constrained by their respective parties but in radically different ways that reflected interesting organizational differences in delegation regimes. Jakobsen was tightly policy-constrained ex ante and in reality had no choice but to oppose Syse's censure motion. Presthus enjoyed much more policy discretion but was constrained by his ex post office accountability to his party. He was expected to produce office benefits and knew that without results, he was eminently dispensable. Jakobsen, on the other hand, faced less obvious challenges to his leadership but was essentially powerless to make policy compromises. As we shall see, these differences in constraints were critical to the bargaining game between the two parties.

THE BARGAINING GAME: A WAR OF ATTRITION?

Having thus explored the seemingly irrational behavior of the Center party and the Conservatives in the endgame, we now turn to their preceding bargaining game, where the assumption of full information clearly does not apply. This game was over the policy content of the no-confidence motion, of which there were three possibilities: the policy favored by the Center party (the farm bill amendment), the policy favored by the Conservatives (possibly the national

budget), or some compromise. Each party had two feasible moves (or strategies): insistence and acquiescence. Insistence (I) meant accepting only a censure motion based on the party's own preferred policy (the farm bill amendment for the Center party and the national budget for the Conservatives). Acquiescence (A) meant accepting the other party's preferred no-confidence motion. There were four possible outcomes: mutual insistence (II) (in which case no censure motion is made), mutual acquiescence (AA), Center party insistence and Conservative acquiescence (IA), and Center party acquiescence and Conservative insistence (AI).

Neither the Conservatives nor the Center party had complete information about the preferences of the Progress party and thus could not predict Hagen's behavior with certainty. The Progress party, on the other hand, did not have full information about the other parties' preferences. Yet we can confine the uncertainty concerning each potential censure motion to a single party: the Progress party in the cases of the farm bill amendment and the potential compromise motion, and the Center party with respect to a motion chosen by the Conservatives.

Table 3 describes three probability terms (p, q, and)r) related to the likelihood of passage of the different potential censure motions. Lacking reliable information, the Center party and the Conservatives may have thought of the Progress party as having two possible types: one that would support the farm bill amendment and one that would oppose it. Absent any information to the contrary, I assume that the Center party and the Conservatives had common prior beliefs about the Progressives. Terms p and q represent these prior beliefs of the Center party and the Conservatives concerning the Progress party's preferences. Term p denotes the probability estimate of these two parties that the Progressives would support a censure motion based on a farm bill amendment (IA). (For IA, etc., see Figure 2.) Similarly, q is their (substantively less interesting) subjective probability that the Progress party would support a no-confidence motion reflecting mutual concessions by the Center party and the Conservatives (AA). Since AA could be no further from the Hagen's ideal policy than IA, q must be equal to, or greater than, p. For both parties, cell IA thus offers either an office bonus (with a policy outcome valued positively by the Center party and negatively by the Conservatives) with probability p or a substantial electoral liability with probability 1-p. These parties' beliefs about the value of p (the Progress party's "type") must therefore have played a critical role in their decisions. Finally, r is the Progress party's probability estimate that the Center party would support a no-confidence motion based on a policy issue chosen by the Conservatives (e.g., the national budget). Table 3 contains subscripts denoting the parties whose beliefs we thus characterize. I shall, in the remainder, dispense with these subscripts for simplicity.

These assumptions allow us to represent the initial bargaining game in strategic form as a two-by-two matrix, shown in Figure 2. The payoffs in each cell list

		CONSERV	VATIVE PARTY (H)
	-	Insist (I)	Acquiesce (A)
CENTER PARTY (SP)	Insist (I)	K_s,K_H	$p(G_s + F_s) + (1-p)V_s,$ $p(G_H + F_H) + (1-p)V_H + C_H$
	Acquiesce (A)	G _s +C _s , G _H	$qG_s + (1-q)W_{s+}C_s,$ $qG_H(1-q)W_H + C_H$

the row player (the Center party) first and the column player (the Conservatives) second. This representation makes one important simplification: the value of the endgame in the event of a farm bill amendment censure motion is given as an unidentified expectation V. The value of Via absigned morty emotion and

tion V. The value of V is obviously party-specific and derived from the endgame solution. Similarly, W denotes the value of an endgame following mutual acquiescence (i.e., policy concessions on both parts). (Under strategy sets II and AI, there would have been

no endgame.)

We can use our knowledge of the endgame to simplify these payoff functions. Assuming that Jakobsen's constraints and Presthus' incentive incompatibilities could be foreseen, we substitute the payoffs from outcome III for *V* in cell IA in the payoff matrix in Figure 2. For the continuation value W in cell AA, we substitute the payoffs from outcome IV of the endgame since, if Jakobsen had already acquiesced in the first round, there would have been no basis for a second censure motion. Consider next the payoffs to the Center party and the Conservatives if the Progress party's "type" had been known. Figures 3 and 4 illustrate the two possibilities by setting p equal to 0 and 1, respectively. With p = 0, and assuming K $\geq C$ and 0 < q < 1, both parties have the same preference ordering over the four outcomes: AI > AA> II > IA. Note that the actual outcome is the worst possible result for both parties. In this game, both parties have dominant strategies. The Center party will acquiesce, and the Conservatives will insist. If, on the other hand, p = 1 and q = 1, then the Center party has the preference ordering IA > AA, AI > II, and the Conservatives, AI > AA > IA > II. This is essentially a game of chicken.1

But the one-shot, simultaneous-move nature of chicken is not the best representation of the bargaining game between the Center party and the Conservatives. Since their bargaining proceeded over a considerable period, the war of attrition is a more

accurate model. The war of attrition is essentially chicken "stretched out over time." Each player can in each time period choose to stay (insist) or exit (acquiesce). The last player to stay earns a reward, but until all but one exit, all who stay pay a penalty in each period (Fudenberg and Tirole 1991, 119–26; Rasmusen 1989, 74–75). A continuous-time version of this game also has the advantage of effectively eliminating the implausible outcome AA, that is, both parties' acquiescing simultaneously.

Chicken has two equilibria in pure strategies: IA and AI. In the two-player chicken or war of attrition, II (mutual insistence or intransigence) is the worst outcome for each player, and in any pure-strategy equilibrium, one player insists and the other acquiesces. The game itself, however, does not provide much of a clue as to which player will acquiesce ("swerve"). And if the players can adopt mixed strategies, the strategy set II occurs with regularity (Rasmusen 1989, 74). The war of attrition also has mixed-strategy equilibria, in which each player exits (acquiesces) with a constant probability at each move as long as the opponent has not already done so. Contrary to the regular war of attrition, however, in the Presthus bargaining game, the penalty for mutual intransigence rose dramatically toward the end of the parliamentary session, when the opposition parties suddenly would have had to wait four months and go through local elections before they would have another shot at the government. As the end of the session approached, the incentives to acquiesce therefore increased dramatically. It is no surprise, therefore, that the Center party and the Conservatives reached agreement on a censure motion only two days before the summer recess.

Why Presthus Blinked

The Center party took a tough bargaining stance vis-à-vis the Conservatives. Its precommitment to

Bargaining Game Payoffs by Progress Party Type: Farm Bill Amendment Rejected (p = 0) CONSERVATIVE PARTY (H) Insist (I) K_{s},K_{H} $C_{s}+K_{s},C_{H}+K_{H}$ CENTER PARTY (SP) Acquiesce (A) $G_{s}+C_{s},G_{H}$ $qG_{s}+C_{s},qG_{H}+C_{H}$ Note: Subscripts denoting parties represent the initials of their Norwegian names: H = Conservative party, S = Center party.

intransigence is a time-honored strategy in chicken and the war of attrition. If one player can find a way to move first (e.g., through credible precommitment), then he can safely insist, knowing that the second player, on observing this behavior, can do no better than to acquiesce. The fact that the Center party (and perhaps, less successfully the Conservatives) so clearly adopted this strategy suggests a belief that p was significantly larger than 0. Yet, credible precommitments in chicken are difficult to make, and they can be disastrous if they fail to deter (Taylor 1987, 45-49). Every player has an incentive to bluster, and credible commitments may require dramatic measures ("throwing the steering wheel out the window"). While Jakobsen's tough stance probably helped the Center party prevail in the bargaining game, it did not deter the Conservatives from embarrassing him with a second censure motion in the endgame.

What other factors might account for the Center party's success against the Conservatives in the bargaining game? Two factors suggest themselves: (1)

the credible policy constraints on Jakobsen versus the wide discretion given to Presthus and (2) the greater impatience (higher discount rate) of Presthus, due again to his personal incentives. The resistance Jakobsen faced from the "Hedmark guerrilla" helped make his threats credible. Conservative leaders presumably knew of this opposition, as it was in the Center party leader's interest to inform them that his hands were tied. Presthus' greater impatience contributed to the same outcome. In the war of attrition, players' ability to hold out generally depends on their discount rates. All else equal, the player with the higher discount rate (i.e., the more impatient one) is more likely to acquiesce early. The Conservative party congress in May had done Presthus a disservice by publicly requesting a coalition before the parliamentary recess. Center party negotiators knew that Presthus had the latitude to make policy compromises and that he needed a quick fix. These circumstances eventually doomed the Conservatives in their negotiations.

		CONSERVATIVE PARTY (H)	
		Insist (I)	Acquiesce (A)
CENTER	Insist (I)	K _s ,K _H	$G_s + F_s$, $G_H + F + C_H$
PARTY (SP)	Acquiesce (A)	G _s +C _s , G _H	$G_H + C_S$, $G_H + C_H$

THE PROGRESS PARTY: THE VALUE OF PRIVATE INFORMATION

Clearly, the Progress party was the greatest unknown factor in the Presthus debacle. Guesses about the preferences of the Progress party were therefore critical to the calculations of the other parties. Such private information as the Progress party had about its own "type" can be a source of considerable power. Carl I. Hagen exploited this advantage to the hilt, playing his cards extremely close to his chest. The public statements that Hagen made prior to his dramatic press conference on 11 June were studiedly ambiguous. When it became increasingly clear that the farm bill would be the basis of the no-confidence motion, Hagen tempered his enthusiasm about the bourgeois coalition. On 6 June he admitted to doubts about the three-party coalition, citing internal differences and the lack of a clear policy alternative to the Labor government (Rommetvedt 1991, 196). Two days later he reiterated his opposition to increased agricultural transfers, yet the following day he told journalists that his party might well vote against the government's farm bill anyway.

In his equivocation, Hagen kept stressing the need to consult his party's national and regional officers, many of whom were known to oppose any policy compromise. And in Hagen's public justification of his final decision, the voice of his party's activists had a prominent place. Were Hagen's hands, then, tied by his organization due, perhaps, to his party's commitment to reducing transfers to Norwegian agriculture? Not likely. Hagen's personal authority in his party was immense, and on a similar issue in 1985, he had, without serious repercussions, voted against his party's program in order to save Willoch. Hagen's deference to his party's officers was therefore probably more of a rationalization than a rationale.

What was fundamentally at stake for the Progress party? Consider Table 4, which shows the endgame payoff matrix. Assuming that Hagen correctly anticipated the eventual result of opposing Spilde's censure motion (outcome III), then the Progress party's decision hinged on the magnitude of its benefit from nonsocialist conflict relative to the three other terms. Specifically, the party should vote for the censure motion if the net benefit of a change of government minus the cost of an amended farm bill and the electoral cost of voting contrary to its program exceeded the party's electoral rewards from conflict between the other nonsocialist parties. Hagen's strategy, then, would be to vote yes if and only if G_F + $F_F + C_F \ge K_F$. Before 11 June, presumably no one outside the Progress party knew whether this condition held. But although the Progress party kept Norwegians in suspense about its intentions, one thing was perfectly clear. If the government's farm bill and the no-confidence motion were separated, the Progressives would support both and thereby give the country a bourgeois cabinet without higher agricultural spending. Hagen had committed his

party as early as 29 May by declaring that the Progress party would support a Conservative censure motion (AI) (Rommetvedt 1991, 191). As it turned out, Hagen would not have the luxury of separating these two issues. Until 11 June, however, he remained noncommittal concerning the farm bill amendment (IA) favored by the Center party.

In all likelihood, Hagen's precommitment to no confidence on the revised national budget was designed to strengthen the Conservatives in their negotiations with the Center party. If the Conservatives could prevail in their bargaining, then the Progressives would have their most preferred outcome. But why did Hagen not further boost the bargaining power of the Conservatives by fully revealing his type, that is, by clearly rejecting a farm bill amendment? The Progress party, after all, had both electoral and policy incentives to vote against Spilde's censure motion. One possibility is that the Center party might not have believed such a threat. After all, Hagen had an incentive to engage in cheap talk of this kind

regardless of his actual intentions.

But equivocation may in fact have been Hagen's best strategy. At first glance, this seems incompatible with the claim that a no-confidence vote based on the Conservatives' policy choice would have been the Progress party's most preferred outcome. But Hagen may not have been convinced that if he revealed his "type," the Conservative censure motion would in fact pass. That outcome would hinge on whether the Center party leadership could convince at least one of the two "Hedmark guerrillas" to go along. Assume that Hagen attached a probability of r to this outcome. Alternatively, if Hagen had revealed his private information and the requisite Center party support had not been forthcoming, the result would essentially have been a null event. The Center party and the Conservatives would both have had an incentive to paper over their differences, no censure motion would have been made, and no policy would have been affected. I therefore attribute a subjective probability of 1 - r and a 0 value to this outcome for the Progress party. The Progress party should rationally only reveal its type if $rG_F \ge K_F$, that is, if the value of a nonsocialist government times the probability that the Center party would bring it about at least outweighed the electoral benefit that the Progressives could draw from a coalitional debacle. If Hagen's estimate of r was low, then he had an incentive deliberately to mislead the coalitional parties down the garden path.

Hagen's electoral considerations (K_F versus C_F) were in themselves probably something of a lottery. If he accepted increased farm subsidies, he stood to suffer a predictable loss of votes among his libertarian supporters. A poll published on 11 June showed little popular support for the farm bill amendment. Only 11.7% of the voters thought this was an appropriate issue on which to defeat the government, and even among nonsocialists, two-thirds considered this a contrived way to generate a cabinet crisis (Rommetvedt 1991, 205). If, on the other hand, Hagen made the much more dramatic gesture of opposing the bourgeois no-

confidence motion, the electoral fallout would be much more unpredictable. In a similar situation in 1963, when the Socialist People's party brought down Einar Gerhardsen's Labor government, it suffered stinging losses among voters incensed at its complicity in the formation of Norway's first postwar bourgeois government. Hagen, however, gambled on his ability to frame the issues in such a way that he would appear the principled and responsible statesman rather than the traitor to the nonsocialist cause. Given the party's precarious electoral position, the Progressives could ill afford even a small but certain electoral setback. Even a drop of .5% in the national poll could have deprived the party of its parliamentary representation. It was better, then, to gamble at the high-stakes tables. His party's weakness among the voters made Hagen highly risk-acceptant over electoral outcomes and willing to take his case to the people. In this endeavor, he was amazingly successful, probably beyond his wildest imagination.

CONCLUSION

I have examined a spectacular case of coalition bargaining failure in recent Norwegian history. The Presthus debacle of 1987 is not easily accounted for by traditional explanations based on cooperative coalition theory. Through an alternative extensive-form game model, I have analyzed the ill-fated strategies of the protagonists and teased out their motivations. This mode of analysis allows us to consider the implications of asymmetric information, variations in objective functions, structured versus unstructured bargaining environments, the sequence of bargaining, time preferences, and internal delegation regimes in political parties.

Whereas Hagen was seeking electoral gains and Jakobsen farm subsidies, Presthus was primarily concerned with office payoffs. These differences in party behavior were in large part a function of intraparty politics. The Presthus case offers us no happy medium between the Scylla of strict ex ante policy constraint (Jakobsen) and the Charybdis of ex post office accountability (Presthus). Both the Conserva-

tives and the Center party featured delegation regimes ill suited to the bargaining situation in which they found themselves, and much of the responsibility for the Presthus debacle rests with these internal structures of Norwegian political parties. The lack of similar constraints in the Progress party made it possible for Hagen to place greater weight on electoral objectives and to take substantial risks. Hagen's humiliation of Jakobsen and especially of Presthus demonstrates the value of private information. His control over private information enabled him to entice the coalitional parties into behavior that was ultimately self-destructive. And Jakobsen's tough bargaining stance highlights the attractions and perils of precommitment.

Even without misdelegation or deliberate manipulation, however, the bargaining situation faced by Presthus and his associates invited disaster. The game of chicken (played with mixed strategies) occasionally produces outcomes that are catastrophic for everybody. The war of attrition, which the Presthus debacle even more closely resembles, can have similarly unpleasant results. Moreover, misperception and bargaining failure were particularly likely in the Presthus debacle due to the impending summer recess. The closer the end of the session drew, the higher the stakes, until the flurry of frenzied activity in the last few days before the "guillotine" fell. Such frantic times have a way of producing ill-considered behavior, a phenomenon well known from congressional budget debates in the United States (Cox and Kernell 1991).

Thus, the calendar, information, and structure are important keys to the dismal fate of Rolf Presthus and his would-be coalition partners. Given time, knowledge, and appropriate incentives, the story might have been a happier one for Presthus, Jakobsen, and Bondevik (and less triumphant for Hagen and Brundtland). Although, sadly, the main protagonist cannot now benefit from this knowledge, the student of coalition behavior can. Bargaining failures can offer us valuable insight into party behavior, and noncooperative game theory affords us useful tools with which to explain the dramas that elite-level politics occasionally produces.

APPENDIX

TABLE A-1				
Players in the Presthus Debacle				
PARTY AND INDIVIDUAL	POSITION			
Labor Gunnar Berge Gro Harlem Brundtland	finance minister prime minister			
Conservatives Jo Benkow Rolf Presthus Arne A. Spilde	president of the Storting party chair MP, member of Agriculture Committee			
Jan P. Syse Petter J. Thomassen	parliamentary party leader MP, former minister of industry (1985–86)			
Kåre I. Willoch	former prime minister (1981–86)			
Christian People's party Kjell M. Bondevik	party chair and parliamentary leader			
Center party Ragnhild Q. Haarstad	MP, member of "Hedmark			
Johan J. Jakobsen	guerrilla" party chair and parliamentary leader			
Anne E. Lahnstein Lars Velsand	MP, deputy party chair MP, member of "Hedmark guerrilla"			
Socialist Left party Hanna Kvanmo	parliamentary party leader			
Progress party Carl I. Hagen	party chair and parliamentary leader			

Notes

I am grateful to the American–Scandinavian Foundation for its generous funding of this research through a King Olav V Fellowship. My thanks also to Paul R. Abramson, Scott Gates, John D. Huber, Herbert Kitschelt, Arend Lijphart, Arthur Lupia, Carol A. Mershon, Wolfgang C. Müller, Sharyn O'Halloran, Bjørn E. Rasch, Steven S. Smith, Ruth M. Sylte, George Tsebelis, Henry Valen, and especially Gary W. Cox for helpful comments on earlier drafts; to Jennifer Oh for assistance; and to Hilmar Rommetvedt for graciously making his personal notes and files available to me. Yet my greatest intellectual debt is to William H. Riker, who opened our eyes to the intrigue and logic of coalition bargaining. I dedicate this article to his memory.

- 1. The 1965–71 cabinet, headed by Per Borten of the Center party, was a four-party coalition, whereas the 1983–86 government failed to include the Liberals (Venstre). The Liberals lost their parliamentary representation in 1985 and were therefore not a relevant party in the events described here.
- 2. I use the terms nonsocialist and bourgeois interchangeably. This usage is consistent with Norwegian convention and is neither derogatory nor indicative of a class-analytic perspective. In fact, the nonsocialist parties frequently apply the label bourgeois (borgerlig) to themselves.
- One member of the Center party, Ragnhild Queseth Haarstad, voted against her party's position on this bill. After

disposing of Spilde's no-confidence motion, the Storting overwhelmingly, by a vote of 123 to 34, rejected a motion by Arent Henriksen of the Socialist Left party to increase agricultural appropriations.

4. Here, as elsewhere, I am responsible for all translations

from the Norwegian.

5. It might seem that one or more additional no-confidence motions could follow end node III. As the argument will show, however, outcome III implies that no such motion would pass. Since I assume that the coalitional parties bear an electoral cost if they propose divisive no-confidence motions, we can disregard any further agenda.

6. Perfect information implies that each player knows (1) who the other players are, (2) the set of actions available to each player, (3) all potential payoffs to each player, and (4) all previous moves. Certainty means that the outcome is not affected by random, unpredictable events ("moves by Nature") that take place after the players have made their choices

(Rasmusen 1989, 51).

- 7. A Nash equilibrium is a set of strategies from which no player has an incentive to deviate as long as no other player does. In other words, no player can make himself better off by unilaterally changing his strategy. The Nash equilibria identified in Figure 1 are subgame perfect because they satisfy the requirements of backward induction. On subgame perfection, see Selten 1975 and Rasmusen 1989. I also assume here that all players adopt pure strategies, that is, that no player's strategy is a lottery over several feasible moves. This assumption seems both plausible and parsimonious here.
- 8. All preference relations are strict unless otherwise indicated.
- 9. This does not imply that the Center party or the Conservatives prefer voting for their respective programs to not voting. Hence, the Conservatives have no *policy* incentive to introduce the Syse motion.
- 10. Such voters may base their decisions on preferences over final policies rather than preferences over parties (or candidates) per se. Austen-Smith and Banks (1988) argue that policy preferences are the more theoretically satisfactory assumption in models of multiparty competition and use this stipulation to generate strong analytical results.
- 11. This interpretation is corroborated by the 1987 events. Polls taken immediately after the Presthus debacle showed marked declines for the Conservatives (from 30.2% to 27.1%), as well as the Center party (from 5.9% to 5.2%). The KRF, which had been conciliatory throughout, registered a modest gain (8.1% to 8.4%). But the big winner was the Progress party, which lept from 5.1% to 7.1% (Rommetvedt 1987). These figures represent the means of polls by several authoritative firms. The preevent poll figures are the averages of polls by Norsk Gallup, Markeds- og Mediainstituttet, and Scan Fact. The postevent figures represent the means of polls by Norsk Gallup and *Opinion*. See Rommetvedt 1987 for details. The Progress party later continued to surge, reaching a stunning 12.3% of the national vote in the September regional elections, more than twice its poll in any previous election.
- 12. This unofficial faction takes its name from the province of Hedmark, an agricultural area in east central Norway and the constituency of Ragnhild Q. Haarstad. The province is a traditional stronghold of the Center party.
- 13. In his memoirs, Petter Thomassen reports that the internal debate in the Center party was so intense that Jakobsen wept when Velsand came around to supporting the farm bill amendment (1991, 33). Thomassen is a leading Conservative who served as minister of industry in one of the Willoch cabinets (1985–86).
- 14. Bente Bakke, a maverick Conservative backbencher, reports that the need for a precommitment from the other coalitional parties was strongly felt within the Conservative parliamentary caucus. After the failed "autumn hunting season" the previous year, parliamentary leader Jan P. Syse had reportedly assured the members that no further assaults on the Brundtland government would be attempted without the

binding agreement of both the KRF and the Center party (Bakke 1990, 179-86).

15. Jan P. Syse later replaced Presthus as chair of the Conservative party. In October 1989 he became prime minister in charge of the same three-party coalition that Presthus had tried to establish. Syse's coalition, which was fragile from the very beginning, broke down after only one year in office.

16. The three skeptics were a prestigious group, however. They included Willoch, Benkow, and one backbencher.

17. The payoff structure in this game would have been true chicken had the Center party strictly preferred AA to AI. The party's indifference between these two outcomes makes no difference to the game's equilibrium, however.

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THE POWER OF THE EUROPEAN PARLIAMENT AS A CONDITIONAL AGENDA SETTER

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he European Parliament under the current cooperation procedure has an important power: it can make proposals that, if accepted by the Commission of the European Communities, are easier for the Council of Ministers to accept than to modify, since only qualified majority is required for acceptance, whereas full unanimity for modification. The importance of this power, which I call the power of the conditional agenda setter, has not been recognized in previous scholarly work. For structural reasons explained in the text, this power is likely to increase in the future. I conclude by arguing that the conditional delegation of power to international actors (the European Parliament, Commission, and the Court of Justice) is a frequent phenomenon in European institutions. This delegation presents three important advantages: it makes possible the selection of one among many possible equilibria, it accelerates European integration, and it diffuses responsibility for politically unpopular measures.

he European Parliament (EP) is widely considered a weak parliament (Dehouse 1989; Edward 1987; Fitzmaurice 1988; Lenaerts 1991; Lodge 1989; Wessels 1991). Moreover, discussions are frequently advanced about the "democratic deficit" of European institutions, which, among other things implies a weakness of the EP (Bogdanor 1989; Bowler and Farrell 1993; Thomas 1988; Williams 1991).

Yet in 1989, the EP, when confronted with the common position of the Council of Ministers specifying low standards on exhaust emissions for small cars, raised the standards and was able to impose its decision on both the Commission of the European Communities and a "reluctant majority" in the Council of Ministers (Jacobs and Corbett 1990, 170). According to the cooperation procedure established by the Single European Act treaty, the Council of Ministers can modify the position of the Parliament by a unanimous vote, but in this case the Council could not agree on any alternative. The legislation in question is far from trivial, since it affects around 60% of all passenger cars in the European Community (Kim 1992). Moreover, the differences in positions between the EP and the Council were significant: compared to the Council's position, the legislation adopted raised the price of small cars by more than five hundred dollars apiece and significantly improved the envi-

I shall explain this surprising power of the EP. I argue that under the current cooperation procedure, the EP has an important power: it can make proposals that, if accepted by the Commission, are easier for the Council of Ministers to accept than to modify (only qualified majority being required for acceptance but unanimity, for modification). I call this the power of the conditional agenda setter and study its properties.

My answer generates a second question: If the EP is able to influence the legislative process so significantly, why does it not do so all the time? Alternatively, if the conditional agenda-setting power is significant, the EP should have been recognized in the literature as a strong parliament. To address these issues, I shall specify the conditions under which the EP can make use of its agenda-setting power.

The European Community fascinates observers and scholars because it is a unique object of study. Accordingly, a series of neologisms have been invented to describe it. It is "neither a state nor an international organization" (Sbragia 1992, 257); "less than a Federation, more than a Regime" (W. Wallace 1983, 403); "stuck between sovereignty and integration" (W. Wallace 1982, 67); a "part formed political system" (H. Wallace 1989, 205); "institutionalized intergovernmentalism in a supranational organization" (Cameron 1992, 66), and the "middle ground between the cooperation of existing nations and the breaking of a new one" (Scharpf 1988, 242). Some scholars have even seen advantages in the situation. Krislov, Ehlermann, and Weiler claim: "The absence of a clear model, for one thing, makes ad hoc analogies more appropriate and justifiable. If one may not specify what are clear analogies, less clear ones may be appropriate" (quoted in Sbragia 1992, 258).

Instead of using (appropriate or inappropriate) analogies in this analysis, I examine the logic and the outcomes of decision making among the three institutional actors generated by the cooperation procedure. In this sense, my approach is part of a series of studies that attempts to apply the institutional approach developed through the study of American politics to the institutions of the community (Garrett 1992; Garrett and Weingeist 1993; Scharpf 1988; Weber and Wiesmeth 1991).

My results complement empirical studies. I explain why they find little influence for the EP in most cases. I explain why the EP in some instances (e.g., the automobile emission standards) was so influential. Finally, my analysis clarifies why cases involving high parliamentary influence are likely to multiply in the future.

The first section presents the rules of the cooperation procedure. The second section formally analyses the interaction among the three institutional actors (the EP, the Commission, and the Council of Ministers). The third section explores the implications of the analysis for the role of the EP. In the conclusion, I discuss the theoretical problem of specifying the driving forces of European integration in light of the results of my model.

THE INSTITUTIONAL FRAMEWORK

Three major institutional actors are involved in European decision making: the Council of Ministers, the Commission, and the EP. These actors interact according to quite complicated rules, and their decisions, along with those of the Court of Justice, supersede the national law of member countries. The Council is composed of the relevant ministers of the member countries (ministers of the environment for decisions concerning the environment, ministers of agriculture for agricultural matters, etc.). The Commission is composed of 17 members appointed by national governments for their competence and "whose independence is beyond doubt" and assures the everyday operation of the institutions of the Community. Finally, the EP is composed of 518 representatives elected (since 1979) by universal suffrage of the member countries of the community.

There are currently (before the application of the Maastricht treaty) three different legislative procedures in play: the assent procedure, the cooperation procedure, and the consultation procedure. Each of these attributes different powers to the three institutional actors. This is why, sometimes, a political and legal battle among the three actors takes place before the discussion of particular pieces of legislation in order to decide which procedure will be followed (Garrett 1992; Lodge 1987, 1989). I shall not discuss these institutional battles here. Instead, I shall focus on presenting the cooperation procedure.

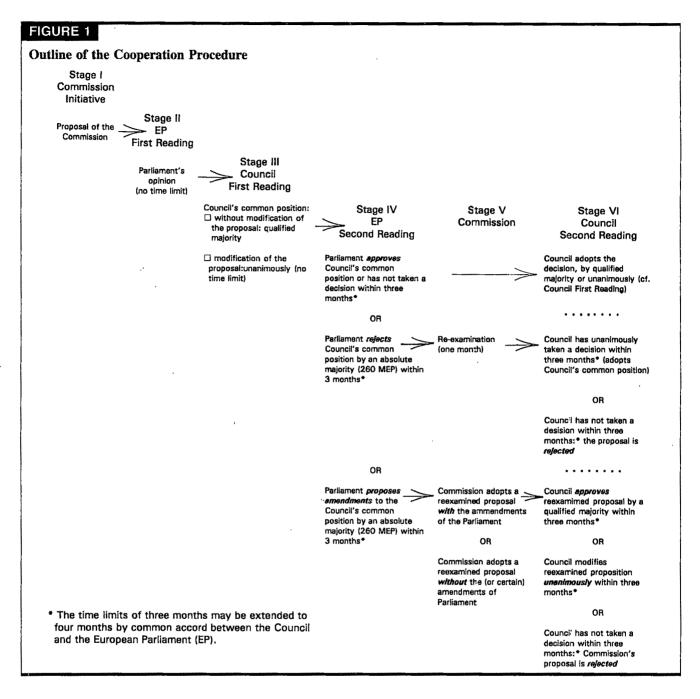
The cooperation procedure does not cover all areas of Community legislation (Jacobs and Corbett 1990, 169; Lodge 1989, 69). It applies to nine articles of the Rome treaty: prohibition of discrimination on the grounds of nationality (art. 7); freedom of movement of workers (art. 49); freedom of establishment (art. 54.2); coordination of provisions providing special treatment of foreign nationals on grounds of public policy, public security, or public health (art. 56.2); mutual recognition of diplomas and the like and coordination of provisions on activities of self-employed persons (art. 57.1-2); harmonization of measures for the establishment and functioning of the internal market (art. 100a and b); the working environment and the health and safety of workers (art. 118a.2); economic and social cohesion (art. 130e); and research and development (art. 130q). Of all these issues, the most important has been the harmonization of the internal market and then social policies, research programs, and regional fund decisions. Legislation examined under the cooperation procedure constitutes between one-third and one-half of parliamentary decisions (Jacobs and Corbett 1990, 169).

The cooperation procedure entails two readings of each piece of legislation (initially introduced by the Commission) by the EP and the Council of Ministers. The Council makes the final decision either by qualified majority or by unanimity. In the abstract, the procedure is reminiscent of a *navette* system between the two houses of a bicameral legislature, where the upper house (the Council) has the final word.² The European Community procedure is presented schematically in Figure 1.

The legislative process begins with the submission of a Commission proposal to the EP. At the same time, the Council may begin deliberating but cannot reach a decision until it receives the EP's position. The EP may, in the first reading, accept, amend, or reject the proposal; it may also withhold its opinion by referring the legislation back to committee, thereby effectively aborting the proposal. Once the EP decides, the proposal goes back to the Commission, who may revise the initial proposal to accommodate the EP. The Commission presents the proposal, as amended, to the Council, who adopts a "common position" by qualified majority (54 out of 76 votes). No time limits on deliberation exist in this first reading of the proposal. It is therefore obvious that any of the institutions can effectively abort legislation at this stage of the process.

Once the Council adopts its common position, the second reading of the proposal begins. The Council sends its common position back to the EP, along with a full justification of its adopting this position. The full justification of the Council's and the Commission's positions is required by article 149.2b of the Single European Act. However, in the early phase of application of the procedure, the Council provided extremely sketchy reasons or even no reasons at all. In one case, it even apparently failed to notice that the EP had tabled amendments to the Commission proposal (Bieber 1988, 720). Parliament formally protested, its president declaring on 28 October 1987 that "as a minimum, the Council should provide a specific and explained reaction to each of Parliament's amendments" (Jacobs and Corbett 1990, 173). On 18 November 1987, the EP, in two resolutions, threatened the Council with legal action (Bieber 1988, 720). As a result, the Council altered its approach and from then on it provided an account of its point of view on each of the substantive issues raised by draft legislation (Jacobs and Corbett 1990, 173).

Parliament then has three months to select one of three options: to approve the common position of the Council (or, equivalently, take no action), in which case the Council adopts the proposal; to reject the common position by an absolute majority of its members (currently 260 votes); or to amend the common position, again by an absolute majority of its members. In this second round, time is of essence. The clock starts when the president of the Parliament



announces the receipt of all relevant documents in all nine official languages.

The Commission may or may not introduce legislation rejected by the EP to the Council; if such legislation is introduced, the Council can overrule the rejection by unanimity. Amended legislation is presented to the Commission, who must revise the proposal within a month. Parliamentary amendments that are accepted by the Commission can be adopted by the Council by qualified majority (54/76), whereas any other version requires unanimity in the Council (Nugent 1989, 248). If the Council fails to act within three months (four, with the agreement of the Parliament), the proposal lapses.

This account makes the Commission appear as the agenda setter. Indeed, a proposal by the Commission

is required to initiate the legislative process, and it is the Commission's proposal that the Council accepts by qualified majority or modifies by unanimity. The EP or the Council have no right to initiate legislation. However, the Council was given the right under article 152 of the European Economic Community treaty to request that the Commission undertake studies and submit to it the appropriate proposals. Similarly, the EP, on its own initiative, has several times adopted resolutions calling for new legislative proposals (e.g., concerning the ban on imports of baby seal skins to the Community and transfrontier television broadcasts) (Jacobs and Corbett 1990, 181). In 1982, the Commission agreed in principle to take up any parliamentary proposals to which it did not have major objections; if it had objections, it would

undertake to explain its reasons in detail to the EP (ibid.). Nugent argues that it is difficult to establish the initial impetus for legislation and that even resolutions from the EP could have in fact originated with the Commission, which may have wanted to reinforce its own position vis-à-vis the Council (1989, 240). In any case, all three institutional actors can in fact place items on the legislative agenda.

Once discussion is initiated by a Commission proposal, there are no restrictions on the amendments that the EP can introduce in its first reading. There are, however, such restrictions on its second reading. Parliamentary amendments in the second reading are restricted not by the Single European Act but by the Parliamentary Rules themselves. According to art. 51.2, only amendments that concern the parts of the text that have been modified by the Council and that seek to adopt a compromise with the Council or to restore the EP's position in the first reading are acceptable during the second reading (Bieber 1988, 722). Further, only a committee comprising a group of at least 23 EP members may present amendments (Fitzmaurice 1988, 397). Did the EP tie its own hands with these rules? Nothing of the sort. These restrictions simply increase the efficiency of the EP during its second reading, since, substantively, they permit the adoption of any position in the interval between (and including) the EP's initial position and the adoption of the Council's common position.

There is, however, a very important restriction on the EP's second-reading amendment power. Amendments require absolute majorities to be adopted. In practice, the 260 required votes constitute a two-thirds majority of members present. Moreover, given both that the 518 EP members from the 12 countries are organized into more than 10 (cross-national) parliamentary groups and that voting alignments occur more frequently by political group and less frequently by country and also that voting discipline is weak, 260 votes is a stringent requirement. The most likely combination to achieve an absolute majority is a coalition of Socialists and Christian Democrats—the European People's party—that currently controls 301 seats.

To summarize, according to the cooperation procedure, in its second reading the EP can, by an absolute majority of its members, make a proposal that, if adopted by the Commission, can be accepted by a qualified majority (54/76) of the Council but requires unanimity of the Council to be modified. This proposal can be anywhere between the EP's and the Council's first reading of initial legislation, including a reiteration of the EPs previous position. Consequently, if the EP manages to make a proposal that makes the Commission and a qualified majority of the Council better off than legislation that can be voted unanimously, this proposal will be adopted by all institutional actors. If, however, such a proposal does not exist or if the EP cannot adopt one by an absolute majority of its members or makes the wrong choice, then the agenda is transferred into the hands of the Council, which can modify the EP's proposal

by unanimity. These conditions describe the power of the conditional agenda setter that is attributed to the EP by the cooperation procedure. I shall examine how this power has been used and then focus on its theoretical properties.

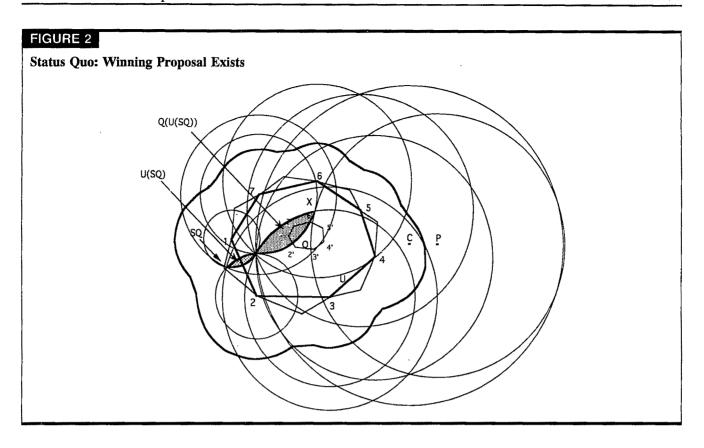
CONDITIONAL AGENDA SETTING

Agenda-setting players have power when it is impossible, difficult, or costly for decision makers to modify their proposals. Modification of proposals may be precluded by the prevailing institutions. For example, when the president of the United States nominates a candidate for the Supreme Court, the Senate cannot modify the proposal. In the first theoretical paper analyzing the importance of agenda control (McKelvey 1976), the agenda setter could make a series of proposals that would be voted under "closed rule," that is, without amendments. This agenda setter had quasi-dictatorial powers, being able to drive a society through a series of successive votes to select the agenda setter's ideal point. However, an agenda setter looses this power under open rule, because the proposals can subsequently be modified by amendments of the deciding body (Krehbiel 1987; Shepsle and Weingast 1987a, 1987b). Agenda setters also have power if the deciding body is impatient, that is, if it pays a price as long as there is no agreement. Impatience creates an asymmetry in favor of the proposal of the agenda setter and against its modifications (Baron and Ferejohn 1989; Tsebelis and Money n.d.).

The cooperation procedure presents a different mechanism for agenda-setting power. Regardless of impatience, it is more difficult for the Council to modify a Parliamentary proposal (provided it is accepted by the Commission) than to accept it. Indeed, qualified majority is needed for acceptance but unanimity for modification. This procedure may enable the EP to offer a proposal that makes a qualified majority of the Council better off than any unanimous decision. If such a proposal exists, if the EP is able to make it,⁴ and if the Commission adopts it, then the EP has agenda-setting powers. If, however, these conditions are not met, the EP looses its agendasetting power. This is why I characterize the EP's agenda power under the cooperation procedure as conditional.

I shall study the last reading of the cooperation procedure and provide necessary conditions for the existence of a winning EP proposal (i.e., a proposal that will be accepted by the Commission and a qualified majority of the Council). I will call such a proposal X.

In the Appendix, a series of definitions and elementary geometric properties necessary to analyze the powers of the conditional agenda setter are provided. Here, I shall provide a nontechnical account of the argument. Consider that the members of the Council are concerned simultaneously about two different issues.⁵ Figure 2 presents a graphic repre-



sentation of the position of the members of the Council. I assume that the Council is composed of 7 members, that a qualified majority of 5 is required for acceptance of the EP's proposal, and that the members of the Council have circular indifference curves (Euclidean preferences)—that is, they are indifferent between proposals of equal distances from their ideal points. I assume 7 members, instead of 12, in order to simplify matters while keeping a reasonable approximation of the qualified majority requirement. The cooperation procedure requires a 54/76 (.710) qualified majority, while the qualified majority I shall use here is 5/7 (.714).

Consider that the status quo is outside the heptagon 1–7 that represents the Council, as indicated in Figure 2. Consider also that the positions of the Commission and the EP are on the other side of the heptagon—points C and P, respectively. The points in the figure are selected so that the horizontal axis represents integration. According to the standard argument, the EP and the Commission are more prointegration than the members of the Council (Garrett 1992).

If the EP is able to find out what the Council can do on its own (i.e., unanimously) and present a proposal that makes the Commission and a qualified majority of the Council better off than either the status quo or what the Council can do on its own, this proposal will be accepted by both the Commission and the Council and will be the outcome of the cooperation procedure. So let us follow the EP in its calculations.

The Council can unanimously adopt any proposal inside the area indicated by the unanimity set of the

status quo, or U(SQ). This area is generated by the intersection of all circles whose centers are the ideal points of the members of the Council and pass through the status quo. This area is included between the circles around the two states closest to the status quo (1 and 2 in the figure).

However, which point inside U(SQ) would be selected by the Council is not clear. It all depends on how convincing different governments are in proposing their alternatives for a vote. For example, the Danish parliament is known to have a permanent committee on European Community legislation, which extracts statements from the government prior to Council meetings, so that the Danish representative in the Council is particularly inflexible (Williams 1991, 159).6 Under such circumstances, would other members of the Community accept the Danish position as the alternative to the status quo, or select a different point? In Figure 2, no ideal point of the member countries is included inside U(SQ), so the different countries would have to come to a compromise.

Since the unanimity position is not unique, I shall impose on parliamentary proposals a severe restriction. I shall require that in order to be accepted, they must be preferred by the Commission and by a qualified majority of the Council to *any* proposal that can be voted unanimously by the Council. This way we will have a (very conservative) estimate of the conditional agenda-setting power of the EP.

In Figure 2, five out of the seven members of the Council can be made better off by proposals inside the qualified majority set of the unanimity set of the

status quo, or Q(U(SQ)). Indeed, members 3–7 prefer any point inside this area over any point inside U(SQ). The area Q(U(SQ)) is generated by the intersection of five circles with centers the points 3–7 going through the edge of U(SQ)). Therefore, the EP can select inside the area Q(U(SQ)) the point that it prefers most, that is, the point closest to its own ideal point (provided that the Commission prefers it over U(SQ), which is the case in the figure). This is point X in Figure 2.

However, such a winning proposal does not always exist, as the situation depicted in Figure 3 indicates. Here, the status quo is further away from the positions of the Council (the heptagon 1–7); consequently, the Council has a wide set of options on its own (by unanimity). There is no proposal that can defeat, by qualified majority, all points inside U(SQ). For example, a five-member majority consisting of members 3–7 is impossible, because the circles around 3 and 7 tangent to U(SQ) do not intersect. The same is true for all other possible majorities.

Another interesting case exists when the status quo is inside the heptagon 1-7. In this case, the Council cannot modify the status quo by unanimity, because at least one member will object to any particular move. Consequently, the status quo can be modified only through a parliamentary proposal. Figure 4 indicates that in this case, the EP can select one of several possible coalitions. The top lens of Q(U(SQ))is preferred to the status quo by a coalition of members 3–7. (It is generated by the intersection of circles around 3 and 7.) The bottom lens is preferred by a coalition of 1-5. The middle lens is preferred by the coalition 2–6 (intersection of circles around 2 and 6). In this case, the EP will propose X and will be supported by the Commission and the coalition of states 2-6.

In these examples, the EP was sometimes able to make a winning proposal (Figures 2 and 4), sometimes not (Figure 3). Sometimes it could select its allies (Figure 4), sometimes not (Figure 2), while on still other occasions, there were no allies available (Figure 3). Is there any *systematic* argument connecting these three figures?

First of all, if the status quo is located inside the heptagon 1–7 (the Pareto set), it cannot be changed by the unanimity of the Council: at least one member will object to any particular move. Second, in two dimensions, for the qualified majority specified by the cooperation procedure (54/76), there is a central area of the Council (technically the 54/76-core, hereafter the Q-core). If the status quo is inside the Q-core, it cannot be modified by any 54/76 majority coalition. So if the status quo is located in the Q-core, it cannot be modified by either the EP's proposal or by the Council on its own. The Q-core is represented in Figure 5 by the heptagon 1'2'3'4'5'6'7'.

Figure 5 uses calculations from the appendix and divides the plane into four different areas. If the status quo is inside the Q-core (area IV) it cannot be changed (either by unanimity or qualified majority). If the status quo is outside this central area but inside

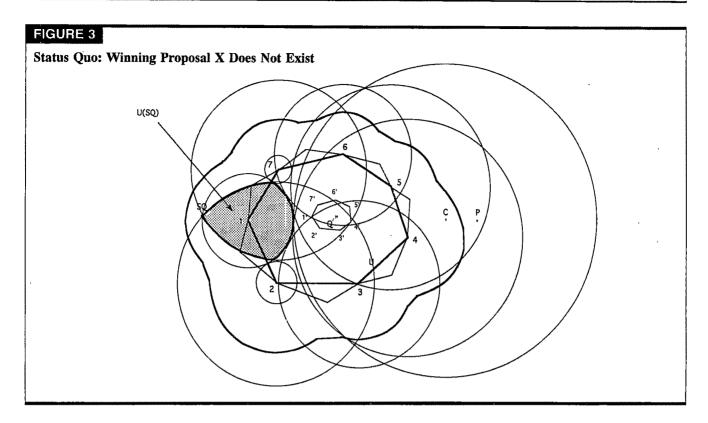
the heptagon 1234567 (the Pareto set), then the Council cannot modify the status quo by unanimity. In this case, the EP can select a majority in the Commission and a qualified majority (Q) in the Council as allies and make a proposal that is preferred by its allies to the status quo. If the status quo is outside the Pareto set but close to it, then again the EP can make a winning proposal X to the Council (provided the Commission adopts it in its report).

If the status quo is further away from the Pareto set of the Council, the set of alternatives unanimously preferred to the status quo may or may not intersect with the O-core. If there is an intersection, then there is no point that can command a Q-qualified majority of the Council, so the EP has no agenda-setting power. The set of status quo points for which the unanimity set intersects with the Q-core is defined by the curve I(Q-core). If the status quo is outside this line (area I), the EP has no agenda-setting power, and the Council will select its preferred solution by unanimity. If, however, the status quo is inside the area defined by the I(Q-core) curve (area II), a winning proposal by the EP may exist. Consequently, in two dimensions, the agenda-setting power of the EP increases as the status quo approaches the Pareto set of the Council and reaches its maximum when it is inside the Pareto set (no change is possible without the EP) but decreases again when the status quo moves inside the Q-core. (This is what I have called curvilinear property in the Appendix, Theorem 2.)

In all these figures, I have presented the EP's and the Commission's ideal points as close to each other. This is the most frequent case, because both are supranational actors and because the Commission is politically responsible to the EP. However, if the position of the Commission were far away from the position of the EP, it might be that no winning parliamentary proposal existed, because a parliamentary amendment would be rejected by the Commission *before* it reached the Q-majority in the Council.

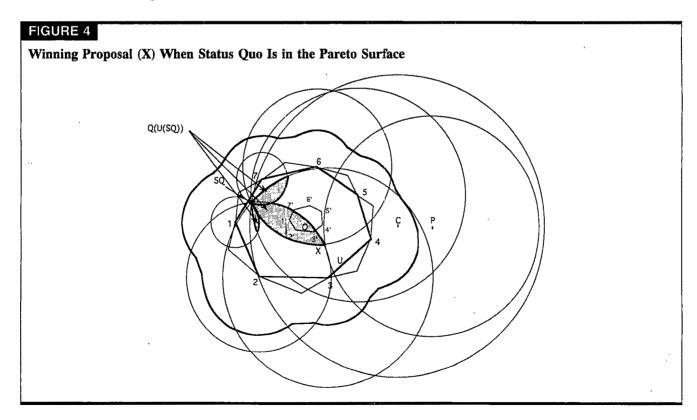
Paradoxes can arise in such an institutional setting. For example, the final outcome selected by the cooperation procedure might not be in the Pareto set of the Council. Consider a case in which the status quo is in the Pareto set, Q(SQ) has points outside the heptagon 1234567, and the EP's and the Commission's ideal points are located in such a way that they make a proposal outside the Pareto set. Figure 4 would present such a case if states 3-5 were moved to the left of X. Politically, such a situation could arise if there were a polarization between a relatively cohesive qualified majority in the Council and the minority. The EP could then step in, making a proposal between its own position and the qualified majority's but still better than any feasible solution by unanimity. This result would be opposite the conclusion of Weber and Wiesmeth who claim that the outcomes of the cooperation procedure are always efficient (1991,

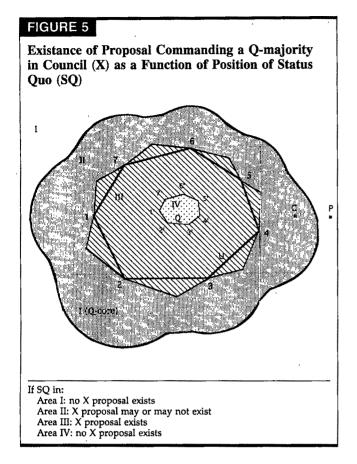
Another paradoxical property of the cooperation



procedure is nonmonotonicity. Consider that the status quo is at the position presented in Figure 3, where there is no winning proposal by the EP; if country 1 moves to the left of the status quo, the status quo will be included in the Pareto set. In this case, there is always a proposal that the EP can make to move the status quo further to the left. In this

hypothetical example, the only change is a movement of one government to a less integrationist position, and the resulting change in the outcome is more integration (nonmonotonicity). Why did this happen? Because before, there was a wide range of unanimity outcomes, and no qualified majority could win all of them, while afterward the movement of





one country persuaded the others that there was no unanimously preferred change of the status quo, so they had to accept the EP's proposal by qualified majority.

To summarize, a 54/76-core is guaranteed to exist in two dimensions. In this case, the curvilinear property described in Theorem 2 holds: when the status quo is inside the Q-core or far away from it, there is no winning parliamentary proposal, so that the EP's conditional agenda-setting power is null. But whenever the status quo occupies some intermediate position, a winning parliamentary proposal (assuming adoption by the Commission) is either guaranteed or may exist. This is the conditional agenda-setting power of the EP in two dimensions.

What happens in more than two relevant dimensions? In this case, a Q-core is not guaranteed to exist. The implication of the absence of a Q-core is that no point inside the Pareto set is invulnerable. There is always at least one coalition of 54/76 votes in the Council that will be made better off by a change of the status quo. If the EP can identify these coalitions and the changes they are willing to support, it can propose the one that it most prefers. In other words, the set of alternatives available to the EP increases when there is no Q-core. On the other hand, if the status quo is far away from the Pareto set, there is no winning proposal from the EP.

One additional interesting feature of conditional agenda setting is the importance of information. According to the informational property (Theorem 3

in the Appendix), when the EP's information about what the Council will unanimously do increases, its agenda-setting power increases. In the limiting case, when the EP knows exactly which point inside the Pareto set the Council will select, it will always have agenda-setting power (unless there is a Q-core and the Council decides to select a point inside it).

As we saw earlier, the EP and the Council fought bitterly about the informational content of the common position rendered by the Council. Some commentators regarded this struggle and the subsequent shift in the Council's position as "preconditions for rationalizing and coordinating the legislative procedure" (Bieber 1988, 720). The informational property helps us to view this struggle in a different light: it is a fight for control of the agenda. The conclusions of this analysis are fourfold.

The Position of the Final Outcome. The final outcome of the cooperation procedure will most likely be inside the heptagon defined by the states. However, it is possible that Q(U(SQ)) has points outside the Pareto set, and if one of these points is the closest to the EP (and accepted by the Commission), then the cooperation procedure leads to an inefficient outcome for the members of the Council.

Curvilinear Property. The EP's agenda-setting power is a function of the position of the status quo. If there is a Q-core, this power is a curvilinear function of the position of the status quo. It does not exist if the status quo is inside the Q-core or far away, and it does or may exist in intermediate positions.

Multiple Dimensions. In multiple dimensions, it is likely that the Q-core does not exist. In this case, the agenda-setting power of the EP increases when the status quo is inside the Pareto set or close to it. (The EP may be able to select among several possible coalitions.)

Informational Property. Accurate information in the EP about the positions that are likely to be adopted by unanimity in the Council increases the agenda-setting power of the EP.

EUROPEAN PARLIAMENT AND EUROPEAN INSTITUTIONS

I use these four conclusions to discuss empirical results of the cooperation procedure, as well as to compare my model with other analyses.

The Car Emission Story

On 15 February 1988, the Commission introduced legislation concerning small-car emission standards to the Council (composed of the 12 ministers of the environment). The proposal would reduce emissions from small cars by 58% by 1992–93.

The Council commenced discussion and found the positions of the 12 ministers to be divergent: Luxembourg, Ireland, and Belgium agreed with the proposal; France, Britain, Italy, Spain, and Portugal found the standards too strict; and Holland, Denmark, Greece, and Germany found the standards too lax. Before the EP's first reading, several countries modified their positions (notably France and Germany), allowing the Environmental Council to approve the Commission's proposal by qualified majority.

The EP, in its first reading (September 1988), voted for what are called U.S. 83 standards, which require catalytic converters. The Commission rejected the EP's amendments. The Council, after a series of compromises, adopted the Commission's proposal as a common position by qualified majority.

In April 1989, the common position came before the EP for a second reading. The EP insisted on its previous set of U.S. 83 standards. This renewed (but not new) proposal met the agreement of the Commission this time around. Jacobs and Corbett claim that the EP threatened the Commission by stating that unless the Commission accepted its version, it would reject the Council's common position (1990, 170). Given the absence of unanimity in the Council, this would be equivalent to aborting the legislation. This threat was credible because European public opinion is very sensitive to environmental issues, and the election of the European EP was approaching. The Council, faced with a proposal that incorporated higher standards and knowing that it could not come to a unanimous decision, adopted the EP's position by a qualified majority.

The car emission story is the EP's most spectacular success, although there are others similar to it.9 In general, only one out of two amendments of the EP gets incorporated in legislation, as statistics will indicate.

Cooperation Procedure Statistics

During the period from July 1987 to November 1991, the EP dealt with 208 Commission proposals under the cooperation procedure.10 The Commission accepted 1,626 out of the 2,734 parliamentary amendments, and the Council accepted 45 percent of the EP's first-reading amendments. In the second reading, the EP adopted 716 amendments, 366 of which were adopted by the Commission and 194 by the Council (27%). In three cases, the EP rejected the common position of the Council. If the amendments that are proposed by the EP in the second reading are repetitions (or possibly watered-down versions) of the initial amendments, then out of the 2,734 parliamentary amendments the Commission accepts 1,992 (73%), and the Council, 1,410 (52%).

Using similar figures, Lodge notes out that "the EP's second-reading amendments stand a far slimmer chance of survival" (1989, 75), while Bogdanor comes to the conclusion that "the main effect of the second reading of Community legislation . . . is to increase the importance of the first reading as a point of

leverage for Parliament" (1989, 208). Similarly, Fitzmaurice argues that "the second reading 'navette' will have little real purpose" (1988, 390).

Little can be made of these first-versus-secondreading statistics, because without further investigation, we cannot assess the importance of the EP's amendments, nor consequently, can we tell whether amendments were accepted in the first reading because they would have been accepted in the second, whether the Council accepted some amendments in order to weaken the Parliamentary majority and make the EP unable to offer amendments in the second reading, or whether the Council accepted the less-important amendments in the first round. However, what clearly emerges is that of every four EP amendments, two are accepted, one is rejected by the Commission, and one is rejected by the Council. This mixed result can be well understood within the conditional agenda-setting framework: the EP has conditional agenda-setting power; consequently, it may find proposals with the property of making both the Commission and a qualified majority in the Council better off than the status quo. The reason successful amendments are not the rule is that such proposals do not always exist.

Comparisons with Other Analyses

The model I present combines the existing evidence to make several points, including the importance of information and the curvilinear property and the fact that the cooperation procedure can generate paradoxes. However, two findings are especially important: (1) the conditional agenda setter, under the conditions specified by the model, has an important input in decision making; and (2) the EP is the agenda setter in the cooperation procedure. Both points have not always been understood or analyzed correctly. Most analyses base their conclusions overwhelmingly on the observation that the final decision in the cooperation procedure is made by the Council; others are incorrect in terms of the strategic calculations of the actors; and in still others, the EP's role has been underestimated because the agenda-setting power is attributed to the Commission.

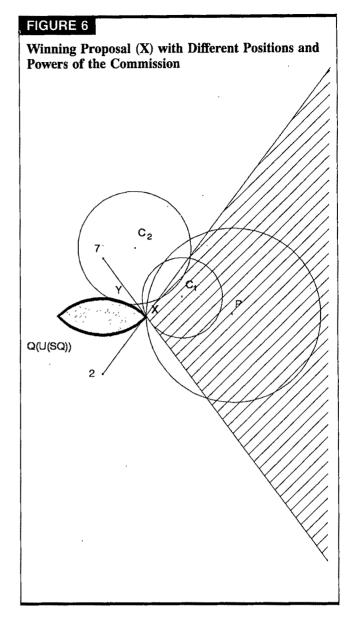
Bieber, Pantalis, and Schoo argue that "with regard to the European EP, the Single Act is an inconsistent document: Where it increases the EP's powers of participation in decision-making the practical effect is either very limited or diminished because the exercise of the powers is conditional on the attitude of the Council and the Commission" (1986, 791; emphasis mine). Similarly, Fitzmaurice argues that "despite the appearances of a co-decision model, the Council virtually retains the last word" (1988, 391). Both accounts underestimate the EP's role. My analysis demonstrates that the Council has the last word only if the EP fails to make a winning proposal. Moreover, if the "attitudes" of the Council and the Commission are not whimsical and if each actor, when confronted with a choice, selects the best alternative for itself (thereby adopting maximizing behavior), the EP,

through astute selection of its proposals, sometimes has the power to impose its will upon the other actors.

Lodge notes the power of the EP "in an alliance with one or more member states prepared to thwart the attainment of the necessary majorities (qualified or unanimous) unless EP's views and amendments were accommodated" (1987, 23). If Lodge's analysis concerns the power of the EP to block the decision in the Council, one or two allies in the Council are not necessary. Both the EP and the Council can block legislation by simply sitting indefinitely on a proposal during the first reading of the cooperation procedure. However, in the second reading, the EP has more than blocking power. If the analysis concerns the power of the EP to impose its will on the Council, one or two members in the Council are not enough: a qualified majority is required. To make this point clear, consider the configuration in Figure 3. Move the EP's ideal point to coincide with state 4. There is now an objective alliance between the EP and state 4. However, there is no winning proposal that the EP can make, and if the EP makes a proposal, even if state 4 argues that it supports the EP and will vote down an otherwise unanimous proposal causing the status quo to prevail, it will not be believed by the other members of the Council.

The argument that the EP has agenda-setting powers is perhaps the most controversial position in this article. Most analyses of the cooperation procedure attribute agenda-setting power to the Commission, not the EP. For example, Garrett argues that "parliament amendments merely allow the commission again to make its own proposals" (1992, 551); and Lenaerts argues that the Commission has a "monopoly of legislative initiative" (1991, 22). I have already argued that in practice, the initiative may come from any one of the three actors. The real question is whether the Commission is constrained in its proposal by the amendments of the EP in the second reading. The evidence indicates that it is.

In the EEC Treaty there are two rules specifying the role of the Commission. Article 149.3 gives the Commission wide powers of revision and specifies, "As long as the Council has not acted, the Commission may alter its proposal at any time during the procedures mentioned in paragraphs 1 and 2." According to this article, there are no restrictions on time (as long as the Council has not acted) or on the content of the revisions. In contrast, Article 149.2d, which refers specifically to the role of the Commission after the second reading by the EP, adopts a more restrictive procedure: "The Commission shall, within a period of one month, re-examine the proposal on the basis of which the Council adopted its common position, by taking into account the amendments proposed by the European Parliament. The Commission shall forward to the Council at the same time as its re-examined proposal, the amendments of EP which it has not accepted, and shall express its opinion on them. The Council may adopt these amendments unanimously." According to this second procedure, the Com-



mission will simply incorporate or reject the Parliamentary amendments to its proposal.

Empirical reports on the cooperation procedure do not mention any Commission amendments adopted or rejected by the Council, which indicates that the Commission does not make amendments of its own at this stage. Finally, while there are complaints from the EP that the Commission rejects parliamentary amendments, there have been no complaints that the Commission has altered the amendments that it accepted.¹¹

As long as the Commission accepts or rejects parliamentary amendments and does not significantly modify them or add its own amendments, agenda-setting power lies with the EP. Politically, it is not very important whether the EP or the Commission is the agenda setter, because their positions are usually close to each other. (The Commission accepts three-fourths of parliamentary amendments).

However, for reasons of completeness, in Figure 6,

I present a counterfactual situation under two assumptions: (1) the Commission decides to apply article 149.3 instead of 149.2d in the second reading of the cooperation procedure; and (2) the positions of the Commission and the EP are substantially different from each other. In this figure, I present the set of points that command a qualified majority over anything that can be voted unanimously by the Council, Q(U(SQ)), and the ideal points of the EP and the Commission. Under the two assumptions, the outcome of the procedure will be Y, instead of X. However, if one of the two assumptions is relaxed, the outcome reverts to X, the EP's proposal. For example, if the Commission's ideal point in Figure 6 is inside the shaded triangular area, say, C_1 , then, even if the Commission could completely ignore the EP's proposal, it would still propose X. Similarly, if the Commission's ideal point is C2 and the Commission can only accept or reject the EP's proposal, it will accept X.

Figure 6 can also help us relax the unitary-actor assumption regarding the EP in a particular case. Consider that a majority of EP members are located inside the shaded triangular area that include P in the figure; in this case, all these members would prefer X from the set of feasible point Q(U(SQ)). Further research is required to relax the EP as a unitary-actor assumption in other cases.

According to my account, the EP has agendasetting power as long as it can make a winning proposal in the second stage of the cooperation procedure. There are essentially four relevant points, which I shall discuss in turn.

Existence of an Absolute Majority in the European Parliament. This is the requirement for successful parliamentary proposals that I have least discussed. I avoided it by assuming that the EP was a unified actor and by studying the internal divisions of the Council. However, as I said earlier, the 260-vote requirement for a second-round proposal is not a trivial matter. It essentially requires congruence on the part of socialists and Christian democrats from different countries. This is not a frequently observed alliance at the national level. 12 I think that such a coalition can be formed more frequently on social or quality-of-life issues (environment, health, education, and research) than on economic issues. To the extent that the former prevail on the agenda, the EP will see its influence increased.

Acceptance by Commission. The EP and the Commission have had positions close to each other in the past. The statistics already reported indicate that the Commission accepted three-quarters of parliamentary amendments in the cooperation procedure. There are two ways in which the EP can keep this relationship close in the future. The first is through the political responsibility of the commission in front of the EP. The second is illustrated by the car emissions case: it can threaten to reject a proposal in its second reading. Such a measure requires unanimity

in the Council and consequently would probably kill the Commission proposal, damaging the Commission's reputation. However, if there is sufficient divergence between the EP's and Commission's positions, a winning EP proposal may not exist, since it will not be adopted by the Commission.

Position of the Status Quo. An unconditional agenda setter has more power when the status quo is far away, because then the former has more leeway to make a "take it or leave it" offer. In contrast, the European EP (a conditional agenda setter) has less power the further away the status quo, because there are many positions that the Council can adopt on its own by unanimity to avoid both the status quo and the EP's position. It is reasonable to assume that throughout the history of the European Community the status quo has continued to move toward more integration. If this assumption is accepted and if integration continues, then as the status quo approaches or gets inside the Pareto set of the Council, the EP's role is likely to increase. The simple displacement of the status quo toward more integration will transform winning parliamentary proposals into the rule. Obviously, this is a ceteris paribus prediction, and it assumes the same institutional structure (the current cooperation procedure) and the same distribution of tastes among the different actors.

Dimensionality. A 54/76-core is guaranteed to exist in two dimensions but not in three or more dimensions. Lack of a core makes every point inside the Pareto set vulnerable and consequently increases the likelihood that a parliamentary winning proposal will exist. So if issues become more complicated, the EP's role is likely to increase. This conclusion is congruent with the argument in Weber and Wiesmeth (1991) that the likelihood of cooperation increases through issue linkage. The only difference is that issue linkage is a conscious effort (i.e., a strategy) to connect different issues, while my argument is that regardless of the reason for the connection (conscious effort or objective complication), the outcome is not only more cooperation but a shift of power to the EP.

CONDITIONAL AGENDA SETTING AND EUROPEAN INTEGRATION

There are two competing theories of European integration, neofunctionalism (Haas 1958, 1971; Lindberg 1963; Scheingold 1970) and intergovernmentalism (Moravcsik 1991). Their main point of disagreement is whether governments play the principal role in the process of integration. A series of empirical studies focuses on particular areas of European integration and finds evidence to support and to contradict each theory (Cameron 1992; Lange 1992; Peters 1992; Shapiro 1992).

I shall conclude by arguing that the concept of conditional agenda setting can help us understand

the process of integration differently from the major competing theories. The cooperation procedure provides a formal expression of the conditional agenda-setting concept. The idea is simple—delegation of powers as long as certain limits are not crossed and loss of these powers otherwise. One can hardly think of a principal–agent relation where the delegation of powers is unconditional. Simple and universal though the concept of conditional delegation may be, the cooperation procedure presents the rare feature of specifying the conditions, instead of leaving them to an implicit understanding.¹⁴

One important feature of the cooperation procedure is that the outcome is not necessarily Pareto optimal for the states. In this sense, it is not necessarily an efficient institution (Tsebelis 1990, chap. 4). However, most of the time the outcome will be inside the heptagon defined by the ideal positions of the states. But which Pareto optimal outcome will be selected? Krasner (1991) raises this point with respect to the international regime of communications. Garret and Weingast (1993) forcefully present the problem of equilibrium selection and argue that ideas, as well as the interests of the most powerful states are important in equilibrium selection. The cooperation procedure presents an important mechanism for equilibrium selection combined with other desirable features.

European integration proceeds through a series of measures that may have redistributive or investment aspects. In both cases, sacrifices are required by some social or national actors. It is obvious that such actors would prefer alternative policies with lower or no cost for them. If such actors represent brakes on European integration, what is the accelerator? Figures 2-4 provide a visual representation of the answer. In all these cases, a supranational actor (the EP) is provided with conditional control of the agenda. Whenever the EP exercises this power the outcome is more integrationist than that which the members of the Council would have selected on their own (by unanimity). It is even more integrationist than the positions of the Council's pivotal members (country 7 in Figures 2 and 4) and can be even more integrationist than any member of the Council (outside the Pareto set). Moreover, if efficiency gains from the common policy are high (if the status quo is far away), the Council can resolve redistributive issues on its own. (Remember that in this case, the EP has no agenda-setting power.) If efficiency gains are low (i.e., the status quo is close to or in the Pareto set), the EP is empowered to solve the problem of equilibrium selection. So equilibrium selection is one feature of the conditional agenda-setting mechanism.

But conditional agenda setting by the EP presents some more desirable features. The speed of integration is under the final control of governments. Most of the time the outcome will be inside the Pareto set of the Council, but in any case the Council is able to overrule the EP if it pushes integration too fast. Another important advantage of European institutions is that they diffuse responsibility for unpopular

measure from national governments to some combination of supranational institutions who were able to impose their will despite existing objections.

Other European institutions offer the same advantages. In the consultation procedure, the Commission has the role of the conditional agenda setter. In the cases that the Council can decide by qualified majority, the mechanism of delegation of agenda-setting powers is exactly the same: the Council needs qualified majority to agree and unanimity to modify.

When the European Court of Justice made the decision concerning cassis de dijon, thereby creating the doctrine of mutual recognition, it was making a decision that was bound to be in serious disagreement with important social interests in all member countries at some time or another. Similarly, the Court practically rewrote the interpretation of the Community value-added-tax directive. Such measures were subsequently adopted by the Single European Act (i.e., by the unanimity of governments). In all these case, governments in disagreement can opt out of the application of particular legislation or even of the system altogether. It is up to the supranational actor to make decisions that will carry the Community further along, rather than lead to disagreement, dissent, and ultimately to disintegration.

Conditional agenda-setting powers are likely to increase in the future for two reasons: (1) the status quo approaches the positions of the members of the Council; and (2) issues become more complicated, so that a 54/76 qualified majority core is not likely to exist. As I demonstrated earlier, both features lead to increase in powers of the conditional agenda setter.

In conclusion, European integration happens, among other reasons, because national governments have built institutions attributing conditional agendasetting power to supranational actors. If in the future popular sentiment wants to reduce the speed of integration, the citizens of Europe may use the electoral process to make sure that some particular points of view are present in or absent from the EP; that is, they may change the EP's policy position. Altering the position of the EP in Figures 2–6 would not affect the agenda-setting powers of the EP (it will still be able to make winning proposals under the conditions I have specified), but it does affect the content of the winning proposals, that is, the outcomes of the cooperation procedure.

Appendix

DEFINITION 1. Pareto Set. The set of points where the welfare of all members cannot be improved simultaneously.

LEMMA 1. The Pareto set of the states in Figure 5 is defined by the contour of the heptagon 1234567.

DEFINITION 2. Q-Core. The set of points where the welfare of Q out of N states cannot be improved simultaneously.

LEMMA 2. The Q-Core (for Q = 5/7) of the states in Figure 5 is defined by the contour of the heptagon 1'2'3'4'5'6'7'.

LEMMA 3. The Q-core exists if Q > n/(n + 1), where n is the dimension of the space (Greenberg 1979 theorem 2).

COROLLARY 1. In the cooperation procedure, a 54/76-core always

exists in two dimensions. A 54/76-core is not guaranteed to exist in more than two dimensions.

DEFINITION 3. Unanimity Win Set of a Point. For any point SQ, define U(SQ) as the set of points that are unanimity preferred to it. COROLLARY 2. U(SQ) is empty if SQ belongs to the Pareto set.

Definition 4. Qualified majority Win Set of a Point. For any point SQ, define Q(SQ) as the set of points that are qualified-majority-Q-preferred to it.

COROLLARY 3. Q(SQ) is empty if SQ belongs to the Q-core.

DEFINITION 5. Qualified Majority Win Set of a Set. For any set of points X, define Q(X) as the set of points that are qualified-majority-Q-preferred to any point in X.

COROLLARY 4. If X intersects with the Q-core, $Q(X) = \emptyset$.

Proof. The proof follows from definitions 2 and 5. Call x one of the points of intersection. According to definition 2, there is no point preferred to x by a Q-majority, consequently Q(X) is empty.

DEFINITION 6. Conditional (Q, U) Agenda-setting Power. For a status quo point SQ, an actor has conditional (Q, U) agenda-setting power if when U(SQ) is not empty, Q(U(SQ)) is not empty, and when U(SQ) is empty, Q(SQ) is not empty.

COROLLARY 5. If SQ belongs to the Q-core, there is no conditional (Q, U) agenda-setting power.

Proof. Both U(SQ) and Q(SQ) are empty.

COROLLARY 7. If SQ belongs to U\(Q-core), there is conditional (Q, U) agenda-setting power.

Proof. U(SQ) is empty; therefore, we have to examine Q(SQ). But Q(SQ) is not empty, since SQ does not belong to the O-core.

THEOREM 1. If SQ is in the "neighborhood" of U, there is conditional (Q, U) agenda-setting power.

Proof. Select a point SQ close to but outside the Pareto set (the sides of the heptagon 1234567), say along the segment 17. In this case, U(SQ) will be a small set defined by the indifference curves of 1 and 7. Call SQ' the symmetric point of SQ with respect to the segment 17. If SQ is close enough to 17, the other 5 members will prefer SQ' to any point inside U(SQ), and SQ' will not be included in the Q-core. So Q(U(SQ)) = Q(SQ'), and Q(SQ') exists (Corollary 7). Construction of "Neighborhood" of U. In the proof, I used the

Construction of "Neighborhood" of U. In the proof, I used the point SQ', which is preferred to any point inside U(SQ) by all other five members. If we draw the lines 16 and 27 and call 1" their intersection, all the points SQ' inside the triangle 171" have this property. The circles from 2 and 7 through SQ' intersect once to the right and once to the left of segment 27. But since SQ' is to the left of 27, the other point is to the right, which means that there is no point of U(SQ) preferred to SQ' by 2. Similar argument can be made with respect to the other four points of the heptagon (in particular, for point 6). For SQ'

between these points and SQ) will be on the heptagon 1'2'3'4'5'6'7'. Indeed, if SQ lies on a segment of a straight line (e.g., the one symmetric to 1'7' in Figure 5), the U(SQ) will include the indifference curves of 1 and 7, which intersect in symmetric points along the axis 17; if SQ lies on a segment of a circle (e.g., center 1 and radius 11'), the U(SQ) will include the indifference curve of 1, which by definition goes through 1'. Since points SQ along the boundary of I(Q-core) produce U(SQ), who are on the boundary of the Q-core, points further away (area I in Figure 5) will produce U(SQ), which intersect with the Q-core. Therefore,

THEOREM 2. Curvilinear Property. For any two-dimensional configuration of the Council, the plane is divided into four subsets according to the position of SQ, as in Figure 5: AREA I: the inverse Q-core of the Council; no conditional (Q, U) agendasetting power. AREA II: agenda-setting powers may or may not exist. AREA III: the Pareto set (excluding the Q-core), along with "neighboring" areas; always conditional (Q, U) agenda-setting power. AREA IV: the Q-core; no conditional (Q, U) agenda setting power.

THEOREM 3. Informational Property. For any two points x and y, if U(x) is a subset of U(y), then Q(U(y)) is a subset of Q(U(x)).

Proof. From the definition of Q(A) it follows that Q(AUB) is a subset of Q(A). The proof follows if one calls U(x) = A and U(x)/U(y) = B.

Notes

This is a revised and abbreviated version of a paper that received the Franklin L. Burdette Pi Sigma Alpha Award for the best paper presented at the 1992 meetings of the American Political Science Association. An earlier version was released as a Working Paper No. 1.7 by the Center for German and European Studies of the University of California. I would like to acknowledge financial support by the Hoover Institution and the Center of German and European Studies of the University of California. For useful comments, I thank A. Comfort, J. Fitzmaurice, J. Frieden, M. Golden, P. Lange, G. Schwartz, K. Shepsle, and the participants in seminars at the University of California at Los Angeles and San Diego and McGill, Harvard, and Washington Universities. I thank N. Jesse and B. Kilroy for research assistance. Finally, I thank M. Olmsted for his comments, as well as for providing me the story on exhaust emission standards.

1. Scharpf (1988) studies the decision-making process of the Community, and argues that it resembles German, much more than American, federalism and that the fact that decisions are taken by the Governments and by the unanimity rule leads to "a joint-decision trap" with suboptimal outcomes. Weber and Wiesmeth (1991) analyze the Single Euroqualified majority is needed to approve and unanimity, to modify. Bieber comes to the conclusion that the translation of the Single European Act in different languages is ambiguous and, therefore, that unanimity is required (1988, 719). However, on several occasions, the Council has decided by qualified majority, so in the remainder of my account, I will assume that only qualified majority is required for the adoption of a common position. The four largest countries, (France, Germany, Italy and the UK) have ten votes each; Spain has eight votes; Belgium, Greece, the Netherlands and Portugal have five each; Denmark and Ireland have three votes each and Luxembourg has two votes, making a total of 76 votes on the Council.

4. The reader is reminded that 260 votes are required for a

proposal.

- 5. I select a two-dimensional representation instead of the (marginally simpler) one-dimensional one, for four reasonsfirst and foremost, because one-dimensional models typically produce equilibrium results (Shepsle 1979), while two-dimensional ones not only generically lack such equilibria but produce chaotic behavior, that is, cycles all over the space (McKelvey 1976). The model here includes a mechanism for equilibrium selection that the reader will not be able to identify unless the generic model has the possibility of producing chaotic results (i.e., is at least two-dimensional). Second, the results from two dimensions are easily generalizable to more than two dimensions, which is the most realistic assumption in the politics of the European Community. Third, two dimensions is the minimum required to give the EP the possibility of selecting a supporting coalition inside the Council. Finally, as it will become clear later, the one example of the cooperation procedure that I shall present cannot be represented in a less than two-dimensional space (in fact, at that point, it will become clear that the representation of the status quo requires more than one dimension).
- 6. What Denmark does in this case is nest the international game of European policymaking inside its domestic politics game in order to achieve a credible threat (Putnam 1988; Tsebelis 1990).
- 7. It is easy to verify that any point outside the heptagon 1'-7' can be defeated by its projection on the closest side of this heptagon by a 54/76 qualified majority.

8. The following account is based mainly on Kim 1992; see also Jacobs and Corbett 1990, 170, and Stephen 1992.

- 9. Guy Peters argues that the case of occupational health provides another example of the power of the EP (1992, 102); and Juliet Lodge adds the case of a medical research program to the list (1989, 75).
- 10. The data come from European Parliament 1992 and cover the period from July 1987 to November 1991.
 - 11. John Fitzmaurice, personal communication, March 1993.
- 12. Exceptions include tripartism in post-World War II France, the Grand Coalition in Germany (1965–69), post-1960s coalitions in Italy, and coalitions in Belgium and the Netherlands.
- 13. See Keohane and Hoffman 1991; Scharpf 1989; Webb 1983.
- 14. Other cases of such conditional delegation that I can think of are the veto power of the American president (conditional upon not violating the will of 2/3 of either House of Congress), the power of the German president to nominate the chancellor (conditional upon selecting a candidate acceptable to the Bundestag in the first round), and legislative veto in the United States.

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ARISTOTLE'S SCIENCE OF THE BEST REGIME

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ristotle's science of the best regime brings to light an almost forgotten but by no means settled quarrel between reason and faith concerning the best way of life and its political embodiments. Aristotle denies the claimed superiority of divine legislation, in favor of the guidance supplied by unaided reason. Aristotle knows, however, as contemporary political science may not, that only by confronting the divine law as such can science avoid collapsing into dogmatism. The present study attempts to sketch that confrontation by considering Aristotle's analysis of justice—the concern fundamental to both Aristotle and divine law—in order simply to encourage the kind of reflection necessary to reinvigorate rationalism, or that which, according to Aristotle, is closest to the divine in man.

his essay contends that Aristotle's scientific account of the best form of government or "best regime" in the *Politics* deserves serious reconsideration today because, quite apart from its specific conclusions or prescriptions, it brings to light a nearly forgotten but hardly settled dispute between reason and faith as to what the best way of life and its political embodiments are. In brief, can human beings by their own lights discover the good life, or are they necessarily dependent on the divine to reveal this to them?

The urgency of the need for political science to recover this question as a question and to think it through once again is perhaps not obvious at present, but this is likely to change. For modern liberalism's practical answer to it-in brief, the separation of church and state-now rests on theoretical foundations that have all but given way under the sustained critical onslaught of Nietzsche and his successors, and it is only reasonable to expect that this practical provision will one day collapse as well. Indeed, there are already indications that the subordination of religious beliefs to the private or subpolitical sphere is being severely tested, as in the emergence, for example, of Christian "fundamentalism" as a legitimate political force on the Right (Corbett 1990, 209-30; Jorstad 1987; Wald 1992, 222-78). Moreover, despite the apparently global rise of liberalism and its impressive victory over communism, there remain regimes virulently opposed to liberal democracy that, far from being soon-to-vanish relics of a benighted age, are at least holding their own and are arguably on the rise (see Fukuyama 1992, esp. 39-51; Hiro 1989; Naipaul 1981). In the most impressive cases, these antiliberal regimes ground their opposition to liberalism in an appeal to religious truth, especially Islamic law; the ordering of political life there owes, or is said to owe, its origin to the one true God whose law is as thoroughly political as any merely human law but is necessarily free of imperfection. Accordingly, these regimes deny not only that freedom or equality is the correct organizing principle of political life but also

that reason is the means appropriate to the discovery of that principle.² The difficulty with which Aristotle's science of the best regime wrestles proves to be with us still, mutatis mutandis. And in order to live up to the rationality and openness that characterize liberalism at its best, we must try genuinely to think through this political quarrel between faith and rationalism and never permit ourselves recourse to indignation or derision to settle it. Both internal doubts and external challenges thus bid us to try to recapture the contest between reason and revelation as it was once played out on the level of politics and political philosophy.

To this end, I suggest a return to the exposition of this question in the classic of premodern political science, Aristotle's Politics. For the political science of Aristotle, especially in its guiding inquiry into the "best regime," is very much aware of and concerned to meet the challenges that any rational account of politics encounters in the face of suprarational claims to know what the best way of life is.3 It is therefore particularly helpful in recovering that theologicopolitical understanding of the world against which the early philosophers of liberalism fought and according to which religion and politics rightly belong together-a view that not only remains the usual understanding in much of the non-Western world today but that gives every indication of reappearing here in one form or another. Of course, there are considerable differences between classical and modern political science on the one hand and between ancient piety and the living religions of today on the other. Only subsequent reflection can decide the extent to which Aristotle's specific answers can be adapted to guide us in our own circumstances. It is hoped, however, that this return to an older political science may be helpful to the liberal political scientist today in preparing the way for a truly fundamental debate between us as liberals and our contemporary antagonists both within and without, insofar as that debate ultimately concerns the competing claims of religion and reason to guide political life.

THE RATIONAL SCIENCE OF POLITICS AND THE DIVINE CITY

Aristotle's scientific attempt to determine the "best regime" in book 2 of the *Politics* includes both the denial of the claimed superiority of divine legislation and the contention that human beings are capable of discovering for themselves the best way of life and the political means needed to bring it about. Aristotle's science of politics may earn the right to criticize the divine city above all because it possesses a thoroughgoing analysis of the fundamental concern common to both Aristotle the scientist and the inhabitants of the divine city, the concern, namely, for justice.

The Quarrel Concerning the Best Regime

Aristotle first turns to the question of the best regime in book 2 of the Politics,4 and it is there that he lays out, in a number of different ways, the fundamental issue of the origin and character of the account of the best regime. For example, Aristotle relates, in passing, how a certain Onomacritus is said to have been the first to become skilled at legislating (1274a22–31). A Locrian living on Crete, he practiced the art of prophecy (see also Herodotus, History 7.6). He was also said to be a companion of the philosopher Thales, future teacher of the lawgiver Lycurgus and Zaleucus. The story of Onomacritus is a digression from its immediate context and, moreover, is regarded by Aristotle as false. Why, then, does he tell it? This story or myth is significant because it raises the question of the source of legislative skill: Did Onomacritus become skilled on the basis of his prophetic art (practiced on the island of Crete, the home of the divine laws of Minos) or did his expertise result from his association with Thales, the tutor of Lycurgus and Zaleucus and perhaps the most famous of philosophers?⁵ In other words, does the expertise in political affairs and, above all, the science of the founding of the best city, rely on knowledge accessible to the unassisted human mind or on superhuman inspiration? Does it owe its origin to "Thales" or "Onomacritus" (philosophy or prophecy, human reason or the gods)?6

Lest we miss the wood for the trees, the outline of book 2 as a whole points to much the same difficulty; for Aristotle there criticizes not only other rational or scientific attempts to delineate the best regimethose of Plato, Phaleus, and Hippodamus—but also three existing regimes, contenders to the title of best regime on account of their superlative laws: Sparta, Crete, and Carthage. In order properly to appreciate Aristotle's boldness, it is necessary to understand the divine character of every classical city: "We must not lose sight of the fact that, among the ancients, what formed the bond of every society was a worship. Just as a domestic altar held the members of a family grouped around it, so the city was the collective group of those who had the same protecting deities, and who performed the religious ceremony at the

same altar" (Fustel de Coulanges 1956, 146). Again, "There was not a single act of public life in which the gods were not seen to take a part" (ibid., 163). Accordingly, "The founder was the man who accomplished the religious act without which a city could not exist. He established the hearth where the sacred fire was eternally to burn. He it was who, by his prayers and his rites, called the gods, and fixed them forever in the new city" (ibid., 142; see also, e.g., Rousseau, Social Contract 4.8). To criticize a city of emphatically divine origin and to replace it with a city of one's own making is therefore daring (see Politics 1260b33-36); Aristotle implies that he can do by his own powers what no human being or god before him had done adequately, namely found, if only in speech, the truly best regime or the city "in accordance with what one would pray for." The gods and the divine form the background against which Aristotle sketches his rational science of politics in general and his best regime in particular, a background that occasionally but tellingly protrudes into the foreground.8 Let me turn now to the most important elements of that science as they bear on my themeto Aristotle's critique of law as such and then to the difficulties he discerns in the divine law of Crete.

The Rule of Law

Aristotle initially broaches the problem of law⁹ in the third and last of his criticisms of a certain Hippodamus, the first to have attempted to give a theoretical account of the best regime without being engaged in politics himself (*Politics* 1268a6–8, 1268b22–1269a28). Hippodamus calls for the honoring of those who propose some innovation beneficial to the city, even if this involves changing the traditional laws. According to him, innovation or the departure from traditional practices has benefited the other arts, sciences, and capacities. The political art or science rightly belongs among these, and the departure from traditional ways will therefore benefit political science as well. Two pieces of evidence may be adduced in favor of this view. First, the old laws are overly simplistic and barbaric because those who drafted them were so. It is known, for example, that the Greeks used to carry arms and purchase their wives from one another. Why, then, should we retain their beliefs? We must be open to changes in the law. Second, there is a difficulty with written laws as such, for they are necessarily general while actions, as such, are particular. Laws should try to overcome this inherent shortcoming by being as flexible as possible: we must be open to changes in the law (1268b31-69a13).

Aristotle nowhere disagrees with the evidence adduced by Hippodamus or the "innovationists" that there has been an advance over the earliest times (e.g., Politics 1269a3-4; 1271b23-24) or more importantly that the generality of the law as such prevents it from being simply adequate in every particular circumstance or crisis: "Every law is universal and it is impossible to speak correctly and in a universal way about some things. Now where it is necessary to

speak universally but impossible to do so correctly, the law grasps what is for the most part the case" (Nicomachean Ethics 1137b12–19; Politics 1282b4–6). Nonetheless, Aristotle rejects Hippodamus' proposal as "unsafe" (Politics 1268b23–24). He does so in part because "the parallel of the arts is false": the law is indispensable to the political art and "holds no strength in regard to obedience apart from habit; this arises only over a period of time" (1269a19–22). Aristotle maintains, then, that the law is not simply rational and that the human beings who create the law as well as those who obey it do so on the basis of a reason alloyed with irrational opinions or passions: "Thus the exchanging of the existing laws for other, new laws weakens the power of the law" (ibid. 22–24).

Aristotle's view of law and the danger of legal innovation might well seem to be conservative in the extreme. It ought to be recognized, however, that Aristotle does admit the desirability of changes in particularly foolish or barbaric laws (see Politics 1269a15-17). But more important, the basis of his conservatism is radically different from that of the conservatives who seek to maintain the traditional laws on the grounds that they are good because traditional (see 1287b5-8 and context). Aristotle's conservatism, far from being a kind of celebration of the traditional, receives its impetus from the insight into the flawed character of all law. While agreeing more or less with the practice of the conservatives, then, Aristotle is more "radical" in spirit than even the innovationists insofar as he denies to the law the very possibility of full rationality. From Aristotle's point of view, the conservatives and the innovationists are united in their unwarranted optimism regarding the possible goodness of the law (see 1281a34-39, 1282b1-10, 1283b35-42). When thought through, the conservatism informing Aristotle's Politics is a point of entry into philosophy, not an exhortation to devotion to the classical polis.

Against all of the foregoing, one might adduce Aristotle's elevated and even beautiful remarks regarding the law in book 3. The rule of law is there said to be akin to the rule of "the god and nous." Devoid as it is of desire and spiritedness, the law is held to be reason itself, to be "intelligence without longing" (1287a10–32). A consideration of the context of these remarks, however, makes it clear that this is the view advanced by a "republican" partisan (ibid. 10–12; cf. Leyden 1967, 9). And in this section of book 3, Aristotle repeats almost verbatim a remark from the Hippodamus passage, namely that "the example of the arts seems to be false" (cf. 1269a19 and 1287a32-33). In the present context this means that while we wish doctors to exercise their art on a case-by-case basis without being strictly bound by a set of formal rules, it is better for the practitioners of the political art to follow rules (laws) because the politicians are much more inclined than are doctors to do harm to those they are supposed to help. The obedience to set laws, then, will at least curb this tendency. Doctor and patient have an obvious common good in the health of the patient; much less obvious is the good common to both ruler and ruled. To overstate the case, Aristotle too comes to recommend the rule of law, not because the law is so good but because politics is so defective. The law is indeed a mean between the rule without laws characteristic of the doctor, which seems to have no counterpart in politics, and the rule without laws characteristic of tyranny or anarchy (see 1287b4-5). The rule of law is then a sensible compromise but it is not the rule of god and nous (ibid. a32-b4). It would seem, in addition, that the rule of a god cannot be exercised by means of law alone. Regardless of its origin, any law that wishes to rule human beings here and now, without benefit of a miraculous change in human nature and hence in the nature of politics, must partake of the limitations inherent in law as such. The law must thus be supplemented by wise judges and interpreters—by human beings who, possessing practical wisdom (phronêsis), are capable of discerning and adapting the spirit of the law to a given circumstance. Even if the law is as perfect at the outset as one would pray for, it must nonetheless be administered by subsequent generations of human beings who constitute a regime that is, as such, superior to (because able to interpret and change) the law (see 1281a36-39, 1282b8-13). From these general considerations let us pass to the specific difficulties Aristotle discerns in the divine law of Crete.

The Critique of Divine Law: Crete

Minos was the legendary son of Zeus renowned in the classical world for his justice¹⁰ and who, after keeping company with Zeus, gave to the island of Crete its laws (see, e.g., *Iliad* 13.450 and Plato's *Apology* 41a, *Minos*, and the beginning of the *Laws*; see also Huxley 1971). The Cretan regime is then ancient and of a most august heritage. It is also seminal. Carthage resembles Sparta most, but Sparta itself owes much to Crete, Lycurgus having spent considerable time there (*Politics* 1271b25–27; 1272b25–26). Minos is thus the example par excellence of the divine lawgiver in the *Politics*, Crete, of the divine city (or cities).

The first part of Aristotle's examination of Crete concerns domestic or economic questions, the second, the ordering of the regime, or political questions strictly speaking. Aristotle begins the latter by criticizing the Cretan manner of electing the orderers (kosmoi), for while the regime is not compelled to choose from the demos, it nonetheless selects at random those who possess no more merit than the demos. And although the senators (gerontes) are chosen only from those who have served previously as orderers, they are unaudited and rule in that powerful office for life. Finally, the demos meets in an assembly but is nothing more than a rubber stamp for the orderers and senators. While the fact of the demos's peaceableness is laudable, it is misleading: as Aristotle indicates presently, this tranquillity is due, above all, to Crete's location, distant from those

who would incite or corrupt the people. In his concluding remarks to the analysis of Crete (*Politics* 1272b7–23), Aristotle allows us to wonder whether the demos will not be infected by the instability of the orderers on the one hand and by a recent war on the other. With the exception of Minos's arrangements regarding the messes (1271a26–29; 1272a12–27), every virtue of the Cretan regime (the manageability of its slaves, the placidity of the demos, and the regime's very preservation) is traceable to its location, that is, to chance (1269a39, 1272a39–b1, b16–19). A recent attack on Crete suggested that that natural buffer was no longer effective. It would seem that for Crete and the Minoan legislation, long in a period of political decline, the writing was on the wall.

Aristotle's critique of Crete would indeed seem to cast a genuine shadow on the Minoan legislation. It is difficult to imagine that Minos was indifferent to the stability of the whole regime or to the very survival of its people (and therewith of the laws themselves). In other words, it seems that the Minoan legislation, as legislation, claims to do in a more perfect manner what merely human legislation tries to do, namely to secure the well-being here and now of the political community dedicated to the true good, the goodness of which is meant to be visible to all who have eyes to see. This essentially political character of divine law in the classical city has its counterparts also in our own tradition: "Behold, I have taught you statutes and judgments, even as the Lord my God commanded me, that ye should do so in the land whither ye go to possess it. Keep therefore and do them: for this is your wisdom and your understanding in the sight of the nations, which shall hear all these statutes, and say, Surely this great nation is a wise and understanding people" (Deuteronomy 4:5-6). It is precisely as political entities—meant to effect change for the better in this life and with a view to human beings as we know them-that the divine law and the divine city admit of the same kind of analysis as do the admittedly human institutions of Plato, Hippodamus, and the rest. By virtue of this claim, common to the laws of both Moses and Minos because common to law as such, there appears to be a common ground between Aristotelian political science as the science of the best regime on the one hand and the comprehension, necessary to the dutiful obedience to it, of the divine law or the virtuous city on the other. By making a claim meant to affect human beings in humanly comprehensible ways, therefore, the divine legislation is open to a human science, namely to political science.

This statement of the importance of political science nonetheless ignores a crucial possibility. Might not divine legislation have access to privileged knowledge unavailable as such to the scientist and in the light of which apparent failings on the plane of ordinary politics would reveal themselves as the mark of "Zeus," that is, of true perfection? Are not the ways of Zeus partly and even wholly mysterious? How is the scientist, including the political scientist, to proceed in the face of this claim? If at one point

Aristotle praises Minos for having "philosophized," is there any indication that Minos himself wished to be judged by the standard of philosophy or that this standard is in any way germane (*Politics* 1272a22–23)? If there is no solution to this difficulty that is acceptable in principle to both scientists and citizens of the divine city, the political scientist would be revealed to be a boaster whose putatively rational findings rest on a nonrational belief in the power of reason. Political science would remain at least as pretentious—and therefore as laughable—as Aristotle himself indicates it was at its "Hippodamean" origins (1267b22–30).

To be sure, maintaining this extreme position carries a price: the divine city and its laws cease to be indications to all of the wisdom and understanding of those who follow them. In the light of what understanding, then, would the adherent of the divine law be led to deny the otherwise powerful critique of the divine law that Aristotle presents? Some other consideration, some x, must be at work in the heart and mind of the faithful to lead them to do so. Insofar as Aristotle himself turns, immediately after his critique of other, would-be best regimes, to a consideration of justice (book 3), he at least raises the possibility that the x in question is the concern for justice. For the law wishes above all to be just or righteous and to instill in those who follow it an understanding of and devotion to justice or righteousness. The concern with the law points ultimately to the concern for justice, and divine law is distinguished above all by its superior justice. Such concern for justice, moreover, is at the center of political philosophy in general and of Aristotelian political philosophy, in particular. I suggest that this shared concern for justice supplies to both the citizens of the divine city and the political scientist a genuine common ground.

THE ANALYSIS OF JUSTICE

The analysis of justice, if it begins from and remains true to the concerns of most importance to the adherent of the divine law, supplies the proper starting point to understand that law and the piety at its core. The analysis of justice may therefore shed light on the difficulty with which book 2 as a whole leaves us, the problem of divine legislation and the status of political science as the science of the divine city.

The Problem of Justice in the Political Community

Aristotle begins the most important section of his analysis of justice (*Politics* 3.9–13) by stating a point of agreement between all political parties in their reasoning about justice and injustice, namely that justice is equality for those who are equal and inequality for those who are unequal (1280a9–22). Now this agreement, important as it is, does not go very far, for the wealthy, the free, the nobly born, and so on all contend that their peculiar excellence or preeminence is most important to the city's well-being and as such

is the just title to rule: "Some who are unequal in something, for example, wealth, suppose that they are unequal generally, and others who are equal in something, for example, freedom, suppose that they

are equal generally" (ibid., 22–25).

While the partisans may disagree vehemently regarding the rank of the various preeminences, they do not disagree-or would not, if pressed-concerning the avowed aim of the honor or office allotted thereon, namely, the happiness of the citizens living in a healthy political community. Aristotle devotes a good part of Politics 3.9 to fleshing out what is implied in this common presupposition because it proves to be, or at any rate to lead to, the immanent standard by which he judges the justice of all regimes. Aristotle argues that the various claims to rule advanced by the partisans look only to an incomplete version of what the city is meant to aim at according to the self-understanding of the partisans themselves. Just as the sharing of a common location, the practice of intermarriage, and commercial exchange do not by themselves constitute a city, neither do wealth, freedom, and military excellence yet make a political community in the full sense. All of these "must be present if there is to be a city, but there is not yet a city when quite all of these are present"; for the city is "a partnership in living well, for both households and families, for the sake of a complete and selfsufficient life" (1280b31-35). This means in turn living "happily and nobly," so that "one must posit that the political community exists for the sake of noble actions" (1281a2-3). Out of the turmoil characteristic of the fundamental disputes over the question of rule, then, Aristotle discerns this provisional agreement regarding the goal of politics as such: all agree that the community exists for the sake of a complete and self-sufficient, (i.e., a happy and noble) life, and all believe in principle that those who contribute unequally to this most important end should enjoy unequal reward. Politics 3.9 concludes with a concise statement of the standard of judging thus arrived at: "Accordingly, those who contribute most to such a community have more of a share of the city than either those who are equal or greater in freedom or family but unequal in political virtue, or those who outdo them in wealth but are outdone in virtue" (1281a4-8). And yet this principle of adjudication is perhaps not so unproblematic as it may seem, for Aristotle adds immediately, "It is clear from what has been said, then, that all of those who dispute concerning the regimes speak of a certain part of justice" (1281a8-10, emphasis mine). Could it be that no faction speaks of the whole of justice?

Aristotle begins *Politics* 3.10 by wondering "what the authoritative element of the city should be": The multitude, the wealthy, the decent or equitable (*epieikeis*), the one best, or the tyrant? And just as the principle of adjudication adduced in chapter 9 seems to be problematic, so "all of these [elements] appear to involve a difficulty" (1281a13–14; emphasis added). In order to spell this out, Aristotle focuses not on the fulfillment of the community's highest goals—on

"political virtue" and the performance of "noble actions"-but on the problem of securing, once any one group has come to power, that civil stability requisite to the higher goals indicated (ibid. 14-). It is unjust if the poor, by virtue of being the majority, rob the rich of their possessions, even if such robbery is sanctioned by the authoritative assembly. This will destroy the city, and virtue does not destroy that which possesses it. Such action is based on nothing but superior force and as such is indistinguishable from the actions of a tyrant. No better is the rule of the few over the many if they too will pillage the possessions of the ruled (ibid. 24-28). But one might well suppose that some of the few or the many would not be rapacious and could rule moderately. The next claim to rule mentioned in chapter 10 is precisely that of the decent or equitable human beings. Since these are necessarily few, however, their rule would exclude from office, and hence from the honor always associated with it, the vast majority of the community (1281a28-32). This same difficulty is all the more acute in the case of the rule of the one most outstanding human being. Thus the rich and the poor will be at one another's throats, and the decent, like the one best, will be at the mercy of the many indecent, whether rich or poor. Since Aristotle does not stoop to consider the tyrant's claim to rule, he substitutes for it a consideration of the rule of law (cf. 1281a13 and 34–39). It is wrong to suppose, however, that the laws can somehow rise above the interests and errors of the human beings who formulate them. The law itself must be either democratic or oligarchic, for example, and so will share in the defects inherent in those regimes (ibid. 34-39). There does not seem to be any easy solution to the problem of rule.

After attempting, with only limited success, to sketch a "republican" compromise to the problem of rule (Politics 3.11), Aristotle makes something of a new beginning at chapter 12 (cf. the beginnings of Politics 1 and 4 and the beginning of the Nicomachean Ethics). The political partisans are now said to agree not merely with one another but with "the arguments in accord with philosophy" that justice is certain equality (cf. 1282b18–20 and 1280a9–22). Justice will now be considered from the highest point of view, that of "political philosophy" (1282b23). Aristotle argues at some length that the capacity or preeminence serving as the basis of the distribution of goods must be relevant and that the well-born, the free, and the wealthy thus all have a genuine claim, since there cannot be a city of slaves or paupers. Furthermore, "justice and the military virtue" are necessary to a city's being nobly administered, and those who excel in these have a claim, as well. To this point, Aristotle's argument recalls the lower standard of adjudication seen in *Politics* 3.10–11, namely, that which looks to the demands of the city's existence. The crucial shift occurs at the beginning of chapter 13: "All, or at least some, of these might be held to contend [for office] correctly with a view to the city's existence. However, with a view to the good life, education and virtue, above all, would contend justly" (1283a23-26;

emphasis added). That is, good birth, wealth, freedom, military virtue, and even justice are necessary preconditions to "the good life"; but they do not by themselves constitute that life. While chapter 13 thus recalls that other, higher strand of justice seen in chapter 9 that looks to the good of each, it is not clear, now that "political philosophy" has been introduced, whether even these two "higher" standards are simply identical. Whereas in chapter 9 the higher end whereby equality and inequality were to be judged was the happy and noble life consisting of the performance of the "noble actions" proceeding from "political virtue" (1280b5, 1281a2-3, 7), the end in question here is simply "the good life" to which education" and "virtue" are means. It is clear, in other words, neither whether the performance of "noble actions" is the same as "the good life" nor whether "political virtue" is equivalent to "virtue" unqualified. Might "political philosophy" lead beyond the claim of any one political partisan, or beyond politics as such, in the search for the true good?

This possibility is played out in Aristotle's treatment of kingship or, more precisely, of a certain kind of "absolute kingship" (Politics 3.13-18).12 If some human beings should arise in the city who surpass all others in point of virtue and political capacity and yet are insufficient in number to found a city unto themselves, one nevertheless ought not to consider them a part of the city: "for they shall be done an injustice in being deemed worthy of equal things when they are unequal to such an extent in regard to virtue and the political capacity"; a member of them is "like a god among human beings" (1284a3-11). Since the political community is an association of free persons similar in kind and since the law or legislation is an integral part of any political community, it is necessarily the case that "legislation pertains to those who are equal in regard to stock and capacity and that there is no law for such [outstanding] persons. For they are themselves a law" (ibid. 12-14).

Would not the best regime, however, as such take its bearings by justice in the sense of giving to each what is truly good for him? "As for the best regime, there is much perplexity as to what one ought to do if someone arises who differs, not by being outstanding with respect to the other goods (e.g., strength, wealth, and abundance of friends) but in point of virtue. For surely they would not say that they ought to expel and cast out such a person, but neither ought they to rule such a one. It would be as if, in dividing up the offices, they deemed themselves worthy to rule over Zeus" (Politics 1284b25-31). According to the higher principle that Aristotle has outlined to adjudicate the claims to rule—and to which he here returns—the human being of outstanding virtue in the best regime must rule: "So it remains—a fact which seems naturally to arise—for all to obey such a person gladly, so as to make such persons permanent kings in the cities" (ibid. 32–34). This, then, would seem to bring to a close Aristotle's discussion of justice and rule. All regimes wish to promote the common good as they see it, and all wrongly suppose

that the part of excellence they call their own is the whole of excellence. All regimes, even and especially aristocracies, would have to yield in the face of the one best human being. Given the vivid portrait in the Politics of the quarrels between the various political factions, however, does Aristotle believe in all seriousness that, while it may be in some sense "natural" to yield to such a one, any regime, including the best regime, would do so "gladly" (e.g., 1281a16, b18-20)? The merit to rule must be accompanied by the capacity to be obeyed (1284b32), and we have seen already the difficulty involved in the claim to rule of even the decent or equitable human beings: in order to be obeyed, they would likely have to employ the services of the indecent—of a "people's security force"—and this changes fundamentally the character of the rule. Can kingship solve this aspect of the problem of rule (see Vander Waerdt 1985, 261-64)?

In order for the absolute king to rule justly in the sense both of the deserts of the individual and of the common good, not only must he possess the true peak of virtue (whatever that may be) but the multitude must recognize that virtue for what it is and thus willingly accept his rule. If these two criteria are met, the rule of an absolute king is fully just—and is so, Aristotle argues, according to the logic of the arguments of those who found aristocracies, oligarchies, and democracies (Politics 1288a19-25). What, then, is the outstanding virtue in question that is as such the title to rule? However vague Aristotle may be here, he is as forthright as possible in the Nicomachean Ethics 10.7–8: theoretical (contemplative) virtue is superior to moral virtue, and one possessing the higher virtue to a greater degree is superior to one possessing the lower (1098a16–18). Even in the present context, Aristotle determines "virtue" in the light of "the good life" (Politics 3.13), and in the final analysis, the good life is the philosophic one, as is confirmed not least by Aristotle's sketch of the best political life in chapters 7-8: the best regime is best because it is directed above all to "leisure," the closest imitation of the philosophic life that is possible within the confines of the political community. Aristotle ultimately judges political life, that is, in the light of a philosophic and apolitical standard. Wisdom is in a sense the supreme title to rule, and the human being who possesses it most merits rule above all others. The extreme kingship under discussion is, in short, the rule of a "philosopher-king." Aristotle's silence on this singular institution in Plato's Republic 2.2–5 is to some extent compensated for by his discussion here of the "absolute king" (Vander Waerdt 1985, 268-73). To be sure, this is the political—and therefore limited or distorted-presentation of the philosopher, the only presentation possible in the Politics.

Once one sees the extreme character of the kingship in question, ¹⁴ the difficulty of its realization becomes clear. Only in primitive or barbaric times would there likely be a community willing to accept such absolute rule, but only in later times is one likely to find a human being of such outstanding virtue, for philosophy does not belong to the early times (*Politics*) 1252b19-26; 1285a16-18, 30 and b4-5; 1286b20-22; see also Strauss 1964, 37). It is thus unreasonable to expect both of the foregoing conditions ever to be met, and the absolute kingship Aristotle here describes is for all practical purposes impossible. The description of this kingship is then a kind of experiment on Aristotle's part, an attempt to give virtue its day in the political community. As quickly becomes evident, the political community properly speaking (i.e., the association of free and equal persons meant to secure a complete and self-sufficient life) cannot tolerate the peak of human excellence, so great is the possible disparity in virtue among human beings. The political community must either become something like a large family headed by a human being of outstanding virtue as a kind of beneficent parent or it must ostracize him. The discussion of this extreme kingship is thus linked with the preceding discussion of justice because, while it more or less ignores the strand of justice that takes into account civil stability (and hence the consent of the governed), it takes most seriously the other strand of justice that promises to provide true virtue and happiness to all, including those capable of attaining the peak of virtue.

Justice and Divine Law

Let me try to summarize the preceding and bring out its implications for my theme. In every political community, human beings hold fast to deeply opposed opinions concerning the true political or common good. These conflicts over what constitute the essentials of a good life impose limits on the extent to which any one faction can force its own view of the good on the citizen body as a whole, for the group in question would be compelled either to yield to the conflicting claims that always threaten to erupt into civil war (i.e., dilute its claim to rule on the basis of X) or to resort to violence to enforce it (i.e., pollute its claim to rule on the basis of X). Moreover, if a human being should arise who embodies what is according to Aristotle the genuine peak of human excellencephilosophy, or contemplation—the legislator must nonetheless act "with a view to the advantage of the whole city and the common [advantage] of the citizens" (Politics 1283b40-42). Even the "most correct laws," then, look not to the highest that a human being can attain but to the advantage of the whole or to that which is common to all the citizens (ibid. 38). It is too much to hope that laws can look to—much less secure—the highest human good. This means that even the best law or the best regime is only imperfectly just: every community is compelled to secure what I have called civil stability. The attainment of justice in this sense prevents the community from being just in the higher sense of giving to each what is his due, that is, what is good for him.

Any appeal to the divine, then, insofar as it wishes to be political, is similarly confronted with the brute fact of the persistence of conflicting opinion regarding the good and of the recalcitrance on the part of most human beings to alter or adapt their deepest convictions. In the absence of a wholly unambiguous sign available to all alike, the appeal in question would not enjoy the unanimity that proceeds from knowledge alone (see Isaiah 11:9). The heterodoxy of nay-sayers or "heretics" would pose a *political* problem whose resolution would require either the dilution or corruption of the claim to rule and therefore would cast a shadow on the justice of the claim in question.

More important, if the general thrust of the preceding analysis of "absolute kingship" is correct, Aristotle means to suggest that no political community can be fully just because none can give to the peak of excellence its due within the confines of a community. The highest human good is then suprapolitical. By contrast, the divine law, as law, maintains that human happiness is attainable on the plane of politics (properly guided and supplemented) or that the problem of justice admits of a political solution. The most powerful reason for this greater optimism concerning politics is the contention that the gods are not only political but also providential; that is, they look after the city and its citizens and provide for their well-being in accord with the justice or injustice of their actions:

It is quite clear that both Greeks and Barbarians believe that the gods know all things, present and future. All cities, at any rate, and all tribes ask the gods, by means of divination, what they ought to do and what not; all at any rate ask the gods to avert wretched things and grant the good. So you see, these gods who know all things and are able to do all things are my friends in such a way that, through their care for me, I have never yet escaped their notice, night or day, wherever I may be setting off or in whatever I may be about to do. Through their foreknowledge of what will result from every action, they signify to me whatever I must do and what I ought not to do by sending me voices, dreams, and birds of omen as messengers. Whenever I have obeyed them, I have never yet repented of it; but now and again, when I failed to trust in them, I was punished. (Xenophon, Symposium 4.47–48)

At the core of the law, then, is the belief in providential gods. Such gods know full well that some excellent individuals are in a sorry and grievous plight whereas some wicked individuals are in good and pleasurable circumstances" (Maimonides, Guide of the Perplexed 3.16). They know "the private and public fortunes of bad and unjust human beings ... and that there are some who have partaken of many terrible impieties and through these very things have risen from low positions to tyrannies and the highest stations" (Plato, Laws 899e-900a). Being manifestly concerned with human affairs, however, the gods will rectify, in this life or after death, any discrepancy between merit and reward. In short, providential gods ensure the eventual prosperity of the just and the suffering of the unjust. It is not only the attachment to political justice, then, that might lie behind the rejection of Aristotle's rational science of the divine city but rather or more precisely the understanding that the gods support that attachment by making justice unambiguously good for one. It follows that however perplexing the *means* of providence may appear to us here and now (e.g., the very destruction of the political order based on the law), no one who is seriously devoted to the law would deny *that* the gods will, in their manner, provide for the good of the just. Even the destruction indicated must ultimately be held to be for the best. And to defend the law by appealing to the mysteriousness of Zeus runs the risk of blasphemy if in so doing one renders doubtful the fact of the goodness of the law and hence of divine providence.

The necessity of this recourse to providence implies that justice or moral virtue is, by itself, an insufficient end of political life because it does not, by itself, always provide for the happiness of the just or virtuous. It is important to see that so far from denying this view implicit in the law, Aristotle agrees with it: "Now someone might suppose that this [i.e., moral virtue] is even more an end of the political life [than honor]. But virtue too seems to be a rather incomplete end. For it seems possible for one to possess virtue . . . while suffering terribly and undergoing the greatest misfortunes. No one would say that a person living in this way is happy" (Ethics 1095b30-1096a2). The question with which we are concerned may now be made more precise: Since Aristotle, too, concedes the difficulty with moral virtue that belief in providential gods would solve, why does he look not to piety but to contemplation as its necessary supplement?

To begin with, the resolution of the problem of the goodness of justice by recourse to providential gods bears witness to Aristotle's dictum, "Everyone does everything for the sake of what is held to be good" (Politics 1252a2-3; see also, e.g., Politics 1324a33-35; Ethics 1129b5-6 and context, 1155b23-26, 1159a10-12, 1166a14-18). For the devotion to the common good and the willingness to sacrifice oneself on its behalf, while noble and hence attractive on their own, do not go unnoticed by superintending gods. Such sacrifice is as a result made still more attractive because it holds out the promise of our happiness: "One who, in accord with his capacity, does not fall short in honoring the gods should take heart and hope for the greatest goods. For one who is sensible would not have greater hopes from others than from those who are capable of bestowing the greatest things, nor would he be more sensible in any other way than by pleasing these [gods]" (Xenophon, Memorabilia 4.3.17). Human beings cannot but pursue what seems good to them, and the very belief in providential gods concurs with—in fact it affirms and underscores—this philosophic view according to which what we hold to be good is our primary end or aim. Here too, then, Aristotle and the divine law are at one.

It is at this point that one comes to something of a crossroads, for much (not to say everything) depends on one's understanding of the manner in which the good appears to us. Aristotle's own view is difficult of access, and it may be helpful first to sketch a philosophic position he presents but explicitly rejects in

the Nicomachean Ethics. According to it, not only is the good primary, but human beings are not in the end responsible for the manner in which that good appears to them. It is therefore irrational either to praise or to blame another on moral grounds. Aristotle reports this contention as follows:

Someone might say that all seek what seems good, but that they are not in control of the appearance [of the good]; rather, whatever sort of person each one is determines the sort of end that appears to him. Now if on the one hand each one is somehow responsible for his own character [hexis], then he will be somehow responsible also for the appearance [of the good]. If on the other hand he is not responsible, no one would be responsible for his acting badly; instead, he does what he does through ignorance of the end, supposing that through these actions, he will attain what is best for himself. The aiming at the end is not of one's own choosing, but necessarily arises by nature, just as one possesses sight, by means of which he shall judge nobly and choose what is truly good. He is of a good nature to whom this has nobly arisen by nature. For this is the greatest and noblest thing, and cannot be taken or learnt from another; rather, such a one possesses it by nature. When this has well and nobly arisen by nature—this is the complete and true meaning of "good-natured." (Ethics 1114a31-b12)

According to this view, moral responsibility depends on our being in some way responsible for our hexis ("character," "fixed state [of soul]"). The hexeis, moreover, "arise from the corresponding activities. Hence it is necessary to make the activities of a certain quality, for the hexeis follow upon the differences among these [activities]. It makes no small difference, then, whether one is habituated in this or that way from early youth-it makes an altogether considerable difference, or rather the whole difference" (Ethics 1103b21-25). The familial circumstances into which we are born and the upbringing we receive are as little our own doing and so as little our responsibility as the still more determinative laws there current (see 1179b31-32). It is therefore difficult to see how, according to this argument, we are responsible for our hexis and hence for the way in which the good appears to us. But is it not up to us to seek out reason and instruction to remedy what our circumstances may have denied us? Alas, "reason and instruction are not effective in all cases. It is necessary instead to prepare beforehand the soul of the listener by means of habits with a view to rejoicing and hating nobly, just as the land that is to nourish the seed [must be prepared beforehand]" (ibid. 23-26). We seem to have arrived at the paradox that only a proper upbringing can make possible one's openness to the instruction capable of remedying a deficient upbringing. If some do seek out instruction and alter their behavior accordingly—if they pull themselves up by their own bootstraps, so to speak—what could either lead them to do so or enable them to be benefited thereby, apart from a particularly good nature? Yet "what comes from nature is clearly not present on account of us, but rather is present to those truly fortunate ones through certain divine causes" (ibid.

21–22). According to this view, then, some combination of nature and nurture always determines our *hexis* and hence the way in which the good appears to us.

This understanding of the determined character of the good would have important consequences for belief in providential gods and, therewith, the adherence to divine law. For the very gods who are concerned with human affairs make known that concern by punishing those who choose badly or basely and by rewarding those who choose well or nobly. One's own good, then, is provided for with the greatest possible certainty even as one attempts to secure, through self-sacrifice, the good of others. Moreover, since the determinants of the appearance of the good can always be traced, according to this argument, to some cause "outside" the agent (nature, upbringing, or both) we are not strictly speaking responsible for the choices in question. But this means that it would be wrong, precisely on moral grounds, to punish those who cannot help but pursue the good as they cannot help but conceive it. To suppose that the gods punish those who act under the force of compulsion would be to attribute gross injustice to them—to deny the fundamental character of the gods and hence of the law (see Thucydides, Peloponnesian War 5.105.1-3). There is so to speak nothing left for the gods to do. The expectation of divine intervention would be irrational on terms ultimately agreeable to and even demanded by the suppliant. In brief, the divine law presupposes the rationality of responsibility while at the same time corroborating unawares the philosophic position that denies it, insofar as the law promises to secure the good of the obedient. The law therefore contradicts itself, and it could in principle be shown to do so to anyone willing to listen.

To repeat, Aristotle explicitly rejects the main premise of this argument, as appears toward the end of his discussion of responsibility (*Ethics* 3.5): "Whether indeed the end, of whatever sort it may be, appears to each not by nature but is to some extent present in himself, or whether the end is natural, virtue is voluntary in that the morally serious man [spoudaios] does all else voluntarily. . . . If then, as it is said, the virtues are voluntary (for we are somehow jointly responsible for our characters and we have posited this or that end by virtue of our being of a certain sort), the vices would be voluntary as well" (1114b16–24 and context).

One might say provisionally that Aristotle's own, more moderate approach is a mean between the view just sketched and the understanding presupposed by the law, for while refusing to call into question the responsibility of the individual, he takes a step in this direction as regards the regime. We have seen that according to Aristotle, every regime understands itself to be based on the truly relevant claim to political rule and therefore to embody and to foster true virtue. Every regime, in other words, is concerned to justify its rule and believes that it can do so. The variety of regimes is therefore traceable to the

variety of the views of what is truly good (see the beginning of *Politics* 4.3, 1289b27–1290a13 and 1328a40–b2). All are concerned with (what they take to be) the highest things and strive after (what they take to be) the good. As Aristotle states at the beginning of the *Politics*, "Every community has been constituted for the sake of some good, for everyone does everything for the sake of what is held to be good" (1252a1–4, emphasis mine; see also the beginning of the *Ethics* 1094a1–3).

This means in the first place that since all regimes are concerned with what they believe to be good, it cannot be said that they knowingly or voluntarily found a vicious regime or that they voluntarily instill vice either in themselves or in others. As a result, the distinction that Aristotle draws in Politics 3.7-8, according to which the unjust regimes look to the advantage of the rulers rather than to the common advantage, or ignore justice in order to pursue their own good, is too simplistic because it overlooks the concern present to all regimes to justify their rule in terms of a common good. In chapter 13, Aristotle maintains rather that the "deviations" are simply mistaken as to what they take to be the just basis of the distribution of prerogatives. They are nonetheless very much concerned with justice. Aristotle accordingly concludes there that "all dispute in a certain manner justly, but all do not do so justly without qualification" (1283a30-31, emphasis mine)—that is, democracy and polity, oligarchy and aristocracy, tyranny and kingship are all to some extent just but none is so unqualifiedly (see 1279b11-15). In the Ethics, Aristotle gives a more adequate statement of the distinctions among regimes that takes into account the primacy of the good on the level of the regime: "By instilling [good] habits in the citizens, the legislators make them good, and this is the wish of every legislator. All those who do not do so well, miss the mark; it is in regard to this that a good regime differs from a poor one" (1103b2-6; emphasis mine). It is therefore more reasonable to conceive of defective regimes as foolish, blind, or mistaken than as unjust or evil. To demand retribution or revenge—to call upon the gods to punish them—may be politically salutary, but it is strictly speaking irrational (cf. Plato, Laws 860d and 873e-874a).

There is thus a gradual movement in Aristotle's political science away from considerations of justice toward those of what is good for human beings. This movement, which begins by "listening" to political argument and by taking seriously the self-understanding of the regimes, culminates in the denial of the possibility of perfect justice on the level of the regime. Not so much justice and its demands, then, as human nature and its health take pride of place. And as the good ascends in importance, so too does theoretical virtue, for in the end the greatest task comes into sight as the pursuit of those virtues whose very possession constitutes the happiness of their possessor and that therefore require no external support or reward. Such is the description in the *Ethics* of theoretical, as opposed to practical, virtue (see

1178a23–1178b5). However rare the full embodiment of that higher virtue may be, however fleeting or occasional its pleasures for the rest of us (see Aristotle, *Metaphysics* 1072b25), the peak of human happiness (according to Aristotle) consists nonetheless in the activity of philosophy. Aristotle's political science would therefore seem to deny the necessity, in order to be happy, of the support that providential gods supply. There is so to speak nothing left for such gods to do.

By thus thinking through our profound longings for happiness, which most naturally find their first expression in the hope for a "best regime" guided by a perfect law (i.e., in political terms), we may come to see the transpolitical activity that is according to Aristotle their true fulfillment. We may become genuinely open to the possibility that the human mind is at home in this world, that looking upon or contemplating it is in principle sufficient for happiness, and that there is as a result no need to make further demands upon the given world. One could put this another way by saying that according to Aristotle, human beings are by nature best suited to worship the god he sometimes describes—the god, that is, who "rules" by being the object of our love and who seems in the end to be indistinguishable from the activity of contemplation (Eudemian Ethics 1249b13-25; Ethics 1178b21–22 and context; Politics 1323b23–26; 1325b28-30).

CONCLUSION

By sketching Aristotle's critique of divine law and his corresponding defense of the possibility of political science, I have tried first and foremost simply to recover as a genuine problem the difficulty with which Aristotle dealt. I cannot hope to have done more than offer a tentative and heuristic outline of the premodern understanding of political things that affirmed the sufficiency of human reason to guide political life against those who, by appealing to suprarational edicts or to special knowledge, would deny it. The preceding remarks, that is, are intended merely to indicate the nature of the doubts that Aristotle's analysis seems to raise concerning divine law as such. For those who remain dissatisfied, I appeal to the authority of the great medieval student of Aristotle, Moses Maimonides (Guide of the Perplexed 2.22):

You may say: Can doubts disprove an opinion or establish its contrary as true? Surely this is not so. However, we shall treat this philosopher [Aristotle] as his followers have enjoined us to treat him. For Alexander [of Aphrodisias] has explained that in every case in which no demonstration is possible, the two contrary opinions with regard to the matter in question should be posited as hypotheses, and it should be seen what doubts attach to each of them: the one to which fewer doubts attach should be believed. Alexander says that things are thus with respect to all the opinions regarding the divine that Aristotle sets forth and regarding which no demonstra-

tion is possible. For everyone who has come after Aristotle says that what Aristotle stated about them arouses fewer doubts than whatever else might be said about them.

As for the applicability of this analysis to the comparable challenges that liberalism is now, once again, beginning to face, it would be premature to attempt to traverse the very great distance between Aristotle and ourselves before grasping adequately Aristotle's own understanding, and in what follows I shall remain at the level of the difficulty at hand, rather than its resolution. I suggest that reflection on the problem at the core of Aristotle's science of the best regime can help us to make full use, on an individual or private basis, of the extraordinary freedom we enjoy, in the first place by bringing home to us the considerable price at which that freedom was purchased; for in order to quell the political tumult that had plagued the "petty Republics" of antiquity (Federalist 9), the modern liberal state saw fit to secure principally those conditions of communal life needed to avoid the incontestable summum malum-violent death—while relegating to the private sphere the necessarily controversial questions concerning the summum bonum. Chief among the latter, however, were opinions about God, the coming into being of the universe, and man's place in that universe. In the end, it was only by "detaching religion from the soul" that liberalism could secure the stability and liberty it sought, and the best way to "attack a religion" proved to be "by favor, by the commodities of life, by the hope of fortune; not by what draws attention to it, but by what makes one forget it; not by what makes one indignant but by what leads to indifference." Even the question of the immortality of the soul belonged, in the final analysis, to the class of "indifferent things," men being made "to preserve, feed and clothe themselves, and to do all the things of society" (Montesquieu, De l'esprit des lois 25.12; see also 24.11, 19-22).

This public and therefore respectable indifference to matters of religion eventually made it possible and perhaps even necessary—for the citizen of the liberal republic to believe, with Jefferson, that "it does me no injury for my neighbor to say there are twenty gods or no god. It neither picks my pocket nor breaks my leg" (1984, 285). 15 Reflection on Aristotle's science of the best regime is therefore helpful in counteracting this indifference, engendered by liberalism, to serious thoughts about God or "whatever it is that keeps this All together" (Politics 1326a32-33), thoughts that in every nonliberal time and place are held to be of first importance for human beings. Once alerted to this deficiency, moreover, there is no manifest reason why one cannot subsequently revive the debate indicated between the two competing accounts of the world, that is, make an attempt to secure, on an individual basis, the liberation that the core of Aristotle's political science seems to promise and that can be achieved, according to him, only by individuals as such.

It is true that no direct benefit is therefore likely to

accrue to modern republicanism with the recovery of Aristotelian or classical rationalism (but see Pangle 1992, 105-59). Indeed, Aristotle would have considered irrational any attempt to found a society on purely rational principles in the absence of all public belief in the gods, his own expectations in this respect being extremely modest (*Politics* 1328011-13; 1329a27-34; 1330a11-13; 1331a24-30 and b4-6, 17-18; 1336b16-17). 16 Still, it is not irrelevant to the health of liberal democracy if some of its members come to understand and make use of Aristotle's powerful arguments in favor of common decency, responsible self-government, and political moderation wherever these may arise—arguments grounded in neither party interest nor "ideology" but philosophy. And however different that philosophy surely is from modern liberal rationalism, precisely as an attempt to perfect human reason it is closer in spirit and a greater friend to the liberal rationalism embodied by the original principles of the American founding than are the many varieties of irrationalism that would supplant and to some extent have supplanted those principles.

The self-understanding of science or rationalism, ancient or modern, is destined to be revealed as an empty posture or groundless act of the will unless it takes seriously the question of the *possibility* of science or rationalism, and this means above all a confrontation with the claims of the divine. The present study has tried to bring to light and to life those claims, albeit in a context very different from our own, together with the outlines of one attempt to meet them, in order simply to encourage the kind of reflection necessary to the reinvigoration here and now of reason or that which is, according to Aristotle, closest to the divine in man (*Ethics* 1177b26–78a8, 1178b20–28).

Notes

1. Numerals indicate book and chapter; and where a more precise reference is required, I have used the standard Bekker numbers as these appear in Dreizhenter's edition. Translations from the Greek are my own.

2. This is not to deny that attempts have been made to graft liberal principles onto Islam, but the cool reception such efforts have received underscores the fact that the separation of church and state is essentially foreign to it. See Binder 1988,

esp. chaps. 4 and 9.

- 3. Whereas according to the Islamic philosopher Avicenna, in his Division of the Rational Sciences, "the treatment of prophecy and the Law is contained in their [Plato's and Aristotle's] two books on the laws," modern scholars have all but forgotten this aspect of classical political science (Lerner and Mahdi 1972, 97). Two recent essays have broached the subject of the religious implications of Aristotle's political philosophy, although each differs substantially from the procedure and findings of the present study (Lindsay 1991; Ponton 1991). Otherwise helpful general accounts include Barker 1959, Bluhm 1962, Huxley 1971, Mulgan 1977, Morrall 1977, and Newman's (1973) standard commentary on the Greek.
 - 4. The turn is prepared by the end of book 1 (1260b8-24).
- 5. The precise identity of this Thales is not known. At the very least, his name recalls (one could almost say "stands for"

philosophy. See *Politics* 1259a6, 18, 31; Newman 1973, 2:379. Jaffa (1972, 91) and Nichols (1987, 172–75) assume that the philosopher is meant.

6. Thus *Politics* 2.12, far from being a spurious or random sampling of the deeds of various legislators, reiterates the crucial question informing the whole of book 2 (see, e.g., Newman 1973, 2:373 and the helpful remarks in Keaney 1981).

7. This is Aristotle's frequent formulation. See *Politics* 1260b29, 1265a17-18, 1288b23, 1295a29, 1325b36 and 39,

1327a4, 1331b21.

- 8. It is true that Aristotle often appears to disregard or dismiss the peculiar status of laws of an emphatically divine origin and to consider piety and the divine in terms of their political utility (Politics 1252b26-27, 1269a28-31, 1335b12-16; Ethics 1133a2-4). Priests, for example, should be separate from and subordinate to political rule strictly speaking (Politics 1299a17-19; 1322b18-22; 1328b11-13; see also 1314b38-1315a4). Nevertheless, according to Aristotle, "'to philosophize' means both wondering precisely whether or not one should philosophize, as well as pursuing philosophical contemplation" (Protrepticus, fr. B6; emphasis mine). Philosophy requires, in other words, that one reflect on the goodness of the philosophic life as compared with its principal contenders, such as the life of pious devotion. Moreover, the history of the transmission of the Politics itself suggests that Aristotle was very much concerned with the question indicated. For no translation of the Politics (at any rate, no complete version) was available to the Islamic and Jewish philosophers of the Middle Ages, and one must at least consider the possibility that this was "the result of a deliberate choice, made in the beginning of this medieval development" (Strauss 1937, 97; see also Pines 1986; Strauss 1990, 5 and 24 n. 2). That is, in contrast to Plato and to his own theoretical inquiries strictly speaking, Aristotle in his political philosophy may address the grounding of the superiority of the philosophic life most directly and hence be particularly obnoxious to the competing claims appealing to divine revelation understood to be the perfect law given by God to a prophet. The testimony of Hobbes supports the view that Aristotle's more "philosophical" writings were in crucial respects exoteric, above all in regard to religious belief. After enumerating the influential errors of Aristotelian metaphysics, Hobbes says, "And this shall suffice for an example of the errors which are brought into the Church from the entities and essences of Aristotle; which it may be he knew to be false philosophy, but wrote it as a thing consonant to and corroborative of their religion, and fearing the fate of Socrates" (1968, 692). I mean to suggest that one should look first and foremost to Aristotle's moral and political writings to discover the rational grounding of his otherwise cavalier view of the gods. Such a grounding, if it exists, would have made difficult or impossible the reception of the Politics in the Jewish and Islamic Middle Ages and may be sufficient to explain the apparent absence of the book. As for the enthusiastic reception of the Politics in the Christian Middle Ages, the critique of the divine city and the divine law present in the Politics have little direct bearing on Christianity, which is strictly speaking without a law or a political teaching. Moreover, insofar as Christianity came to wish to be political, it found itself very much in need of and hence receptive to the science of politics provided by Aristotle's treatise (see Fortin 1987, 248-51).
- 9. See Leyden 1967 and Schroeder 1981 for helpful discussions of Aristotle's view of law.
- 10. The classical accounts concerning Minos are somewhat ambiguous. The reports of his education at the hands of Zeus and his justice in general must be balanced against his demand for human sacrifices, for example, as a result of which he was hated by Athens. See Plato, Laws 624b4–25a1, 706a4–c1; idem, Minos 318d4–10.
- 11. Aristotle may refer to the attack of Phalaecus and his mercenaries in 345 B.C., the subjugation of the island at the hands of the Spartan Agesilaus (brother of Agis III) in 333 B.C., or both. See Newman 1973, 2:360.
- 12. Interpretations of kingship in the *Politics* fall into roughly two groups. According to the first, Aristotle had a

genuine interest in and (qualified) preference for absolute monarchy, either on political grounds (Kelsen 1937) or philosophical grounds (Newell 1987; Vander Waerdt 1985). According to the latter, Aristotle denied the possible goodness of kingship and consistently preferred a more "republican" form of government. A recent and extreme example of this line of interpretation is that of Nichols (1992), who writes as if the absolute king were, according to Aristotle, a would-be tyrant. See the helpful remarks of Vander Waerdt (1985, 251, n. 4) and Mulgan (1974a and b). My own interpretation is perhaps closest to Vander Waerdt and Newell.

13. I therefore agree with Vander Waerdt that the virtue characteristic of this somewhat mysterious absolute king is "more exalted than any moral excellence ordinarily attainable by human beings" (1985, 267), but I am not persuaded that the virtue in question is "a kind of heroic or even divine virtue which differs in eidos from both moral and philosophical virtue" (p. 264). Vander Waerdt himself expresses some puzzlement as to why this lofty virtue is not equivalent to philosophic or theoretical virtue (1985, 268, n. 25). It is true that Aristotle's discovery or invention of practical wisdom (phronêsis) as a sphere of knowledge essentially distinct from theoretical science would seem to make impossible the identification argued for in the text. Even the sober Aristotle, however, is compelled to address questions of metaphysics or theology when speaking of the best regime—the highest theme of practical or political philosophy (see, e.g., Politics 1323b21-27, 1325b28-32). The distinction between theory and practice, in other words, breaks down when one attempts to answer adequately the question of the best way of life for a human being. The invention of phrônesis does have the politically salutary effect of bestowing a science upon morally virtuous actions and thus supports the view that such actions are simply rational.

14. Aristotle begins by mapping out the four principal kinds of kingship to which he curiously appends, after a summary and conclusion (Politics 1285b20-29), a fifth sort, namely the rule of one person who is authoritative over "all" in the manner of economic (household) rule and who "does everything in accordance with his own wish" (1287a1, 9-10). This fifth sort, the only one of which no example in any time or place is given, is at the opposite extreme from the strictly limited Spartan kingship, and Aristotle very quickly narrows his focus-first to these two, then to the fifth sort alone. He thus makes it clear that what follows will be concerned with a

certain extreme case.

15. Consider also Jefferson's remarks concerning the absence of established religion in Pennsylvania and New York: "Religion is well supported; of various kinds, indeed, but all good enough; all sufficient to preserve peace and order. . . . They have made the happy discovery, that the way to silence religious disputes, is to take no notice of them" (1984, 287).

16. Aristotle brings "enlightenment" to the political community only insofar as he attempts (1) to subordinate (but by no means eliminate) the care of the divine vis-à-vis political office strictly speaking (*Politics* 1299a17–19, 1322b18–22, 1328b11–13); (2) to make plausible the contention that the city is a natural rather than a divine entity, the burden of book 1; and (3) to elevate the avowed aim of political life from action (especially war) to rest or leisure properly understood. The strictly limited nature of Aristotle's political enlightenment and his corresponding (and qualified) endorsement of the divine as a necessary object of political concern, stem from his doubts about the possibility of a universal or even widespread freedom from the need to make demands upon the world and hence from the need to believe in the extraordinary means required to satisfy those demands.

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OASES IN THE DESERT: HANNAH ARENDT ON DEMOCRATIC POLITICS

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annah Arendt never wrote systematically on the subject of democracy. In her book of greatest relevance to the subject, On Revolution, she criticized liberal democracy, and defended a conception of virtuous political "elites," leading most commentators to view her as an opponent of democracy. I argue that Arendt defended a distinctive conception of grass-roots democracy, and that her conception of elites is distinctively democratic rather than anti-democratic. I bolster this argument by examining her historical context, and conclude by assessing the relevance of Arendt's conception of democracy.

Then considering the subject of democracy, the name of Hannah Arendt does not quickly spring to mind. Arendt's work is sometimes considered classical, sometimes Nietzschean-but almost always elitist and thus antidemocratic. She argued strenuously in favor of a vigorous, participatory politics. Yet, as Sheldon Wolin has pointed out, she never, in her many books, systematically addressed the topic of democracy (Wolin 1983, 3). She surely was no "democratic theorist" as this term has usually been understood. In her most important work of relevance to the subject—On Revolution, in which she endorses spontaneous forms of political insurgency-Arendt disparages representative government in favor of a politics conducted by an "aristocratic elite," drawing from, among others, John Adams, hardly renowned as a partisan of democracy (1977a, 275-76). These heresies of omission and commission have led many theorists to concur with Margaret Canovan, who has argued that there is a "deep and serious contradiction" between Arendt's elitist and democratic sides, a contradiction that severely limits the relevance of her thought to democratic concerns (1978, 5–6).²

I believe that this interpretation is mistaken, both about Arendt and about the subject of democracy. It would be foolhardy to insist upon a perfect fit between Arendt and democratic ideas, but for two reasons. The first, which scholars have often noted, regards the ambivalences in Arendt's work. While not a nostalgic thinker, she was clearly drawn to certain features of ancient politics and was revolted by many features of modernity, most notably its vast, impersonal character. Arendt disavowed all labels, including the label "democrat"; and her writings elude easy political classification.³ The second reason for the imperfect fit between Arendt and democracy has less frequently been noted. It is that there is no neat, consistent, and self-sufficient package called "democratic ideas" in connection with which Arendt's writings can be judged.

Democracy is a complex set of ideals and possibilities. The clear-cut association of democracy with liberal constitutional regimes is a fairly recent—and problematic—development (Hampsher-Monk 1983; Hanson 1985, 1989). Arendt was often elusive to be

sure, a source of continuing frustration for political theorists. But on the subject of democracy, she was less elusive than scholars have believed. Although she refused the label "democrat" because she correctly saw that in the modern world, this term admitted of different, sometimes hostile, interpretations, this did not make her an antidemocrat. On the contrary, while Arendt was quite clearly against mass democracy, she was not against democracy per se. Indeed, On Revolution advances a vision of grassroots politics that is supremely democratic in its emphasis on the participation of ordinary people in their own governance. Even here, things are more nuanced, for to say that she was against mass democracy is not to say that she anywhere proposed its wholesale transformation. Rather, she supported a kind of insurgent politics, rooted in civil society, that would invigorate rather than replace mass democratic politics.

My argument will turn the conventional argument about Arendt and democracy on its head. Far from there being a tension or contradiction between Arendt's elitist and democratic sides, Arendt's unique contribution to thinking about democracy resides precisely in her argument in favor of elites. While most political theorists have sought to adapt democratic ideas to the requirements of mass politics, Arendt insisted on the deep tension between mass behavior and meaningful citizenship. For her, this meant that citizenship must be bounded, insulated from the masses, and made the province of elites in order for it meaningfully to embody democratic ideals. First, I shall make the case for Arendt as a kind of democrat through exegesis of her writing. I shall bolster this case by reconstructing the historical context within which Arendt thinks about elites, a context ignored in most of the critical literature. Finally, having established the view that Arendt offers a democratic conception of politics, I shall assess the plausibility of this conception, paying particular attention to the arguments about a "parallel polis" that were developed by Czech democrats associated with Charter 77.

My argument is self-consciously reconstructive. While there are no doubt other themes in Arendt's work, her conception of grassroots democracy is a theme that bears reexamination, especially in light of

the current wave of "democratic revolution" that has overtaken us. I view her thoughts on democracy as a kind of "lost treasure" that needs to be retrieved. But retrieving this treasure does not commit me to a wholesale defense of her political theory. I do not believe that Arendt presents a sufficient account of everything democrats need to consider. But I think she does present a necessary account of a kind of democratic politics that is too often ignored.

ON REVOLUTION AND DEMOCRACY

On Revolution is a book intent on retrieving the "lost treasure" of modern revolution. According to Arendt, this treasure has been buried beneath the sands of liberalism and Marxism. Neither of these ideologies understands the distinction between liberation (the removal of impediments to action) and freedom (the exercise of capacities for participation in collective self-government). It is above all such participation—and the empowerment that it imparts to political agents—that Arendt wishes to rescue from a world overcome by a preoccupation with mass consumption and the ritualized spectacles of mass politics.

Arendt's lost treasure is less a tradition than an antitradition, a loose assortment of political visions, experiments, and institutions that includes the writings of Jefferson, Proudhon, and Bakunin, the innovations of the American and French Revolutions and the workers's councils that briefly flourished in Russia, Germany, and Hungary in the twentieth century. What unites these phenomena is a revolt against the principle of concentrated state power and against the party institutions that seek to organize such power. They resist oppression and prize the spirit of political freedom. As such, they might seem supremely democratic. Yet Arendt's description of revolutionary politics strikes a paradoxical chord.

olutionary politics strikes a paradoxical chord. First, it is described as a "regeneration of democracy." Whereas representative institutions produce a professional political class, the councils institutionalized "the average citizen's capacity to act and to form his own opinion." Tragically, these political forms did not—perhaps could not—last. This is tragic because, as Arendt insists, "whenever knowing and doing part company freedom is lost." The councils incarnated a vigorous, participatory, and egalitarian politics. They represented "a new form of government that would permit every member of the modern egalitarian society to become a 'participator' in public affairs" (1977a, 262–64).

Second, Arendt maintains that such councils did not and could not accord with the democratic spirit of the egalitarian age. They are contrary to the typical institutions of modern democracy (political parties, competitive elections, representative government), because of the oligarchical character of such institutions. They also reject the essential principal of modern democratic politics—the principle of mass politics itself, with its watery, fictive equality and its ten-

dency to homogenize public affairs, reducing citizenship to infrequent forms of vicarious participation and public discourse to a kind of public relations game. In one of the most famous—and notorious passages in the book, Arendt describes revolutionary forms as "islands in a sea or oases in a desert." Freedom, she avers, is possible only among equals, but equality itself is not a universally valid principle and requires limits in order to operate. For this reason, Arendt supports the idea that politics be the province of elites; for "the political way of life has never been and will never be the way of life of the many" (1977a, 275). If the spirit of freedom is to be preserved, such elites must strive "to protect the island of freedom they have come to inhabit against the surrounding sea" of necessity and banality.

Arendt's conclusion has chilled the hearts of many a democrat: "To be sure, such an 'aristocratic' form of government would spell the end of general suffrage as we understand it today; for only those who as voluntary members of an 'elementary republic' have demonstrated that they care for more than their private happiness and are concerned about the state of the world would have the right to be heard in the conduct of the business of the republic" (pp. 277–79). Arendt understands that her proposal is highly unconventional, but she allows that "it is not the revolutionary spirit but the democratic mentality of an egalitarian society that tends to deny the obvious inability and conspicuous lack of interest of large parts of the population in political matters as such' (p. 277). Only someone misled by the prejudices of a democratic mentality, she tells us, could fail to see the merit of her argument. This sounds pretty antidemocratic, indeed. Yet Arendt has earlier described her elementary republics as the regeneration of democracy. How can we explain this apparent contradic-

The answer, I would submit, is that Arendt recognizes two different, competing conceptions of democracy. Both derive from the same three distinctively modern, egalitarian principles: that matters of public concern ought to be contestable, rather than insulated from public debate; that forms of contestation be opened up to all who wish to be heard, rather than to a privileged few; and that "the average citizen's capacity to act and form his own opinion" should be recognized. She rejects the logic of mass democracy because, in spite of its inclusiveness, mass democracy is a form of oligarchical politics, in which political parties, through their monopoly upon nominations and upon public debate, serve "as very efficient instruments through which the power of the people is curtailed and controlled" (1977a, 269). In such a system, public debate is characterized by an "obvious phoniness." In spite of the rhetoric of popular will, "the voter can only consent or refuse to ratify a choice which . . . is made without him," a choice that is embellished by techniques of propaganda and political advertising "through which the relationship between representative and elector is transformed into that of a seller and a buyer" (p. 276). Arendt criticizes, then, precisely what defenders of democracy like Joseph Schumpeter and Robert Dahl have presented as the cardinal features of modern democracy.

But she criticizes this system in the name of the regeneration of a grassroots democracy. She loudly celebrates the average citizen's ability to think, deliberate, and act. All of this clearly seems democratic. Yet Arendt paradoxically links such action with a system of elites, a system premised upon the insulation of politics from the masses. How can it then be democratic?

Consider first that the model here is not the socalled democratic elitist model. Arendt's elites are not selected by the electorate in order to rule over them. Arendt's elites select themselves, represent none but themselves, and rule over none but themselves. Arendt quite clearly rejects the common association of the term elite with "an oligarchical form of government, the domination of the many by the rule of the few" (1977a, 276). Her elites are distinguished by their insulation from the many, not by their rule over them. While Arendt acknowledges that some of her examples of revolutionary elites, like the Russian soviets, did "determine the political destinies of the many," she maintains that this was secondary to their principal concern, which was their own freedom to act according to their opinions. Furthermore, such elites are self-selected and self-constituted. Arendt is insistent on this point. Her elites are not authorized or privileged by any official political bodies like states; and they do not, in principle, exclude anyone. They are open, she insists, to "those few, from all walks of life, who have a taste for public freedom and cannot be 'happy' without it" (1977a, 279). To be more precise, there is a principle of exclusion, but it is a flexible one, not discriminating against particular kinds of people, certainly not barring ordinary people. The principle is the principle of interest. Put simply, all you need to do to belong is to care about matters of common concern and to act on this concern in concert with others of similar opinion. There is a certain virtue or nobility involved in such participation, but it is not necessary to accept Canovan's disparaging claim that for Arendt, "political freedom, which is the all-important glory of human existence, is possible only among an aristocratic leisure class undisturbed by compassion for their serfs" (1978, 15). Arendt certainly does not make this argument. Her "aristocracy" is an aristocracy of civic-mindedness, not a hereditary elite based on access to wealth. Indeed, she insists that participation be open to those, from all walks of life, who have a taste for freedom. Her most vivid examples of such an elite are modern workers' councils, hardly the organizations of a leisure class (see Arendt 1977a, 255–75). The virtues exhibited by the participants in these councils (the extraordinary courage of their convictions, a willingness to put one's reputation on the line, rhetorical and pedagogic skills) certainly distinguish such citizens from those many others for whom participation exerted no appeal. But they are hardly the province of a privileged group.

Finally, Arendt does not speak of a single elite. Her language is decidedly pluralistic. She writes about voluntary participation in "an elementary republic," not the elementary republic. She clearly envisions that there will be many kinds of elementary republics and many levels of authority. This is perfectly consistent with her emphasis on plurality in *The Human Condition* (Arendt 1958). Difference, for Arendt, is an intrinsic feature of human existence. There is no reason to assume that she imagines that political virtue can be located in a single place.

Thus Arendtian elites are not conventionally understood elites at all. If in one sense they are extraordinary, in another they are quite ordinary. They are not privileged, and they are open to all who wish to participate. It is their multiplicity and openness, their self-constitution and self-government, that distinguishes them, and it is no doubt for these reasons that Arendt considers them a regeneration of democracy, a new form of government that would permit every member of the modern egalitarian society to become a participator in public affairs.

But what about the masses? Even if it can plausibly be argued that Arendt's conception of vigorous participation has democratic features for the limited and exclusive (even if self-selected) demos, what about the rest, the excluded, those too caught up in the everdayness of social life to take an interest in politics? Arendt's comment about the end of suffrage as we understand it today (she is talking here about universal suffrage) is quite vague, but it gives little succor to those democrats concerned about the condition of the masses, their rights, and the responsiveness of politics to their needs and demands. It also offers few clues about how her vision makes real the egalitarian promise of making participation available to *every* member of society. There are a number of answers here. While they are not explicitly developed by Arendt in the name of a democratic theory, it is possible to reconstruct them from her text. They can leave no doubt about her commitment to a plausible interpretation of democratic politics.

First, for Arendt, the concept of "the masses" is a sociological category. It refers to the members of a mass society, to the kind of character that is produced by a society based upon mass consumption. The member of the mass tends to be anonymous and is treated as anonymous by dominant corporate and state institutions. He or she tends to be what Robert Dahl (1961) called a "homo civicus," rather than a "homo politicus"—a privatized individual who participates only minimally in any kind of public discourse and whose infrequent acts of voting participation are based upon a narrow construction of private interest that is largely taken for granted. Sheldon Wolin (1983) links Arendt's aversion to the masses with Nietzsche. He might also have linked her with Heidegger, her teacher, for whom "Das Man" of modern society was a banal, mediocre, and pathetic creature overcome by forces beyond his control (see White 1991; Wolin 1990). There can be no doubt that she was influenced by the totalistic critique of modernity offered by these thinkers (see Habermas 1987; Megill 1985). But the critique of mass society need not have the kind of antidemocratic implications that Wolin attributes to it. Consider the words of John Dewey, who warned that democratic principles had been eclipsed by the scale of modern society and by the diversions from citizenship that modern society offers: "The confusion which has resulted from the size and ramifications of social activities has rendered men skeptical of the efficiency of political action" (1927, 134-35). Or consider C. Wright Mills, a contemporary of Arendt, who wrote about the "cheerful robots" of mass society—ordinary individuals caught up in disempowering, bureaucratic forces largely beyond their control. Such individuals become "adapted men." They gear their aspirations and their energies to the situation they are in; they become the consumers of products and images that the institutions of their society require them to be (Mills 1959, 170-73). These writers were not antidemocrats; they were democratic social critics, who sought to highlight the failure of modern societies to live up to democratic ideals. Arendt partakes of a similarly anti-authoritarian sensibility. For her, what distinguishes the masses is their tendency toward conformity, a tendency engendered by the institutions of mass media, mass politics, and corporate mass production and consumption. Arendt believes that in the face of these institutions, it is futile to imagine that significant forms of popular empowerment and political participation are possible. Furthermore, to the extent that they are possible, they will be channeled not through these institutions but in response to them, against the conditions of impersonality and sameness that they promote. In this sense, Arendt's elites are counterposed to the masses. But the counterposition is not between a privileged few and an incapable many. It is not between two classes or types of people so much as between two competing attitudes. Those who revolt against the conformity of modern society constitutes themselves, through their action, as citizens of an elementary republic. In so doing, they exhibit a kind of virtue typically in short supply. But this is not a derogation of average people. It is an invitation, perhaps even an incitement, for them to surpass their ordinariness simply through their voluntary association and concerted action.

But again, what about the masses? What about those who, for whatever reasons, of which choice may simply be one, do not become politicized, do not associate, with others, into elementary republics? Once we see that "the masses" is not a psychological category intended to denigrate the capacities of ordinary men and women and that for Arendt, there is not one elite but many elites, it becomes clear that who is a member of an elite and who is a member of the mass is not a question that can be answered once and for all. At different times and for different reasons, some people will become politicized in the face of the disengagement of most others. But there is no reason to imagine that who these people are remains the same. The citizen is not, then, a higher type of

person but an otherwise ordinary person who chooses, under specific circumstances, to throw caution to the wind, to disrupt the routines of mass society by acting against the grain of social routine. This helps to explain how it is possible for Arendt to consider modern labor movements—made up of ordinary proletarians distinguished only by their passionate commitment to justice—as exemplars of her political spirit, their activities constituting "one of the most glorious and probably the most promising chapter of recent history" (1958, 159).

Furthermore, those who at any time constitute the mass are not thereby subjected by Arendt to any particular exclusion or degradation beyond those exclusions and degradations that characterize modern society itself. They remain just as they were before-large groups of anonymous individuals, incorporated into the same remote forms of mass politics, subjected to the same social and economic forces, assuming their assigned places in the social division of labor in exchange for the material benefits that accrue to those places. Arendt is not condemning this way of life, though it is one that she clearly finds wanting. But she rejects the idea that it can meaningfully be considered a life of political freedom. And she also rejects the idea that it is possible to transcend such a society in the name of a "true democracy" based upon meaningful forms of mass political participation. For her, the ideas of mass and participation can have only the most metaphorical connection. She thus insists that only through more limited forms of participation is effective participation and empowerment possible.9 The argument about insulating politics from the masses, then, is not antidemocratic, though it is unconventional. The point is not to exclude a class of people but to establish boundaries that keep out the impersonality and routine characteristic of mass society. 10 Such boundaries insulate the oases of civic engagement from the desert of mass politics. But they are no substitute for mass politics.

It is tempting to believe that Arendt's rejection of "general suffrage as we understand it today" requires a wholesale abandonment of representative democracy. But this is also mistaken. It is true that some of her examples of elementary republics, like the Russian revolutionary soviets, did claim to be alternatives to parliamentary institutions. But Arendt presents these as bearers of a certain political spirit rather than as literal examples to be emulated. 111 Nowhere does she advocate the revolutionary transformation of postwar liberal democracy. Indeed, the weight of her corpus leans heavily against such transformations. It is only with great circumspection, she held, that we can undertake to construct political institutions, and even then we cannot proceed ex nihilo. The problem with modern ideologues is precisely their unbridled confidence in themselves and in their projects of wholesale transformation: "Firmly anchored in the tradition of the nation-state, they conceived of revolution as a means to seize state power, and they identified power with the monopoly of the means of violence" (1977a, 256). Arendt's model of elites is not

an alternative model of state power. Rather than envisioning a total structure of power that might replace the institutions of representative government, Arendt envisions the pluralization of political space. Instead of a single project of mass empowerment, as envisioned in different ways by Marxism and populism, she endorses the multiplication of forms of popular participation. One way of accomplishing this is through a decentralization of political authority; alternative institutions, like the Jeffersonian wards, the council system, and confederal models of authority, play an important role in Arendt's thought (Sitton 1987). But Arendt never puts forth these models as wholesale alternatives. It is likely that she considered the normal institutions of liberal democratic politics too entrenched to be undermined and replaced. And it seems certain that she recoiled from the prospect of such a total transformation in any case.

An overlooked passage in On Revolution—part of the same paragraph as the notorious comment about suffrage—serves to clarify. Commenting on elementary republics, Arendt notes, "it would be tempting to spin out further the potentialities of the councils, but it certainly is wiser to say with Jefferson, 'Begin them only for a single purpose; they will soon show for what others they are the best instruments'—the best instruments, for example, for breaking up the modern mass society, with its dangerous tendency toward the formation of pseudo-political mass movements, or rather, the best, most natural way for interspersing it at the grass roots with an 'elite' that is chosen by no one but constitutes itself" (1977a, 279). Arendt refuses to say too much about the councils. Instead of identifying them with a grandiose program of transformation, she identifies them with nothing more than the impulse toward empowerment itself. Yet while she refuses to turn the councils into a counterideology, she does indicate their political significance. They are instruments not for replacing but for breaking up mass society, countering its homogenizing tendencies, interspersing modern society at the grass roots with forms of voluntary association. I think it is clear in these comments that Arendt sees her elites not as alternatives to the formal institutions of representative government but as complements to them, reproaches to their tendency to treat people as subjects rather than citizens, as consumers rather than makers of their own destinies. As such, her revolutionary elites represent an invigoration of democracy, which takes the democratic idea of citizenship seriously but sees it being most appropriately applied in the domain of civil society rather than directly in the sphere of the state itself.

Margaret Canovan is thus mistaken when she claims that Arendt failed to notice the difference between normal and extraordinary politics and wrongly desired "the replacement of parliamentary government by this council system" (1978, 18). In advocating the end of universal suffrage as we mean it today, Arendt was not urging a restricted franchise or advocating the abolition of parliamentary institu-

tions. She was arguing that we ought to cease treating these institutions as the essence of politics and the apotheosis of democracy and that we should deprive them of their sovereign status. She was advocating an opening up of political space, rejecting the presumptions that only a professional class can act politically and that citizenship is an identity more or less defined by the occasional act of designating those who claim to act on your behalf. In Arendt's democracy, it is reasonable to infer, there would still be elections and representative institutions. But there would also be alternative forms of activity, debate, and participation and alternative forms of membership and citizenship more vital and empowering than the metaphorical equality of membership in the nation-state. The extra-ordinary-that which stands outside the norm-would constitute a form of empowerment in its own right. But it would also stand as a reproach to the political system as a whole, challenging the boundaries of acceptable discourse, keeping political parties honest and accountable, and nurturing a healthy suspicion of constituted authority and a healthy respect for democratic values.

PARTIES, ELITES, AND DEMOCRATIC POLITICS

Canovan considers it "baffling" that Arendt "should have thought such a utopian system obviously preferable to the system of representative democracy existing, for instance, in America" (1978, 19). Kateb makes a similar point, arguing that Arendt failed to appreciate the virtues of representative government, which preserves accountability of officials and (by institutionalizing partisan competition) generates public debate (1983a, 56-58). While Arendt did not offer a system in place of representative democracy, she was a severe critic of it. This is perfectly understandable when we place her criticisms in their appropriate historical context. Arendt was a German Jew who was forced to flee Hitler. She was the product of the failure of Weimar democracy. The experience of her generation was an experience of failed parliamentarism. Disillusioned socialists like Robert Michels (1962), critical theorists like Otto Kirchheimer (1966) and Franz Neumann (1957), and neofascists like Carl Schmitt (1988) concurred that the institutions of mass democracy had failed effectively to mediate between the state and the citizenry, that they had become corrupt vehicles of entrenched corporate interests, and that the public debate that had been generated by parliamentary institutions during the early modern period had given way to the engineering of mass opinion by increasingly remote political elites. 12 This view was particularly influential among those who had experienced the breakdown of parliamentary democracy, but it was not limited to them. And it was not only in Europe that it was articulated. It had an influential American supporter in John Dewey (1927), whose The Public and Îts Problems, written even before the downfall of Weimar, developed similar themes. Democracy, Dewey argued, was possible only with a flourishing of independent publics, groups of citizens bound together by an articulate, common concern. But in the twentieth century such democratic publics had been eclipsed by the growth of a mass society. In such a system public debate had become corrupted and partisan competition ritualized; meaningful forms of civic politics had been rendered increasingly difficult to initiate, much less sustain (Westbrook 1991).

This critical perspective was current among American public intellectuals in the postwar period. For this group, fears of a closure of parliamentary democracy were exacerbated by the emergence of a national security bureaucracy and the spirit of McCarthyism. Perhaps the most notable critic of the American system during this period was C. Wright Mills (1956), whose The Power Elite built upon Deweyan themes in criticizing the growth of large-scale, bureaucratic corporate and state institutions. Arendt's political theory was developed in this intellectual milieu. As someone who had experienced the absence of constitutional government in Germany, Arendt was well aware of its virtues. Her criticism was not mindless. Consider this text, written in 1953 in response to the McCarthyite witch-hunts:

America, this republic, the democracy in which we are, is a living thing which cannot be contemplated or categorized, like the image of a thing I can make. . . . It is not and will never be perfect because the standard of perfection does not apply here. Dissent belongs to this living matter as much as consent does. The limitations on dissent are the Constitution and the Bill of Rights and no one else. If you try to "make America more American" or a model of democracy according to any preconceived idea, you can only destroy it. Your methods, finally, are the justified methods of the police, and only of the police (1953, 599).

Note the conjunction of themes: a staunch commitment to civil liberties, praise for the Constitution as a framework of such liberties, and a vision of democracy as an inherently open-ended process, not to be identified with any formal political arrangements or any end-state. Arendt's critique of McCarthyism points beyond normal constitutional politics in its emphasis on experimentation and dissent, but it also recognizes the value of such a normal politics. 13 Arendt never dismissed the importance of the constitutional "fences" established by liberal democratic regimes. 14 Yet events had instructed her that it was not in the parliamentary arena that democracy could most effectively be defended or deepened. The experience of parliamentarism in Europe and the closure of American domestic politics during the Cold War led her to conclude that it was through vigorous resistance and dissent, including forms of civil disobedience, that liberal democratic values could be preserved at the same time that more participatory forms of democracy could be experienced. Arendt's idea of "oases in the desert" was thus very much informed by the crises of representative government in our time and by the need to go beyond normal forms of mass politics even to defend constitutional liberties, much less to develop more effective forms of empowerment (see Arendt 1972).

A number of critics (Jay 1978; McKenna 1984) have likened Arendt's conception of extraparliamentary civic action to neofascist views prevalent in late Weimar Germany. Arendt, it is argued, like the so-called "political existentialists" (among whom Carl Schmitt was the most notable), rejected the liberal parliamentary state and sought more direct, unmediated forms of popular action. In the name of authenticity, she thus repudiated modern democracy. These critics are right to note the presence of existential themes in Arendt's work and also to establish the antiparliamentary context in which she wrote. But they are gravely mistaken to conclude that she can thus be linked with the antidemocratic sentiments of such thinkers.

It is worth underscoring again that for Arendt, what distinguishes such elites is not simply that they affirm a sense of personal identity and worth but that they embody democratic ideals. They make real the possibility of participation of every member of society (though not at the same time) in public affairs. Such elites are clearly democratic in their own constitution. They are also democratic in a broader sense, as forms of popular empowerment that seek to contest existing forms of authority. The withdrawing of consent—the refusal any longer to submit to authoritative forms of power-is for Arendt a principal feature of a "revolutionary" politics (1972, 82-102). Such a politics claims power for the powerless, but it does not seek to rule over anyone. Any politics that seeks to vilify, exclude, or rule over others or to limit the scope of public debate would seem plainly inconsistent with the spirit of Arendt's argument. While Arendtian citizenship seeks authenticity, it also recognizes the partiality of all political movements and the plurality of the world. 15 This chastened sense of empowerment is the direct antithesis of the romantic appeal to national homogeneity and Volk that characterized fascist ideology.

Fascism, after all, was antiparliamentary and fundamentally antidemocratic. It was, above all else, a form of mass mobilization intent on eliminating cultural and political diversity. Schmitt, for example, invoking Rousseau, writes chillingly about a homogeneous "people's will" that is mobilized by, and organically linked to, a Caesaristic dictator. In contrast to parliamentary democracy, he insists, such an "authentic democracy" involves the immediate rule of the people, conceived as an organic unity, a people seeking "elimination or eradication of heterogeneity" (1988, 15-17, 9). Arendt's notion of citizenship is radically at odds with any such conception. Indeed, given her own experience as an "eliminated" Jew, whose closest friends had been eradicated, it could not have been otherwise. In On Revolution, she explicitly rejects the conception of popular sovereignty traceable to Rousseau, which connotes "a mass that moves as one body and acts as though possessed by

one will." In its stead, she recommends the view of the early Americans: "The word people retained for them the meaning of manyness, of the endless variety of a multitude whose majesty resided in its very plurality." Instead of seeking unanimity, "they knew that the public realm of the republic was constituted by an exchange of opinion between equals, . . . a multitude of voices and interests" (1977a, 92–93, emphasis mine). Arendt's critique of parliamentarism, then, is eminently democratic. The authentic citizens are those who acknowledge the limitations of even their most justifiable claims—acknowledge, in other words, the rights of others to press their claims as well. ¹⁶

Her endorsement of small dedicated groupselites-is no less democratic. While the idea of democratic elites may seem oxymoronic, it is important to see that Arendt was not alone in thinking in these terms. Indeed, she was connected with a group of intellectuals, distinguished by their participation in the resistance to totalitarianism, whose democratic credentials can hardly be questioned (Summer 1992; Wilkinson 1981). These figures were radicals whose revolt against Leninism and, indeed, all forms of vanguardism led them to reconsider the possibility of meaningful forms of mass politics. Consider Dwight Macdonald, editor of politics [sic] magazine and Arendt's close friend, whose influential 1946 essay "The Root Is Man" offered a withering critique of mass parties for having failed as vehicles of demo-cratic empowerment. "The only way, at present, of ... acting," he writes, "seems to be through symbolic individual actions, based on one's personal insistence on his own values, and through the creation of small fraternal groups which will support such actions, keep alive a sense of our ultimate goals, and both act as a leavening in the dough of mass society and attract more and more of the alienated and frustrated members of that society" (1946, 213). Andrea Cafi, the Russian anarchist who was an important influence on the *politics* circle, concurred: "Today the multiplication of groups of friends, sharing the same anxieties and united by respect for the same values, would have much more importance than a huge propaganda machine. Such groups would not need any compulsory rule. They would not rely on [mass] collective action, but rather on personal initiative and effective solidarity, such as can be developed only by friends who know each other well" (quoted in Sumner 1992, 240). C. Wright Mills, another politics associate, placed a similar emphasis on the importance of local forms of dissent and resistance (1959, 177–94). Perhaps the most influential articulation of this theme was Albert Camus's (1947) novel, The Plague, an allegory of the resistance to Nazism. The protagonist, Dr. Rieux, a leader of the resistance, refuses any macropolitical program, preferring to work tirelessly with his small band of dedicated colleagues. "Salvation," he declares, "is just too big a word for me. I don't aim so high. I'm concerned with man's health; for me, his health comes first" (p. 177).

Arendt was closely acquainted with this circle and their ideas. It is not by mistake, then, that she closes On Revolution by memorializing Rene Char, the French poet who had participated actively in the Resistance. This is one of a number of places where she quotes his aphorism, "Our heritage was left to us by no testament," expanding upon the feeling of empowerment and authenticity that was experienced by Char during the Resistance (Arendt 1977a, 280). Arendt elsewhere writes of the resisters that "without premonition and probably against their conscious inclinations, they had come to constitute willy-nilly a public realm where-without the paraphernalia of officialdom and hidden from the eyes of friend and foe alike-all relevant business in the affairs of the country was transacted in deed and word." Her point is that the resisters had gone beyond resisting oppression, in the way, for example, that Allied soldiers had resisted Nazism: they had constituted themselves as political agents. Drawn into the struggle against the Nazi Occupation, they "had become challengers, they had taken the initiative upon themselves, and therefore, without even noticing it, had begun to create the public space between themselves where freedom could appear" (Arendt 1977b, 3-4). They had become citizens of an elementary republic and experienced the joy of inserting themselves, through their voluntary action, into the public realm.

There is no question that the experience of totalitarianism and the resistance to it had an enormous impact upon Arendt's political thought. Her ideas about revolutionary elites were shaped by her reading of the tragic history of twentieth-century politics, but also by the ideas about the limits of mass politics and the importance of more local forms of resistance to oppression that were prevalent in her milieu. This milieu sheds a new light on Arendt's elitism. In such light, Arendt's argument need not appear either baffling or antidemocratic. Instead, it can be seen as an argument about the importance of new forms of empowerment at a time when the more conventional forms of representative democracy seemed in eclipse.

ARENDTIAN DEMOCRACY ASSESSED

Proving that Arendt's conception of revolutionary elites represents a coherent vision of democratic empowerment does not prove the merits of such a vision. Is it plausible? Is it practical? It is impossible to ignore these questions. The most commonly adduced charge against Arendt here is the charge of utopianism. Arendt, it is said, presents a vision that is contrary to the spirit of modern society. The kinds of spontaneous praxis she endorses are evanescent, insubstantial, pathetic glimmerings of long-past ancient glory.¹⁷

This charge of utopianism is, I think, unfair. It suggests that Arendt's vision exists *nowhere*, that it occupies no identifiable political terrain. But this is plainly untrue. Her argument was a self-conscious effort to capture the reality of a kind of insurgency

and empowerment whose significance had been lost to mainstream political thought. Arendt's works furnish many examples of "elementary republics": the French and Danish resistance to the Nazis, the Hungarian workers' councils set up in 1956, the Israeli kibbutzim, and, in America, the Civil Rights movement, Students for a Democratic Society, and the whole antiwar movement. Each of these was a consequential form of resistance to unjust authority. Each sought to open up, to democratize, the space for civic activity. Does Arendt romanticize these episodes? Perhaps she does. Clearly, she fails sufficiently to explore the limits of these forms of resistance or to explain their connection to more normal forms of democratic politics. But before we move too quickly to this criticism, we might consider her vision in the light of the dissident movements for democracy that struggled against communism in Europe.

Among the most instructive of these movements was the Czech opposition rooted in the Charter 77 movement. Charter 77 was started in 1977 as an ad hoc community of individuals who sought to protest the arrest of Plastic People of the Universe, an avantgarde rock band. Charter 77 was formed around the drafting of a declaration of protest that appealed to the principles of legality affirmed by the Helsinki Accords, to which the Czech regime had been a signatory. But it soon became the nucleus of a number of independent initiatives aimed at the democratization of Czech society (Ash 1990; Goldfarb 1989, 1992; Havel 1990; Skilling 1981). In the course of its evolution, the members of the Czech democratic opposition converged upon a conception of politics with remarkable affinities to Arendt's view.

Among the many parallels to Arendt, three stand out. First, the Czech democrats viewed politics in nonstrategic (though not antistrategic) terms. While they always sought particular objectives (indeed, in their revulsion against grandiose ideologies, they turned particularity into a virtue) they had little aspiration directly to influence public policy. For them, politics was primarily a way of being and acting so as palpably to experience one's power and affirm one's dignity. As Ivan Jirous wrote of the "parallel polis": "It does not compete for power. Its aim is not to replace the powers that be with power of another kind, but rather under this power—or beside it—to create a structure that respects other laws and in which the voice of the ruling power is heard only as an insignificant echo from a world that is organized in an entirely different way" (quoted in Benda et al. 1988, 227). It is, of course, important to remember that the "entirely different way" to which he refers is the deadening and repressive mode of decaying totalitarianism. But his point has, perhaps, a broader relevance. In the face of a political system whose power seems secure and beyond radical transformation, he insists on the necessity of creating independent poleis—elementary republics—beneath and beside it. Such republics do not principally direct themselves toward the state or the formal political system. The activities they sustain are their own goals: "In them, the intrinsic tendency of people to create things of value is realized. By giving meaning to their lives and the lives of those close to them, people are able to resist the futility that threatens to swallow them up" (ibid. 228-29). Ladislav Hejdanek sounded a similar theme: "Such a [democratic] regeneration is possible only in the form of free initiatives undertaken by individuals and small groups who are willing to sacrifice something in the interest of higher aims and values. . . . The beginning of all independence is taking our lives seriously, deciding for something that is worth taking responsibility for, being prepared to devote our energy, our work, and our lives to something of value, or, more appropriately, to someone rather than something" (ibid., 242-43). The Arendtian resonances—creating value, resisting futility, regenerating democracy—are striking.

Second and equally remarkable is the metaphor with which the Chartists described their community. They called it a "small island in a sea of apathy," the "visible tips of the iceberg" of discontent (Benda et al., 1988, 232). For the Chartists, the parallel poleis' insularity was among their prime virtues. Such insularity afforded protection from a repressive state but also established protective walls around activities otherwise threatened with being "swallowed up" by the conformity and consumerism of modern industrial society. As Havel wrote: "It seems to me that all of us, East and West, face one fundamental task from which all else should follow. That task is one of resisting vigilantly, thoughtfully, and attentively, but at the same time with total dedication, at every step and everywhere, the irrational momentum of anonymous, impersonal and inhuman power—the power of ideologies, systems, apparat, bureaucracy, artificial languages, and political slogans" (1992a, 267). 18 The islands of civic engagement and solidarity improvised by the Czech democrats represented, for them, the most effective way to practice such a resistance. The courage and conviction exhibited by the citizens of such islands distinguished them from the mass surrounding them, threatening to engulf them. The proximity of membership and of objective bound them together in ways that mass organizations could never hope to accomplish. These citizens could see and hear their fellows. They could directly experience the results of their action. They could personally be affirmed by their citizenship. Who were these citizens? Artists, writers, historians—persecuted, under-employed, insecure to be sure—but also shopkeepers, housewives, students, even factory laborers. They came from all walks of life, and what distinguished them was their commitment to principle, not their origins or their social status. 19

Finally, Charter 77 and its adjuncts were not elites in the conventional sense. They were elites only in the sense of people bound together by a common refusal to be swallowed up by the conformity that surrounded them. Their members lived in truth where most lived a lie. Where most lived as subjects, accepting the disempowering structure of society, performing rituals of obedience in spite of their

misgivings, the Chartists lived as citizens with the courage of their convictions. Yet the Chartists refused to consider themselves a higher type of person, just as they refused to consider more ordinary, conformist individuals to be inherently corrupt. As Vaclay Havel noted in his famous essay, "The Power of the Powerless," it is impossible categorically to distinguish between the conforming member of society and the true, independent citizen because the line separating the two "runs de facto through each person" (1987, 53). Everyone is in some respects complicit in the ongoing structures of mass society, and no one is so utterly entrapped within them that he or she is incapable of some kind of independence on some occasion. The boundaries separating the islands and the seas are thus ever shifting. At the same time, the connections between islands and seas varies. As Havel maintains: "It is probably not true to say that there is a small enclave of 'completely independent' people here in an ocean of 'completely dependent' people with no interaction between them. There is an enclave of 'relatively independent' ones who persistently, gradually, and inconspicuously enrich their 'relatively dependent' surrounding through the spiritually liberating and morally challenging meaning of their own independence" (quoted in Benda et al. 1988, 237). In this way, the elementary republics are not wholly self-absorbed in spite of their insularity. They point beyond themselves, having a "radiating effect" on their environment, an effect caused by the force of their example, by the embarrassment felt by those who failed to act, by the indirect moral pressure exerted on the regime. At the same time, they discover the appropriate locus of political responsibility-the civic initiative of concrete human beings acting on their own behalf, thinking and speaking for themselves (Havel 1987, 103-4).

There can be no denying that this conception of democratic praxis has proved itself to be enormously powerful, having inspired the most potent democratic movements of our time. But it might still be asked whether it has any relevance in a posttotalitarian world, where the problems of democracy no longer center around how to resist totalitarian rule but, instead, on the problems of orderly government.

The answer, I think, is *yes*. In a posttotalitarian world, resistance movements no longer have such an obvious antagonist. But this hardly means that the practice of resistance to unjust authority has become a thing of the past. ²⁰ The problem of justice can never finally be solved; believing that it could be so solved was one of the most grotesque errors of revolutionary Marxism. And as long as there is the problem of justice, there will be political argument and contestation. But is it not precisely the virtue of liberal democracy that it institutionalizes such argument and contestation (see Kateb 1983a, 43–59)? What can be the relevance of Arendt's vision of elementary republics to a posttotalitarian world in which liberal democracy has triumphed? The answer here must be twofold.

First, liberal democracy has *not* triumphed. It is in some sort of crisis in every posttotalitarian society. Is

Austria the future of Central Europe, or is Bosnia–Herzegovina? The owl of Minerva cannot furnish an answer. I would submit that it is precisely because liberal democracy has not triumphed that it is important to sustain the kinds of independent initiatives pioneered by the former anticommunist opposition, initiatives prefigured in the writings of Arendt. As Arendt saw, in order to achieve liberal democracy, liberal democratic means (parties, constitutional machineries) are not enough. Equally important are the democratic habits of mind that can only be sustained in civil society, in initiatives (publications, civic associations, social movements, forms of disobedience) undertaken at the grass roots (Isaac 1993; Kaldor 1991).

Moreover, just as it would be mistaken to view liberal democracy as the future of excommunism, it would be equally mistaken to view it as the riddle of history solved, the terminus at which we in the so-called West have fortunately arrived. Actually existing (as opposed to possibly emerging) liberal democracy has profound problems of its own, problems identified by Arendt in her critique. It seems unable to sustain high levels of political participation beyond—and even including—voting. It establishes a distance between citizens and their state that generates serious problems of legitimacy. Furthermore, the kinds of bargaining encouraged by liberal democratic political culture tend to produce a government colonized by special interests, impeding effective public policy and further alienating citizens. All of these defects in the functioning of liberal democracy are fairly well known and have been commonplace among political scientists for decades (Dahl 1989; Lowi 1969; Schattschneider 1960). But as important as they are, none of them speak to the normative failings of liberal democracy.

It is fairly well known that in the twentieth century liberal political theorists inspired by Joseph Schumpeter attempted, with some success, to redefine the meaning of democracy in a way consistent with the factual realities of liberal democratic politics (Dahl 1956; Schumpeter 1947). These theorists were certainly correct in arguing that liberal democratic government, based upon competitive elections, political bargaining, and a high degree of political passivity on the part of ordinary citizens, was the most realistic macropolitical system of government even remotely consistent with the idea of democracy (see Ackerman 1992; Havel 1992b; Kiss 1992). The existing macropolitical alternatives (fascism, communism, and various combinations of the two) had clearly proved themselves disasters. In the name of authentic people's democracy and unmediated popular rule, they produced great misery and injustice.²¹ But the Schumpeterian redefinition of the term left a good deal—too much—of its meaning behind (Skinner, 1973). In the name of realism, the idea of rule by the people became wholly metaphorical, meaning nothing more than the right of citizens occasionally to select candidates who appeared to offer them some of what they wanted from government. As Arendt put it, the relationship between politician and citizen was transformed into that of a seller and a buyer, under conditions of a seller's market. This is clearly wanting from the perspective of the democratic ideal of selfgovernment. And simply redefining the term will hardly convince those whose take this ideal seriously. Yet the great difficulty of much so-called participatory democratic and communitarian criticism of liberal democracy has been its imprecise character (see Pateman 1970). The criticisms always seem to imply either a reform of the system that leaves its bureaucratic and mass character intact (as in variants of social democracy) or an alternative system of sovereignty that is left unspecified. In neither case is it clear just exactly how the alternatives represent an advance in terms of democratic values.

The Arendtian conception of elementary republics helps us to refocus this debate. The liberals and the social democrats are right: a wholesale democratic alternative to liberal democracy is inconceivable. At the level of the nation-state, the "rule of the people" must always be more or less metaphorical-channeled, diluted, and corrupted by mass political organizations and bureaucratic structures. But not only are partial alternatives to such a politics (elementary republics, parallel poleis) conceivable, but such oases are a part of our political landscape, though an often-ignored part. What do they look like? Among their more dramatic forms are the Green movements in Europe; the civic initiatives, like the Helsinki Citizens' Assembly, that continue to operate in Central Europe; feminist groups like National Abortion Rights Action League and the many local chapters of Planned Parenthood; and environmental activist groups like Greenpeace. But the less dramatic forms are equally significant. I am thinking here of the many battered women's and rape-crisis shelters, the many local environmental groups and coalitions seeking to clean up their community or to resist the construction of toxic incinerators or hazardous waste dumps, local parent-teacher associations, the religious and social action committees of synagogues and churches, community development organizations, and so on (see Barber 1985; Boyte 1980, 1990).²²

These more participatory forms of democratic citizenship are not alternatives to mass democratic citizenship. They are complements to it. 23 As Arendt put it, they break up modern mass society, interspersing its large-scale organizations and imperatives with more grassroots activities and with concerns that cut against the grain of ordinary politics centered around the sovereign nation-state. As such, they incarnate the spirit of association that might serve to invigorate the normal institutions of the liberal democratic state. But, it must be repeated, they are not and cannot be complete alternatives to these normal institutions. It is scarcely conceivable that members of modern society can experience any civil or political freedom worthy of the name in the absence of a constitutional system based upon the rule of law (see Walzer 1991). Furthermore, a system of competitive political parties to coordinate social interests on a large scale freely is an indispensable condition of any meaningful democratic citizenship. Without such parties, as remote and bureaucratic as they may be, political power will inevitably concentrate in even more centralized and bureaucratic institutions with even less accountability (Kiss 1992). Finally, the imperatives of advanced industrial society make some forms of state regulation of economic and social life inevitable. Such conditions make reformist labor movements and social democratic parties equally indispensable; for without these organized political forces publicly articulating questions of regulation and justice through the liberal democratic process, such matters will surely be articulated in ways even more remote from any kind of civic oversight, much less participation (Esping-Andersen 1985; Harrington 1989).

On all of these important matters, Arendt's conception of a democratic politics centering on a plurality of elementary republics has little to say. Because these concerns have rightly preoccupied political theorists writing about democracy and because Arendt adamantly refused to address them directly, it has been common to consider her beyond the pale of democratic theory. But as democratic theorists wrestle with the legitimation problems that have long plagued liberal democracy, it is perhaps time to reopen the question of what democracy means in the

late twentieth century.

Arendt's work addresses an important dimension of this question that has until recently been ignored, for she recognized a paradox that democratic theorists could do well to ponder: the effort to irrigate the deserts of liberal democratic mass politics can only draw from the wellsprings of a robust civil society of elementary republics; and yet the effort fully to incorporate these oases can only lead to their eventual evaporation. Parliaments, courts, and bureaucracies are indispensable, but when civic associations are reduced to being the target constituencies of politicians and the supplicants of judges and bureaucrats, then they are on the road to ruin. This would deprive liberal democracy of its nourishment. It would also mean the end of the most meaningful forms of democratic participation possible under modern conditions. For this reason, Arendt insists that while the oases of civic initiative may engage and invigorate the larger system, they must always prize their own independence and insularity or else risk their own demise. While such a politics would not necessarily be averse to broader strategies of coalition and movement building, it would always be wary of the diluting—the corrupting—effects of such efforts. There is thus a tension between meaningful participation and strategic effectiveness built into such a politics. Perhaps this tension between civic initiative and mass political organization is the terminal condition of modern democratic politics.

Notes

For their helpful comments, I would like to thank Jack Bielasiak, Dana Chabot, Ray Franklin, Russell Hanson, Jeff Hart, Arthur Lothstein, Lennie Markovitz, Sol Resnick, Ian Shapiro, David Sprintzen, Greg Sumner, Frank Warren, Mike Wrecszin, Gerry Wright, and Burt Zweibach. This paper was originally presented at the Michael Harrington Center, Queens College, the C. W. Post department of Philosophy; and the Political Science Department colloquium at Indiana University.

- 1. For two fairly conventional accounts of the enterprise of democratic theory, see Pennock 1980 and Dahl 1989.
- 2. Canovan's article, because of its sophisticated treatment of ambiguities in Arendt, is particularly notable, because even she assumes that Arendt's talk of elites must be antidemocratic. See also Kateb 1983a.
 - 3. On her rejection of all labels, see Arendt 1979, 336.
- 4. On the elusive, almost mythical character of Arendt's construction of a revolutionary tradition, see Miller 1979.
- The classic statement of this theme is Michels 1962, esp. 43–51.
- 6. See also Arendt's comment that "from the revolutions of 1848 to the Hungarian revolution of 1956, the European working class, by virtue of being the only organized and hence the leading section of the people, has written one of the most glorious and probably the most promising chapter of recent history" (1958, 215).
- 7. This is why she so strenuously objected to Rousseau's conception of the general will (Arendt 1977a, 74–89). Arendt rejects the ideas that there is a single authoritative source of power and that "the people" can be seen as a homogeneous entity in which this power resides. Her clearest statements of the theme of plurality are Arendt 1958 and 1982.
 - 8. Kateb acknowledges this (1983b, 70-74).
- 9. The metaphorical character of the connection between mass politics and democratic participation has long been acknowledged by democratic theorists. Dahl, for example, strenuously distinguishes between democracy and polyarchy, claiming that the latter only approximates the values of the former. Under polyarchy, elections are the central means of citizen participation, and elections reflect the preferences of the citizens in only the roughest of senses, "insuring that political leaders will be somewhat responsive to the preferences of some ordinary citizens" (Dahl 1956, 130–32).
- 10. The same point can be made about her argument for excluding "the social" from politics, namely, that "the social" is for her a functional, rather than a structural category, denoting an unreflective way of being rather than any particular kinds of concerns. On this, see Honig 1992 and Fraser 1989. I examine inconsistencies in Arendt's treatment of the social in Isaac 1992, 158–66. Arendt's view of the social question does not sit easily with any conception of democracy. But for the purposes of this paper, which is principally concerned with Arendt's conception of elementary republics and elites, I shall bracket this issue.
- 11. Arendt's method is indicated in an essay on Walter Benjamin, in which she writes about a kind of thinking that "delves into the depths of the past . . . not in order to resuscitate it the way it was and to contribute to the renewal of extinct ages [but to help it] survive in new crystallized forms and shapes" (1968a, 205–6).
- 12. These same problems were identified even earlier by Max Weber (1978, 1381–1469). For a more recent account, see Habermas 1989.
- 13. For acknowledgements of the virtues of representative government, see Arendt 1977a, 32–33, 268–69; for "the protecting mask of a legal personality," see p. 108.
- 14. See, especially, Arendt's discussion of legality and favorable references to Montesquieu (1968b, 465–67). It is worth noting that Arendt was a consistent supporter of civil liberties, (see Young-Bruehl 1982, 273–75, 288–89).
- 15. See, especially, Arendt's essay on judgment, "Truth and Politics" (1977b, chap. 7); see also Isaac 1992, 166-76.
- 16. It is true that Arendt often failed to couch her arguments in these democratic terms, thus lending credence to those who criticized her for counterposing civic virtue and democratic rights. The most egregious instance of this counterposition was her notorious essay "Reflections on Little

Rock" (Arendt 1959), in which she criticized federal efforts to desegregate southern schools in the name of republican values. The subject of this subtle and complex essay is beyond the scope of this paper. But it is clear that at least in this instance, Arendt's conception of extraparliamentary elementary republics put her in close proximity to the "state's rights" arguments of southern segregationists, however different her reasoning was. See the discussion of the anti-democratic potential of grassroots politics with specific reference to the Ku Klux Klan and the Southern White Citizens' Councils in Evans and Boyte (1986, 52–60). I am indebted to Michael Wreszin and Frank Warren for calling attention to this problem in Arendt's writing.

17. This kind of criticism is presented in different ways by Canovan (1978) and Wolin (1983). Miller (1979) offers a more appreciative account but also suggests a kind of practical

irrelevance.

18. Havel consistently related the tyranny of communism to broader tendencies of modern industrial life and mass society. In his open letter, "Dear D. Husak," for example, he decries the fact that in Czechoslovakia, man is treated as "an obedient member of a consumer herd": "Instead of a free share in economic decision-making, free participation in political life, and free intellectual advancement, all people are actually offered is a chance freely to choose which washing machine or refrigerator they want to buy" (1992a, 60). While Havel, of course, recognized the importance of civil liberties in the West, his essays "The Power of the Powerless" and "Politics and Conscience" clearly relate this consumerism to modernity itself (see Havel 1992a).

19. "Dissident" is a label attached to "ordinary people with ordinary cares, differing from the rest only in that they say aloud what the rest cannot say or are afraid to say" (see Havel

1987, 77–80).

20. This would seem to be the argument of Francis Fuku-yama (1989).

- 21. An interesting argument that both Marxism and fascism represent authentic, antiliberal democracy can be found in Schmitt 1988.
- 22. Would Operation Rescue also qualify as a grassroots democratic movement? This is a difficult question to answer within the terms of Arendt's theory. On the one hand she does not offer any explicit principles of right or justice that would allow one to distinguish Operation Rescue from Planned Parenthood. On the other hand, she does insist on certain values—equality, participation, plurality, reflexivitythat would exclude certain kinds of political formations, like neofascist organizations or revolutionary sects. These values would also clearly exclude the most extreme antiabortion protest groups. The question is whether Operation Rescue is such an extreme group. Arendt would probably hold that Operation Rescue is an example of democratic civic initiative as long as it operates in a respectful, peaceful, self-limiting way. She clearly does not offer a theory of prepolitical rights (e.g., the right to privacy, central to liberal democratic theory) on which one could ground criticism of a group like Operation Rescue. This may constitute a serious limitation of her thought. On the other hand, current debates about whether abortion rights are natural or conventional and about whether they are appropriately secured through the courts or through political contestation suggest that Arendt's view has certain merits in underscoring the political (i.e., contestable) character of such matters.
- 23. The current literature on the importance of civil society develops similar themes and occasionally acknowledges Arendt's influence. See Arato and Cohen 1984; Habermas 1989, 1991; Keane 1988; Lefort 1986; and Touraine 1989.

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MULTICULTURALISM AND THE PROBLEM OF PARTICULARISM

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Then Kant first used the term "culture," he referred to the human capacity to will universal moral laws. Multiculturalists object to the denial of "difference" implicit in Kantian as well as all other Enlightenment forms of universalism. Their objection stems from their more particularistic understanding of culture, which for the most part everyone shares today. Plato is frequently said to be the fount of (universal) natural law theory; yet a medieval Muslim philosopher, Alfarabi, presents a Plato who denies moral universalism but acknowledges the possibility of some form of universalism, at least in the realm of knowledge. Alfarabi's Plato thereby provides a corrective for both extreme contemporary particularism and extreme Kantian universalism.

ulticulturalism encompasses a wide variety of viewpoints. Nevertheless, there is at least one thing that multiculturalists hold in common—an opposition to traditional universalism. We are familiar with the purpose of universalism as it is conveyed in the expression melting pot. The United States was to become a melting pot in which people from many different countries and religions would come together and, by adopting the "American way of life," become "Americans." Today the melting pot is viewed as a sham. Some say that immigrants to the United States have not become "Americans" but merely assimilated themselves to the dominant white European male culture. Of course, the citizens of the United States who devoted themselves to the melting-pot ideal did not hold such a disenchanted view of it. According to them, the common denominator in the United States was not the white European male but shared devotion to the principles of U.S. government, such as the principle that all men are created equal with respect to their rights to life, liberty, and the pursuit of happiness. Many multiculturalists, especially the more radical among them, have attacked even these once-cherished ideals. They detect in the principles of U.S. government a tendency to condemn other cultures for not embracing them. They are not mistaken: it is for this reason that presidents of the United States since Wilson have viewed it as part of their duty to promote democracy throughout the world. Presidents have championed these principles because they have viewed them as the best principles of government for all nations in the long run. Some multiculturalists take this as evidence that universalism is merely a means to the end of cultural hegemony.

In lieu of the cultural universalism of the melting pot, multiculturalists prefer a more particularistic view of culture. Their watchword is difference: we must all maintain our own cultures even while respecting the cultures of others. Multiculturalism's particularistic view of culture is not anomalous. As we shall see, it grows out of the contemporary meaning of culture. Although the particularistic meaning of culture has come to predominate in our

community, there are still many who cling to the universalism of the melting pot.

I shall explore the reasons for our having arrived at such a divide over our culture and its purposes. In the first part, I describe the conflict between Kantian universalism and multiculturalism because this conflict reveals how the unrealistic demands for inclusion by Kant and even some multiculturalists give rise to a particularistic reaction by other multiculturalists. I argue that cultural universalists should temper their universalism and that multiculturalists should not abandon the moral foundations of U.S. government. In the second part, which consists of two subsections. I describe a different understanding of the relation between the universal and the particular in the hope of tempering both extreme universalism and extreme particularism. This different understanding, namely, Plato's natural right teaching, is closely related to the natural law tradition, the source of our own moral universalism. The traditional Western Christian reading of Plato (shared by multiculturalists in general) is that he is the source of Western moral universalism. Consequently, multiculturalists believe that in opposing moral universalism they are opposing the whole Western traditionabove all, Plato. I believe that moral universalism has been read back into Plato in the Christian West. The nontraditional reading I will describe is found in a medieval Muslim thinker, Alfarabi.

KANT AND THE MULTICULTURALISTS

Multiculturalists and their critics frequently describe multiculturalism as the "politics of difference" (Taylor 1992, 38; West 1990, 19; Young 1990, 300). Charles Taylor has traced this politics of difference to an earlier Kantian conception of the "politics of universal dignity." Kant argues that we deserve respect as human beings because of "our status as rational agents, capable of directing our lives through principles" (Taylor 1992, 39, 41). These principles are, of course, Kantian universal moral laws. Kant considers the possession of such universal principles (as opposed to mere particularistic manners) to be the mark

of true "culture" ([1983] 1988, 128). In other words, for Kant culture is at least potentially universal. In contrast, multiculturalists, like all of us in contemporary society, speak not of the one true culture but of cultures. Our view of culture has been shaped by nineteenth-century German historicism.1 To a great extent, this historicism developed a particularistic view of culture in reaction to Kantian universalism. The historicists were reacting against what they viewed as rootless cosmopolitianism. Multiculturalists share with earlier historicists the view that it is impossible to send down roots in universal soil. Universalism undermines our rootedness, rather than serving as its ground. Furthermore, multiculturalists believe this universalism imposes a false form of homogeneity on the world's multiplicity of cultures or moralities. Kant accords respect to human beings who have rational agency in common; most multiculturalists claim to accord respect to all human beings in all their diversity. By making (cultural) difference the basis for respect, these multiculturalists hope to succeed in being all-inclusive where Kant failed. Multiculturalists are justified in asserting that Kant's universal moral laws exclude not only individuals but cultures. I will show, however, that their politics of difference excludes as well. Difference or heterogeneity can run only so deep in political life: Politics always demands a certain amount of homogeneity. And it cannot help but justify such exclusive (particularistic) homogeneity with appeals to something universal. Where Kant went wrong was in treating the universal as if it could become concrete, rather than as if it were something to be appealed to. Although multiculturalism was correct in reacting against this attempt to make the universal concrete, it succumbs inadvertently to the same temptation. Finally, multiculturalists are tempted to attribute Kantian follies to the whole Western tradition. I hope to show that Alfarabi's Plato has a more accurate understanding than either Kant or the multiculturalists of the appeals we make to universal principles and of the limits of inclusion in politics.

Of course, multiculturalism or the politics of difference is itself internally diverse. To understand fully the dispute between multiculturalism and Kant, we must be cognizant of this variety. Multiculturalists can be divided into three types: egalitarian antiessentialists, their fellow travelers, and separatist essentialists. Egalitarian antiessentialists believe that cultures do not have essences. Because cultures are constructed by their adherents, they may be deconstructed. By means of deconstruction, rather than universal principles of action, one can achieve a truly inclusive society. Fellow travelers of the egalitarian antiessentialists acknowledge that difference may in the long run be capable of deconstruction, but they deny that every community in the United States has achieved sufficient cultural identity to have its identity deconstructed. Separatist essentialists view each culture as possessing an essence. They object to the Western claim to have access to universal principles of action.

They hint, however, that their own culture may be better, if not more universal, than Western culture.²

The leading theoretical sources of the first two types of multiculturalism are the French postmodernists, Derrida and Foucault. Taylor characterizes this body of multicultural theory as "neo-Nietzschean" and is eager to dismiss its subjectivism "shot through with confusion" (1992, 69–70). Derrida (1973) also makes ample use of structuralism, Heidegger, and Freud. Both Derrida and Foucault owe much of their egalitarianism, however, to the influence of Marx on the French Left. Their multicultural followers probably owe their egalitarianism as much to the egalitarianism of the principles of U.S. government as to neo-Marxist theory. Derrida, Foucault, and these multiculturalists are all unabashedly eclectic.

Egalitarian antiessentialists have a closer affinity with Derrida than with Foucault. They are not as eager to identify power and knowledge as Foucault (1984, 57, 72–73), probably because they do not find themselves so excluded from power as the other multiculturalists. Unlike Nietzsche, who originally described philosophy as a form of the will to power (1966, 16), multiculturalists identify power and knowledge in order to serve notice on the powerful that the things they pass off as knowledge will no longer be accepted as such. Rather, they will be contested. In other words, however Nietzschean these multiculturalists may be in their subjectivism, they are eager to avoid Nietzsche's antiegalitarianism.

These multiculturalists are devoted to so profound a politics of difference that they are uneasy about employing the term community to describe the practical manifestation of their politics. As we will see, they go so far as to doubt whether even individuals possess a coherent self or subjectivity—not to mention the cultures formed by these individuals (see Young 1990, 320).3 They claim to respect all human beings as equals, not because of what they have in common but because of their differences. One cannot help but suspect, however, that they also have recourse to some notion of what human beings have in common. If not rational agency, perhaps human beings share something like a will to culture. Because of their egalitarianism, these multiculturalists attribute to all human beings the power to make or contribute to the making of a culture—unlike Nietzsche, who restricts the power of creating a new culture to philosophers of the future (1966, 136). Perhaps they would not find it troubling that they, like Kant, have some vision of common humanity, as long as this vision does not exclude anyone. Willing universal rational laws would provide a standard on the basis of which one might exclude certain human beings from equal respect; for in many communities, Kantian practical reason is not considered the guiding light for political life. In contrast, willing a culture in the egalitarian antiessentialist mode would seem not to provide a standard for exclusion: all human beings belong to some culture; therefore, they must partake of the culture-forming instinct.

In principle, these multiculturalists should refuse

to judge any culture as better or worse than any other: even a culture that on principle does not respect other cultures should be worthy of their respect. They employ Derridean deconstruction precisely to overcome cultural difference, or (as they would put it) to show that difference has no essence (Derrida 1973, 134). Such deconstruction may prove to the respectful multiculturalist that difference can be overcome, but would it not be viewed by the adherent of a culture that refuses to respect others as an act of disrespect? Although these multiculturalists might try to respect such a disrespectful culture, I doubt whether the adherent of the disrespectful culture and the multiculturalist could live side by side in the same community. Try as they might to escape exclusion, these multiculturalists can achieve with respect for cultures no more than the founders of the United States achieved with religious toleration, that is, toleration of all religions that tolerate other religions but intolerance of religions that do not. If I am correct, then respect for difference proves that it cannot escape the exclusion of difference for which it condemns Kantian universalism.

Fellow travelers of the egalitarian antiessentialists have a closer affinity with Foucault than with Derrida. Like Foucault, they are eager to emphasize the identity of power and knowledge, probably because they find themselves excluded from power—or having been excluded from power in the past. As I have implied, this identification of power and knowledge is used to reduce all claims-to-knowledge by those who hold political power to ploys for increasing their power—at the expense of others. Like egalitarian antiessentialists, they are Nietzschean in their subjectivism; however, they are eager to avoid Nietzsche's antiegalitarianism.

These multiculturalists are fellow travelers of the first type because they agree with the long-term goal of deconstructing difference. They, too, hope for a time when exclusion will be at an end. In the meantime, however, because they find themselves excluded from power and their culture insufficiently respected, they must give full expression to their culture before they are willing to part with it. The most thoughtful and articulate spokesperson for this type of multiculturalism is Henry Louis Gates, Jr. 4 As perhaps the world's leading scholar on African American literature, Gates has led the effort to establish a canon of African American literature. Indeed, W.W. Norton chose to publish a Norton Anthology of African— American Literature as edited by Gates which was intended to stand at the center of any such canon. Of course, literary critics following the lead of Derrida have attacked Western (i.e., dead, white, European, male) canon formation on theoretical grounds: canons are intended to generate a subject. Subjectivity, however, is one of Western rationalism's many contributions to ethnocentrism. Derrida hopes to undermine ethnocentrism by showing that there is no solid core to the self. Following Freud, he raises doubts about our ability to grasp all of our motivations consciously.

With this attack on subjectivity in mind, Gates anticipates the criticisms he is bound to encounter from his friends in literary criticism for the formation of an African American canon.

The classic critique of our attempts to reconstitute our own subjectivity, as women, as blacks, etc., is that of Jacques Derrida. "This is the risk. The effect of Law is to build a structure of the subject, and as soon as you say, 'well, the woman is a subject and this subject deserves equal right', and so on—then you are caught in the logic of phallocentrism and you have rebuilt the empire of Law." To expressions such as this, made by a critic whose stands on sexism and racism have been exemplary, we must respond that the Western male subject has long been constituted historically for himself and in himself. And, while we readily accept, acknowledge, and partake of the critique of this subject as transcendent, to deny us the process of exploring and reclaiming our subjectivity before we critique it is the critical version of the grandfather clause, the double privileging of categories that happen to be preconstituted. Such a position leaves us nowhere, invisible and voiceless in the republic of Western letters. Consider the irony: precisely when we (and other third world peoples) obtain the complex wherewithal to define our black subjectivity in the republic of Western letters, our theoretical colleagues declare that there ain't no such thing as a subject, so why should we be bothered with that? (1992, 11)

Gates views his defense of the formation of black subjectivity as an interim measure. Gates leads one to wonder, however, whether the Derridean hope of deconstructing the self and in turn culture (for all its theoretical interest) can overcome the human attachment to "one's own," that is, one's "self" and to one's community. (The clearest evidence that it cannot is, as we have seen, that even Derridean respect for difference succumbs, in practice if not in theory, to a refusal to respect those who belong to disrespectful cultures.) Even though Derrida provides the leading theoretical arguments for contemporary multiculturalism, most multiculturalists share Gates's unwillingness to part with their own subjectivity. Indeed, most of the passion behind multiculturalism aims at the reassertion of one's subjectivity. Although Derrida would have us respond to Kantian rational universalism by extending its inclusiveness to those who do not share Kant's rational principles, most multiculturalists respond by intensifying their attachment to "their own."

One might be tempted to call this parochialism. One wonders, however, whether such multiculturalists are not simply more honest. One finds in Gates an admirable candor about the limits of inclusiveness demanded by egalitarian antiessentialists. When one takes one's culture seriously, when one recognizes that it is "one's own," it becomes far more difficult to submit to an assimilating deconstruction.

If one finds a refreshing candor among egalitarian antiessentialist fellow travelers, one finds a disheartening rancor among the separatist essentialists. The most outspoken proponent of this view is Molefi Kete Asante, who claims to have originated the idea of Afrocentricity (1987, 6). I should note from the start

that some multiculturalists try to exclude Asante from the multicultural group. I include his approach for three reasons. First, he claims that he is a multiculturalist.⁵ Second, his claim to be a multiculturalist must ring true to some extent, because many multiculturalists, especially of the second type, are inclined to support the adoption of an Afrocentric curriculum in inner-city public schools in cities such as Detroit and Washington. They are so inclined because they see that such a curriculum would contribute to the constitution of black subjectivity. By analyzing separalist essentialism, perhaps I can shed some light on the credibility or lack thereof of adopting such a curriculum. Third, his approach reveals some of the problematic tendencies of that historicism which is a key theoretical source of multiculturalism.

I have identified Asante's views as essentialist because, unlike the first two types of multiculturalists, Asante holds that there is a hard core to African culture around which African Americans can rally. Because he is an essentialist, we are not surprised to see what scorn he heaps on Derrida (Asante 1987, 160). Egalitarian antiessentialists and their fellow travelers frequently deny the existence of a cultural essence, because they believe that their cultures change constantly over the course of history. Such arguments are most often made by those who are eager to refashion their own culture so that they might be better suited to modern (which often means Western) conditions. It may be that because African culture is not highly codified, Asante feels no need to make such arguments. However this may be, he is an essentialist not merely because he has no tactical reason for being an antiessentialist but also because his thinking is so deeply indebted to nineteenthcentury German historicism-which was both essentialist and particularistic.6

One reason Asante appears to be a multiculturalist is that he shares a central view of all multiculturalists—that Western rationalism, purporting to be the universal culture, is, in reality, merely a particular culture with imperialistic ambitions. As I have shown, there is an element of truth to this argument. Kant's universal claims are the basis for casting doubts on the humanity of certain cultures. Nevertheless, Asante himself can be shown to make similarly universalistic and exclusive claims. He condemns the "Eurocentric view" for failing to integrate reason with feeling, and he implies that African culture, or the Afrocentric view, achieves this integration (1987, 15-18). By implying the superiority of his own culture, he implies the existence of some external (universal) standard by which cultures may be judged. Afrocentrism, despite disclaimers to the contrary, proves to be at least as ethnocentric as the Western culture that Asante condemns. An Afrocentric curriculum would have as one of its leading purposes to convey to students the superiority of African over European and Asian culture (ibid., 18).7 Unlike the other two types of multiculturalists, Asante is all too clearly antiegalitarian. Asante pretends to share in the multiculturalist respect for difference. He proves, however, to haunt the other multiculturalists as their antithesis: Is Asante not one of those adherents of a disrespectful culture (i.e., Afrocentrism, not African culture itself) that troubled the first type of multiculturalist? Perhaps we should not be surprised that Asante finds it advantageous to ally himself with multiculturalists. The willingness of the egalitarian antiessentialist to show respect for the adherent of a disrespectful culture might prove the former's own undoing.

Wisely, Charles Taylor is highly suspicious of multiculturalism in its third, ethnocentric type. He recognizes how contrary it runs to the Kantian politics of universal dignity. To moderate the ethnocentrism of this form of multiculturalism, Taylor proposes a resolution of the conflict between the Kantian politics of universal dignity and the third (ethnocentric) type of multiculturalist politics of difference. This resolution is supposed to be a compromise between these two views (Taylor 1992, 72). I believe that it is less a compromise than an attempt to compel ethnocentrists to submit before the Kantian position. Rather than accord all cultures equal respect (as the first two types of multiculturalist require us to do), Taylor proposes according all cultures a "presumption" of equality (ibid., 66). But equality as compared with what? One comes to suspect that Taylor admits an implicit standard, namely, the standard of our own neo-Kantian liberalism, although Taylor denies this. Having recourse to a proposal of Gadamer, he calls for a "fused horizon of standards" (ibid., 67, 70). As he explains elsewhere, reason is to be a neutral judge among the various horizons of standards. Try as he might to respect difference, then, Taylor is clearly unable to respect ethnocentric cultures (1990, 46-50). Taylor succumbs to the Kantian tendency (as well as the tendency of the first two types of multiculturalism) to strive to be all-inclusive. Perhaps the ethnocentric multiculturalism of the third type, feeling its particularity threatened, will not choose to abandon its disrespectful view of other cultures. It may even be driven to the surface again by advances made by those who long for universal inclusion. Contrary to what one might expect, we may better moderate ethnocentrism by tempering our demands for inclusion in politics. We should doubt the wisdom of Taylor's attempt to fashion a Kant-friendly form of multiculturalism, because Kant's universalistic ambitions (like those of some of the multiculturalists) seem unable to avoid giving rise to a particularistic back-

In spite of the multiculturalist attack on rationalism, Kant and the multiculturalists prove to share an intense ambition to be all-inclusive. Among the first two types of multiculturalists, this ambition appears as an ambition to include all differences. By including all differences, one cannot help but exclude those who do not respect the difference of others or ethnocentrists: even multiculturalism must exclude. Similarly, Kant aimed at the inclusion of all human beings by showing them how to live up to their full

humanity. By acquiring the universal culture of willing universal laws, all human beings were to become included in the human family. Needless to say, cultural practices that emanate from some source other than Kantian practical reason could not be accommodated in Kant's vision of humanity. As admirable as our desire for inclusion may be, it has perhaps made us forgetful of the ineradicable character of exclusion and attachment to "one's own" in politics.

We are tempted to forget these things because the spirited anger that fuels this attachment has been a leading enemy of modern, especially late modern, political theory. As early as Hobbes, the Enlightenment launched an all-out attack on spirited anger in the form of the love of glory and honor. Others (e.g., Epstein 1984) have argued that at least the early moderns redirected spirited anger into other channels. Assuming that the founders of the United States possessed such an awareness of the role of spirited attachment to "one's own" in politics, one would expect that their vision of inclusion was sufficiently moderated by an appreciation for its limits. One must assume that they, like "their theorist," Locke, recognized that religious toleration could not be extended to the intolerant (see Locke 1955, 24, cf. 17 and 42-43).8 Kant and the multiculturalists, however, seem eager to forget altogether the particularistic attachment to "one's own." For Kant, in the long run politics in all its particularity is to be subject to universal moral laws. All human beings everywhere should subject themselves to these laws. Kant thereby not only elevates morals to new heights of universalism but also displaces metaphysics. The universal ceases to exist in the realm of metaphysics when it becomes the ambition of the moralist.

In contrast, according to Alfarabi's Plato, although one always appeals to universal (metaphysical) principles to justify particularistic politics, these principles never become concrete in morals or politics. Metaphysics always remains somehow beyond direct application to politics. However much it may serve as a rhetorical justification, it never becomes a demonstrative foundation. In other words, for Alfarabi's Plato, Kant's effort to bring the universal into morals and politics is doomed to failure. Furthermore, it leads to the destruction of metaphysical inquiry. Although spirited anger requires universal principles as justification for its less-than-strictly-rational attachment to its own political community, universal principlesand the longing for universal inclusion that they inspire—cannot be fulfilled by spirited commitment to one's own community. This longing can only be fulfilled by inclusion in the only truly universal community, the community of philosophers (see Alfarabi: 1964, 80; idem 1986b, 37).

Although the role of spirited attachment to "one's own" may have become submerged in our political theory, it has come back to haunt us in practice repeatedly. Today this anger manifests itself in two ways: in spirited assertions of the supremacy of one's own culture and in spirited demands that one's group

be not only included but celebrated and promoted. Those making the demands often invoke the term empowerment and the equation of knowledge with power. The neo-Nietzschean assertion that knowledge is power is used by those who sense that they have been oppressed in the past to justify a blanket assertion that only those oppressed in the past (and therefore deserving to exercise power now) have access to the truth. From the adage that knowledge is power some multiculturalists infer that the founders of the United States were not unusually thoughtful men but merely unusually powerful men who intended to enshrine their power by legislating the Declaration of Independence and the Constitution. It is natural to want to respond to such claims with spirited arguments for the superiority of the moral principles of U.S. government to the principles of all other governments. I hope to show that a far less contentious response is appropriate. First, one should assert in a pragmatic vein that experience bears out the superiority of our principles at least for citizens of the United States. Although experiences bearing this out are not difficult to find, I shall not provide such proof here. Second, one should show the limitations of the view that knowledge is power or that everything human is essentially particularistic.

The multiculturalist identification of knowledge and power, like so much else in multiculturalism, stems from the Kantian attempt to make the universal manifest in particularistic politics. As we saw, having brought the universal down to the earth of practice, Kant withdrew the universal from the precincts of the theoretical. Morality and politics were to become the realm of the universal. Kant's argument for an allinclusive moral theory inevitably gave rise to a particularistic backlash against it in the form of historicism. (This relation between Kant and historicism is highly similar to the relation between egalitarian antiessentialism—with fellow travelers—and separalist essentialism.) Having reasserted the particularity of the political world, the historicists had no desire to return the universal to the realm of theory. Kant had cured them of this impulse. Today, the universal is nowhere to be found. By avoiding the Kantian mistake of attempting to make the universal concrete, Alfarabi's Plato does justice to the particularity of politics and its inability to partake of the universal inclusiveness of theory. Furthermore, he reminds us that however deeply the universal may inspire politics, it may not become a part of politics. Once we have remembered the distance of the universal from politics, it will be enabled to reacquire its place in theory or philosophy.

AN ALTERNATIVE TO KANT AND THE MULTICULTURALISTS

Kant and the first two types of multiculturalists pursue an illusory politics of absolute inclusion. Let us turn to Alfarabi's politics in the hopes of learning something about the ineradicability of exclusion and the relation of the universal to political particularity.

Political Science Versus Theology

Because Alfarabi's Plato lived in a different time and place than did Kant and the multiculturalists, it is useful, at least to begin with, to contrast this Plato's politics with a politics of inclusion from the medieval period that is highly similar to the Kantian and multiculturalist politics of inclusion, namely, natural law theory. Like Kant's rational moral laws, Thomistic natural law is supposed to be universal. Kant and Thomas agree in the crucial respect: they identify the universal with the realm of practical politics. Although Thomas speaks of natural rather than rational law, he also holds that natural laws are rational. In contrast, Alfarabi's Plato belongs to the natural right tradition. As we shall see, this tradition denies that law (or politics) is, strictly speaking, rational, natural, or universal. To arrive at an accurate understanding of natural right, it is necessary to counteract our tendency, under the influence of natural law, to read moral universalism back into natural right. In this spirit, I turn from the traditional Western understanding of natural right to the nontraditional understanding of a non-Western interpreter of Plato, Alfarabi.

To show what I am contrasting natural right with, I need to say a little more here about the natural law tradition. Natural law attained its full form as a doctrine of moral universalism in the thought of Thomas Aguinas. The source of natural law's universalism is the universalism of the metaphysics or theology upon which it is founded. Of course, metaphysics claims to provide an account of the whole or of everything. It also claims that its insights are not restricted to a particular time or place. If a metaphysics is universal and a law is derived from such a universal ground, then that law should also have universal validity. Thomas identifies the ordering principle of the metaphysical realm as eternal law. Natural law, which derives from eternal law, rules the human things (Aquinas 1988, 20). Natural law is in human beings as a habitus called conscience. The principles of conscience are the principles of practical reason (pp. 1-4, 45). Thus, natural law is both universal and rational. Nature implants in man an awareness of a hierarchy of human ends.

The structure of Thomas's account is essentially the same as that of biblical revelation: God creates the world, human beings are a part of the created world, and their laws come from the God who orders the world. In brief, politics derives from metaphysics. In spite of the differences between the natural law of the moderns and Thomas, modern natural law has the same essential structure as Thomas's: law derives from one or another metaphysical ground, whether that ground be the ego or something else. Heidegger and his postmodernist (and multiculturalist) followers have detected this kinship between the structure of medieval Western Christian and modern Western

thought. Consequently, they refer to the Western tradition as "ontotheological."

I hope to show that they subsequently made the mistake of projecting the ontotheological tradition's structure of inquiry back onto Plato. Consequently, I have sought the guidance of a non-Western, non-Christian thinker in order to recover this understanding of Plato. If I am correct that Plato does not derive a (universal) natural law teaching from a (universal) metaphysical teaching, then how does he structure his inquiry? According to Alfarabi, Plato begins with political things. He does not offer a demonstrative metaphysics: he derives a rhetorical theology from his inquiry into politics. In other words, the structure of his inquiry is the reverse of the ontotheological tradition.

The most obvious implication of this structure is that it does not guarantee (as beginning with metaphysics does) that one's account of politics will yield universal moral laws. For Plato, all laws—even moral laws—are best when their rule is limited in space and time. It is for this very reason that theology must be rhetorical. In lieu of a universal demonstrative foundation for a universal (or all-inclusive) politics, theology must provide a rhetorical defense of a particular community's laws. Precisely because laws are particularistic, however, a community's theology must appear to provide universal support for that community's laws. Individuals do not sacrifice their lives in the name of laws they view as merely their own. To do so, they must believe that their laws are the best as measured in accordance with some universal standard of justice. Consequently, the particularity of laws is not Plato's final word. Political science reveals the particularity of laws precisely because it can see beyond them to a community that transcends particular political communities, the universal community of philosophers.

This theology, then, must defend the beliefs of a particular community but in the guise of a universal account of the whole. By so appealing to the universal, it excludes those who do not share the proposed view of the whole. This theology is conceived under the guidance of the political scientist who is able to keep in view (1) the particularity of the community's laws; (2) the apparent universality of the community's theology; and (3) the genuine universal community of those who, rather than settling for a particular dogmatic theology, lead a life of inquiry into the whole. By promoting a particular theology, the political scientist creates a certain homogeneity among the members of the political community. Without some degree of homogeneity and exclusion, citizens could not be expected to get along with one another. Although the theology has the appearance of having universal validity or of being demonstrative, it is in fact particularistic and rhetorical. In contrast, the political science that promotes such a theology oversees both the theology in all its particularity and the universal community or inclusion toward which politics strives. It is worth contrasting the complementary relation between this theology and political science with the *synthetic* relation between the natural law theorist's theology and political science. For the natural right tradition, even though theology does not provide the demonstrative foundations of political science, it does provide indispensable rhetorical support. Consequently, even though the political scientist may be responsible for laying down the theology that supports his or her politics, the theology is not inextricably entwined with the political science as a foundation is entwined with what it founds. For the natural law tradition, on the other hand, theology provides the demonstrative foundations of political science. Its theology and its political science are inextricably entwined.

Alfarabi's community did not originally feature a complementary relation between theology and political science, however. Rather, Alfarabi transformed their relation into a complementary one in his Summary of Plato's "Laws" (1991a and b) and in other of his works of political philosophy. In traditional Islam, the art of defending the law is called kalâm. This Arabic term is usually rendered as "dialectical theology." (Taken literally, the term is roughly equivalent to the Greek logos, i.e., word, speech, reasoning, and argument. Rather than "theology," kalâm is usually rendered as "dialectical theology" because of the dialectical form in which its theological arguments or disputes took place. Alfarabi describes the original, antagonistic relation between theology and political science in his Enumeration of the Sciences (1949, 1986a). He says that he is setting forth the "generally known" sciences. In other words, his account of political science and theology reflect the popular perception of the sciences, especially of the bearing of the so-called non-Islamic sciences, such as political science, on Islam (see Mahdi 1975, 113). Political science was viewed popularly as having nothing to say about the divine law (sharî 'a) of Alfarabi's community: it spoke only about the various kinds of regimes. Consequently, when he describes political science in the Enumeration, Alfarabi says nothing about law, much less divine law (1949, 102-8; 1986a, 24-27).

Alfarabi's discussion of political science and the Islamic political arts of jurisprudence and theology are to be found in chapter 5 of the Enumeration. In this chapter, divine law is not even mentioned until immediately after Alfarabi has finished describing political science, when he begins to describe the art of 'jurisprudence'' (figh). With the description of the art of jurisprudence, he turns from the foreign, or non-Islamic, sciences to the Islamic sciences or arts. Unlike the original relation between theology and political science, the relation between jurisprudence and political science was not antagonistic at first or in the long run. Jurisprudence is not in contention with political science, because the jurisprudent, unlike the political scientist, takes the goodness of the Lawgiver's purpose for granted. In the Enumeration, the Lawgiver's purpose is to inculcate belief in certain opinions about God and to regulate certain actions relating to God and to interaction with other human beings. (These beliefs and actions are called "roots.") Needless to say, jurisprudence is not in the habit of questioning the roots, because its task is not even to defend them. Rather, its task is merely to infer new laws from the roots or from laws laid down by the Lawgiver. The first to inquire into the roots are those who are appointed to defend them, the theologians (Alfarabi 1949, 107–8; idem 1986a, 27).

Of course, doubts about the purpose of the Islamic divine law (sharî 'a) are raised by other monotheistic faiths that counter Islam's claim to possess the correct account of divine law's purpose. Maimonides, a student of Alfarabi's philosophical corpus and a philosopher and political theologian in his own right, has suggested, however, that kalâm (dialectical theology) first appeared not in debates between the monotheistic faiths but between Greek and Syrian Christianity and philosophy. These Christians—in particular the rulers and the learned among them held that the philosophic opinions were ruinous of "the foundations of their [divine] Law" (Maimonides 1963, 177). Alfarabi hints at this antagonism between traditional theology and philosophy by saying that whenever it serves their defense of the divine law, one group of theologians embrace "what our intellects reject" and another group rejects what the objects of sense tell us (1948, 109, 111; 1986a, 28-29). Of course, philosophy is possible only if one does not reject out of hand what intellect and sensation provide us. Traditional theology ignores these things because its sole aim is to defend the purpose of the divine law. It will use practically any weapon ready at hand to fight what it deems to be its leading opponent, philosophy.

Although there also existed animosity between ancient Greek paganism and philosophy, it was less intense than that between the revealed religions (e.g., Islam) and philosophy. In no small part, this more intense animosity was due to the aim of revealed religion. Some have noted that revealed religion, unlike ancient Greek religion, had a (universalistic) suprapolitical, religious aim (Strauss 1959, 164). Philosophy also has a universal, suprapolitical (albeit nonreligious) aim. Consequently, philosophy and revealed religion are at odds about the suitable suprapolitical aim of human beings. Philosophy, however-and more specifically, political science-views religion in light of its (particularistic) political aim. The aim of religion within the political sphere is the attainment of human happiness, insofar as this is possible within a particular political community. The traditional theology, which Alfarabi describes in the Enumeration, defends the suprapolitical, universal purposes of revealed religion in spite of the effects that these purposes might have on human happiness within politics. This art of theology is antagonistic to political science, because it is blind to the particularistic character of politics. And political science runs the risk of antagonizing theology until it makes divine law its theme.

Political Science and Political Theology

In the Summary of Plato's "Laws," Alfarabi describes both a political science that takes divine law as its theme and a political theology that defends his community's divine law with weapons compatible with philosophy. This theology is, however, merely compatible with philosophy. Although it does not contravene what sensation or intellect tell us, it is not a demonstrative metaphysics. Not only may limitations on human knowledge prevent us from attaining such a metaphysics, but also (and more importantly) the heterogeneity of human and natural things precludes any metaphysics from being an adequate ground for politics. Although this theology must try to pass itself off as a demonstrative theology or metaphysics to provide sufficient support for a particular community, its content cannot be truly demonstrative or philosophic, because it is conceived with an eye to supporting a particular community. Consequently, I have chosen to refer to it as "political theology," rather than philosophic theology.

The logical (although not the temporal) order in the division of labor between political theology and political science is that political science investigates the opinions and actions advocated by the law with a view to human happiness and perhaps offers some revisions of these opinions and actions, whereas political theology defends the revised law. This division of labor is not temporal, because political science never engages in such an inquiry without the cover provided by political theology. Because political science questions the universal authority of any particular divine law, political theology is always required as an adjunct to defend the law's authority.

There are two principal reasons why I believe that Alfarabi's Plato intends theology to play a particularistic, rhetorical, and political role, rather than a universalistic, metaphysical, and scientific role: (1) Alfarabi's Plato attacks universal law and thus any universal metaphysics that might underlie such a law and (2) Alfarabi downplays the importance of theology while elevating political psychology as the true (nonmetaphysical) foundation of the law. I begin with the attack on universal law of Alfarabi's Plato to make clear that Alfarabi repudiates the revealed religions' ambition for universal rule. Although a theology must appear to offer universal justification (viz., must appear to be a demonstrative metaphysics) for the politics of a particular community, it should serve to support only that particular community. Alfarabi argues that diverse communities are best served by diverse laws (Summary 1.2).10

By placing such limited demands on theology and law, he withdraws the longing for universal inclusion from religion and politics into the realm of philosophy, where it belongs. The philosopher, rather than reveling in dogmatic certainty about the universal order of the whole, is able to lead a life of inquiry into this order. In contrast, political life requires that its principles at least appear to possess a degree of certainty; it requires that political theology appear to

be a dogmatic metaphysics with universal validity. I turn, then, to discuss Alfarabi's downplaying of theology and playing up of political psychology because it is only through political psychology that one comes to understand why theology should be apparently universal and in fact particular. Political psychology reveals that our spirited attachment to our political community (the attachment to "one's own") depends on our belief that our merely particular community receives universal support from a just god or gods. Without such support, spirited human beings become eager to oppress rather than serve their community.

A good theology and law (or divine law) serves to attach citizens to their community and to one another: in other words, it homogenizes and excludes by demanding adherence to certain supposedly universal principles. Although these principles may appear to be universal, their particularity is revealed through political psychology. Rather than stifle inquiry, such theoretically defective principles inspire the inquisitive to search for principles beyond politics that are truly universal. In other words, such a divine law points beyond itself to the philosophic way of life. For Alfarabi's Plato, although a universal standard does exist for politics, it resides just beyond the limit of politics.

First, Alfarabi's Plato uncovers the particularistic character of all theology by leveling a stinging critique of universal laws. Rather than subject politics to a dogmatic (universal) theology, as Thomas Aquinas does, Alfarabi's Plato begins his inquiry into political things with what citizens believe about their particular divine law. In this, Alfarabi's Plato resembles the Plato of the Laws known to us. According to the Cretan citizen in the opening of the Laws, the purpose of his city's laws is to make citizens capable of victory in war. Even within the first book of the Laws, the Athenian Stranger, who leads the discussion, shows his two interlocutors that laws have or should have, a purpose above and beyond this (625c7–635b). Nevertheless, in book 3, the Athenian returns to this martial purpose. Here, however, the stage is wider. He describes a league of three Dorian cities: Argos, Messene, and Sparta. He notes that this league fell apart. Furthermore, he implies that this was fortunate because the league's ambition was to rule the world (687b). It may not be surprising that the Athenian opposes such imperialism. In a Greek world filled with small poleis, it is perhaps hard to imagine world rule. When we turn back from Plato to Alfarabi, the picture remains constant. To be sure, we might have assumed that as a member of a monotheistic community, Alfarabi would favor the ambition of world rule. Instead, he intensifies the Athenian's condemnation of such imperialism. Rather than describe a failed league of three cities, he describes an alliance of two cities against a third city to impose their "divine law" (al-nâmûs al-ilâhî). 11 The result of imposing the divine law is not the redemption but the "corruption" (fasâd) of the conquered city (Summary

The Athenian Stranger attributes the undesirable imperialistic aspirations of the Dorian League to that spirited desire to "have things happen in accordance with the commands of one's own soul—preferably all things, but if not that, then at least the human things" (Laws 687c5-7). Human beings become enamored of great things that offer the prospect of fulfilling this desire (686d). Alfarabi begins by alluding to the prospect (which laws often hold out to their adherents) of fulfilling this desire. He turns quickly to consider analogues to the law in the form of a wonderful ship and splendid riches. Although riches might appear to be an end in themselves, a wonderful ship is more evidently an instrument for something else. Both of these, along with the law, appear worthy of approval merely because they are sources of immense power. Alfarabi warns, however, that one should judge such things not by the quantity of their power but by whether they are "conducive to happiness." He suggests that such judgments are best made not by our passions but by our intellects (Summary 3.5). The human desire that all things should happen in accordance with the commands of one's own soul is so great, however, that human beings are led not just to desire but to pray for what is not really good for them (Laws 687c9-e). A law is truly divine only if it promises a happy political life, not world hegemony. Of course, all of the monotheistic religions, including Islam, ultimately promise world hegemony. Alfarabi's opposition to a divine law that aspires to universal hegemony is therefore especially striking. Apparently, law as such is unsuited to universal application. 12

A brief comparison of Plato's Republic and Laws makes apparent why law is unsuited to universal application. The city in speech in the Republic is to be ruled by philosophic rulers without the aid (or, rather, constraint) of written law (423d8-426a4). Philosophers are capable of adjusting their judgments to the particular place, time, and actors involved in each case that comes before them. In contrast, in the city in the Laws, nonphilosophers rule with the guidance of written laws. 13 Although written laws help to compensate for the defective judgment of some of these rulers, they also limit the rulers' ability to adjust for differences of place, time, and actors (Strauss 1959, 163). Consequently, Alfarabi's Plato opposes universal laws. Furthermore, if the best laws are limited in their application, then one would not expect to be able to derive them from a universally valid metaphysics. Alfarabi's Plato denies the homogeneity of nature and politics, which the natural law tradition affirms.

One might be tempted to infer then that Alfarabi's Plato abandons the task of laying down a theology. On the contrary, although he does not need to offer a demonstrative metaphysics in order to establish an account of politics, human needs require that politics receive rhetorical support from theology. The guiding animus of such a theology is provided by political science, rather than the reverse. Politics requires theological support precisely because politics is al-

ways partisan. Each of us obeys the laws of our own city in spite of the existence of other cities with different laws, because we believe that our laws are truly just. We believe that justice receives universal support. Yet because politics and laws are visibly partisan, they are incapable of providing such support for themselves. Consequently, they require theological support or the support of some form of moral beliefs.

The Athenian Stranger accommodates our requirement that laws receive universal support by arguing in favor of the existence of astral, that is to say universal, gods (Laws 821d, 892a, 896e-97d). A theology that provides such universal support without, however, promising universal political hegemony is best. Consequently, in contrast to Homer's anthropomorphic gods, the Stranger's astral gods are hardly partisan when it comes to war. Each of the revealed religions, by virtue of its claim to offer the sole means of access to the one God, aims at universal rule. Of course, their leading purpose is not to acquire political hegemony but, rather, to reveal the religious truth about the one God to the whole created world. Nevertheless, their leading purpose has the same partisan political effect. Because Alfarabi begins with the political effects rather than the religious ambitions of the divine law, he is able to counteract the profound partisanship or intolerance fostered by the revealed religions. He does so by acknowledging the existence of a multiplicity of particular laws rather than only one universal law (Summary 1.2).

I will turn now to the second reason for believing that Alfarabi's Plato intends his theology to play a particularistic, rhetorical, and political role. Book 10 of the Laws is the primary locus of that work's theology. In the Summary's conclusion, Alfarabi says that he was unable to copy book 10 in order that he might comment on it. He does not say why he was unable to do so. Although he gives the impression of not knowing even how many books there are in the Laws, he indicates by means of hints how many there are (Strauss 1959, 134). If I had only the evidence provided by Alfarabi's silence on book 10, I could not conclude that Alfarabi denies that Plato's politics has a theological starting point. I also have, however, as evidence the fact that throughout the Summary, Alfarabi habitually omits Platonic references to the gods or god. Above all, Alfarabi omits any reference to what, at least in the Christian tradition, came to be the most frequently cited lines of the Laws, the Athenian's assertion in book 4 that the god is the beginning, middle, and end of all things (Laws 716a; cf. Summary 4.9 and Strauss 1959, 148). Alfarabi omits reference to this although commenting rather closely on the rest of the passage.

Alfarabi, however, is not absolutely silent about book 10. In the opening of his summary of book 9, he says that Plato presents his discussion (*kalâm*) of the laws' roots in books 1–8. But it is undeniable that book 10 contains the roots as they are traditionally understood, namely, the opinions about god. Books 1–8, then, must contain the roots of the political

scientists' revised law, by which I mean not what are traditionally understood to be the roots but the purposes of the revised divine law. In the defense of the revised law, unlike the defense of the traditional law, there exists a divergence between the theological opinions that the political theologian defends and the ultimate purpose of these opinions. The ultimate purpose of political theology is not belief in certain opinions about god so much as the formation of certain kinds of souls. Although this theology defends certain opinions about god, its purpose in doing so is not essentially theological or metaphysical but political or psychological (see Pangle 1976, 1077). Thus, in his summary of book 9, Alfarabi goes on to say that what follows the opening of book 9 (in other words, most of book 9 and books 10-12) "explain things that adorn and embellish the law and things that are the consequences of the roots" (Summary 9.2). Insofar as the roots are the purposes of the laws, the laws are themselves the consequences of the roots. Most of book 9 and books 11–12 concern laws, or (as Alfarabi says) "the consequences of the roots." What is left over once one subtracts from books 9-12 most of book 9 and books 11-12—namely, book 10-is what "adorns and embellishes the law." Far from containing the foundations of the law, the theology in book 10 provides the necessary adornments and embellishments of the law. In opposition to this view, Kleinias, the Athenian Stranger's leading interlocutor, asserts that book 10 presents "just about [the] noblest and best prelude on behalf of all the laws" (Laws 887c).

In spite of Kleinias' objections, the theological prelude in book 10 is merely an adornment—which is not to say that adornments might not have substantial purposes. The political theology in book 10 is indispensable as a defense of the divine law. An account of the gods of the sort described in book 10 is indispensable for the cultivation of the spirited kind of soul, like Kleinias', that will rule the city in the Laws (Pangle 1976, 1062–65). In summarizing the first nine books of the Laws, Alfarabi reveals the psychological purpose of the laws and of the political theology presented in book 10. The heart of the Summary, then, is Alfarabi's summary of book 5, whose theme is how to honor the soul properly (see Strauss 1959, 149–50). Plato presents the purpose of book 10 in book 5.

In contrast with Kleinias, the Athenian presents his version of the prelude on behalf of all the laws in books 4–5. This version of the prelude falls into two parts, or preludes, along the line separating book 4 from book 5. First, in book 4, he presents a "prelude as regards the gods, those who come after the gods, and the living and dead ancestors" (Laws 724a). Second, in book 5, he presents a prelude as regards "how [human beings] should be serious and how they should relax as regards their own souls, their bodies, and their property" (724b). Alfarabi's interpretation of these two parts of the Athenian's prelude strips away the Athenian's adornments of the law to reveal the purposes of the laws.

In his summary of the first part of the prelude, Alfarabi quickly undermines any inclination one might have to view the account of the gods in this first part as belonging to the prelude on behalf of all the laws (Summary 4.9). As I noted, he omits any mention of the striking opening of this part, in which the Athenian announces that the God is the beginning, middle, and end of all things. Alfarabi only notes that the gods are displeased with the arrogant man. Nor does he make mention, as Plato does, of man's need to sacrifice to, pray to, or serve the gods (see Laws 716c-18a; cf. Summary 4.10). The gods' "support" is acquired, at least by the ruler, merely by avoiding arrogance—which, it so happens, is a way of serving other human beings, rather than gods. The only things that Alfarabi suggests human beings need to care for are their own bodies, souls, and property, both for the sake of themselves and for the sake of their families. In other words, Alfarabi gives an account of the first part of the prelude (in book 4) that leaves it indistinguishable from the second part (in book 5). Finally, when he turns to summarizing book 5, he does not mention the gods as those beings who must be honored before the human soul (Summary 5.1). He merely notes that the human soul ranks third in divinity, and he chooses to add to what the Athenian says the following statement, "The human soul is the noblest of things." Thus, the gods nearly vanish from the prelude of the law as a whole, leaving an account of the human being and, above all, of the human soul. Psychology, rather than theology, is the foundation of Plato's politics. This is by no means to be taken as a sign that Plato views psychology as providing a metaphysical ground in the same way that the ego does for modern political philosophy. For the moderns, the ego is the ground for politics because it is the ground for the whole. For Plato, the human soul is not the ground of the whole. Rather, the soul is the foundation of the law because the purpose of the law is the formation of certain kinds of souls.

From the point of view of philosophy, the political theology is merely an adornment; from the point of view of the spirited soul, the theology provides universal support for its belief that just service of the city will be rewarded and injustice punished. But the theology is intended to serve both the political ends of spirited human beings, which root them in their political community, and the philosophic ends of philosophic human beings. It serves the former by appearing to provide universal support to their particular political community, and it serves the latter by constantly revealing the incompatibility between the form of universal support offered and the particular political ends served. Although Kleinias has no doubt that astral gods should care about the justice or injustice of particular human beings, the Athenian Stranger cannot help but wonder whether astral gods would recognize human injustice as such, let alone care for human beings.

CONCLUSION

Thomas Aquinas, like Kant, believed that politics could become rational. Consequently, he, like Kant, retained the ambition, which they both inherited from the revealed religions, of establishing an allinclusive politics. The search for an all-inclusive politics leads to the particularistic reactions of German historicism and, nowadays, of Asante. Try as Kant and Thomas do to extirpate our attachment to "our own," they are unable to do so. In spite of the Enlightenment attack on spirited attachment to one's community, this feeling is not to be eradicated among human beings. Spiritedness is the basis both for that sense of self or subjectivity that Derrida attacks in the spirit, if not the manner, of the Enlightenment and for that sense of responsibility that will not vanish from human life until politics vanishes from human life. How ironic it would be for politics to vanish from human life at the very moment that human beings were willing to say that all knowledge is political or serves the political interests of certain individuals or groups. We have been led to this point by stages: first, Kant repudiated metaphysics and drew the universal down into morals and politics; then German historicism affirmed the particular as against the universal in morals and politics. Although multiculturalists repeat the dynamic of inclusion and exclusion between Kant and the historicists, they have come to share the historicist view that all knowledge, like politics, is contingent and particular. If knowledge is as contingent and particular as politics, then it is not difficult to see how knowledge would become identified with politics. By avoiding the temptation to pursue a politics of absolute inclusion, Alfarabi avoids the dialectic of denying the existence of exclusion only to then see (as we have seen all too often in the last two centuries) exclusion raise its head in the ugliest forms. By relegating absolute inclusion to the realm of philosophic inquiry, he keeps sight of just how much homogeneity and exclusion political life requires. By distinguishing politics from knowledge, he enables us to see the difference between spiritedness and the love of knowledge. Needless to say, politics will not vanish from among us. The politics of inclusion could, however, compel us to forget, if only temporarily, the difference between spirited self-assertion and the self-forgetful desire to know.14

In conclusion, Alfarabi provides us with a revised understanding of Plato, an important source of our tradition. In so doing, he enables us to reevaluate the natural law tradition from which the moral universalism of the United States derives. Alfarabi's Plato raises doubts about the usefulness of universal laws yet recognizes our need to view our laws as receiving universal support. On the one hand, multiculturalist opponents of Western moral universalism would do well to recognize and respect this need. On the other hand, defenders of U.S. government might do well to temper the fervor of their claims that the moral principles of our form of government are the best

principles for all people everywhere in the long run. Would it not be sufficient to remind the doubtful of the great advantages these principles have provided even them?

By reminding us of the possibility of the philosophic way of life, Alfarabi's Plato raises doubts about the claim that all things, including knowledge, are particularistic or partisan. He shows that knowledge, unlike power, need not be partisan. The claim that knowledge is power misleads us into believing that all claims to possess knowledge are merely plays for power. Instead, we should realize that we are well served when, as in the case of the founding of the United States, knowledge and power can come together to form so rare a combination.

Notes

I would like to thank the Symposium on Science, Reason, and Modern Democracy in Michigan State's Department of Political Science and the Carthage Foundation for providing me with the opportunity to write this essay. Furthermore, the theme of the symposium's 1992–93 lecture series and conference, multiculturalism in the university, served as an inspiration for my theme. I thank my colleagues in the symposium, Jerry Weinberger and Werner Dannhauser, for their helpful insights over the course of the year. M. Richard Zinman, the director of the symposium, made very useful comments on an earlier version of this paper. Ralph Lerner, Muhsin Mahdi, and Nathan Tarcov suggested revisions on an earlier version of the second part.

1. See, e.g., the account of *Cultur (sic)* given by Ranke (1881, 1:v-vi) and the overview of historicism given by Schnadelbach (1984, 33–65).

2. My typology owes something to Michael Geyer's (1993, 517–26).

3. To some extent, Young is uneasy about using the term community simply because she opposes the communitarian conception of community. I fully agree with her opposition to communitarianism on the grounds that it lends itself to totalitarianism. Nevertheless, I would agree with another multiculturalist, Minnich, who is evidently indebted to communitarianism, that in the formation of community one cannot avoid exclusion (1992, 189 and references to Arendt).

4. Some other adherents of this type of multiculturalism are Cornel West and Henry Giroux, both of whom, however, are more self-consciously eclectic and make more ample use of Marx or class analysis than does Gates. See Giroux 1992, 13; West 1990, 30–31, 35–36; idem 1992; see also Butler 1991.

5. Asante defends his multiculturalism in a response to Diane Ravitch's identification of him as a "particularistic multiculturalist," (Asante 1992, 303–8; Ravitch 1992, 276–79). Although Asante defends his multiculturalist, Charles V. Willie takes Ravitch to task for including among the multiculturalists those who would link biological race and culture (1992, 72). Asante claims not to link biology and culture, though he also cites the theories of biological Afrocentrists with approval (1987, 62–63, 124).

6. Cf. Asante's (1987) praise of Africa with Ranke's (1888) belief in the cultural supremacy of Germany. One of the gravest errors Derrida makes—repeated constantly by his multicultural followers—is the identification of Western rationalism with racism and ethnocentrism (1976, 120–21, 130–35; idem 1985, 293, 297). Although nineteenth-century German race theory was originated by historicists and although historicism is essentialist, race theory and historicism are not rationalist. Indeed, they constitute a reaction against Kantian universalist rationalism. Historicism found its primary source in German romanticism, which found its source, in turn, in

Rousseau's attack on Enlightenment rationalism in favor of feeling or instinct. The Enlightenment had engaged in the most concerted opposition to the human attachment to "blood and soil" in Western history. Far from being of a piece with Enlightenment rationalism, nineteenth-century race theory was a reaction against its excessive universalism.

7. Contrary to those who might want to include Asia together with Africa as a non-Eurocentric culture, Asante makes it clear that he has no intention of allowing Asia to share in the cultural outsider status (and thus supremacy) of

8. Following Derrida, multiculturalists often accuse Locke's Enlightenment rationalism of excluding, for instance, women and minorities. They imply that the appeal to universal rights (sameness, rather than difference) must always exclude (e.g., Fish 1992, 249, 258). As I have explained, they are in a sense correct: an appeal to such rights does exclude those who would deny such rights to others. They are wrong, however, in implying that these principles exclude women and minorities. No doubt certain distorted interpretations of them might, but the principles themselves do not. Our failure is not in adopting such principles but in failing to live up to them.

9. For more background on kalâm, see Frank 1992.

10. Both the Arabic and the English translation (Alfarabi 1991a and b) of the Summary attempt to follow Alfarabi's divisions into discourses and paragraphs. (At present, the English translation is more successful in doing so and will serve as the model for correcting the Arabic edition when it is published.) I cite both editions with the abbreviated title Summary. However, when discrepancies exist between the two editions, the reader should refer to the translation.

11. Lest he be seen as indicting Islam, Alfarabi uses a very unusual locution for "divine law," rather than sharî 'a, the term appropriate to a revealed religion.

12. The regime Alfarabi envisions in the Summary is not as limited in size as Plato's city in the Laws (737d). Indeed, it appears to cover more than one climatic region. Nor does this regime appear to contain a multiplicity of different divine laws. Rather, it appears to contain one religious community with a multiplicity of governors ruling the various climatic regions (Summary 8.7-8). Consequently, it is tempting to suppose that Alfarabi here envisions a single homogeneous regime encompassing the entire inhabited world. In the Summary, Alfarabi addresses the aspiration to world rule of his religious community perhaps more realistically than anywhere else in his corpus.

In other writings such as the Virtuous City, he imagines the formation of a virtuous association of the inhabited world. As others have explained, such an association would be composed of associations of virtuous nations, which in turn would be composed of virtuous cities (Alfarabi 1985, 228-31; Mahdi 1987, 220, 222-23; cf. Galston 1990, 150-53). The formation of one virtuous city (let alone such a multiplicity of virtuous regimes) is not realistic. Nevertheless, Alfarabi's vision of this virtuous association of the inhabited world does one crucial thing: it replaces his monotheistic community's aspiration for world hegemony with an aspiration for a unified but heterogeneous world regime composed of a multiplicity of religious communities (Mahdi 1987, 223). In view of the ideal described in his other writings and Alfarabi's criticism of laws seeking world hegemony in the Summary, I believe that Alfarabi's realistic aim in the Summary is that his own community, in view of its religious homogeneity (though it may stretch over a multiplicity of climatic regions) be limited in size (Parens 1992, 143-55).

13. For the relation of the ruler to the laws, cf. Laws 729e and 770b5-71a6. And for Alfarabi's views on the relative rank of the city ruled by living wisdom and of the city ruled by written laws, see Alfarabi 1964, 80-81; idem 1968, 50, 56; idem 1985, 248-53; idem 1986b, 37; idem 1991c, 10, 18.

14. On the respective roles of spiritedness (thumos) and love (eros) in Laws, book 10, see Pangle 1976, esp. 1074-77.

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RESEARCH NOTES

SOCIAL CHOICE IN A REPRESENTATIVE DEMOCRACY

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itizens of a representative democracy are twice removed from legislation. First, they do not deliberate and vote directly on legislation. Rather they elect assemblies that enact such legislation in their stead. Second, and less commonly remarked, citizens do not vote directly for assemblies. Rather they vote for individual candidates, with the candidates receiving the most votes elected. We examine the efficiency properties of these voting systems.

In a representative democracy, citizens stand at two removes from legislation. First, they do not deliberate and vote directly on legislation: rather, they elect assemblies, which enact such legislation in their stead. Second, citizens do not vote directly for assemblies: rather, they vote for individual candidates, and the winning candidates then constitute the assembly. Thus, although in the elections of city councils, school boards, the U.S. Congress, and the legislatures of the states, in each case an assembly is being chosen, voters are called upon to vote for candidates, and these candidates have their votes tallied as individuals.

Perhaps because these elections are candidate-based, analyses of them have often taken voters' preferences over candidates as their starting point. There is something peculiar in this approach, however. At least since Black (1958) and Arrow (1963), social choice theory has viewed society's decision problem as one of choosing an outcome based on the preferences of the individual members of society. That is, the theory assumes that each person has an ordering over the possible outcomes and compares various functions from these orderings to an outcome. In the present context, an outcome is an assembly. Therefore, preferences over assemblies, not candidates, are fundamental. Indeed, any ranking of the candidates must be understood relative to the ranking of the assemblies. An appreciation of this simple fact has important implications for the understanding of our political institutions.

Several authors have argued that voters have preferences first over legislative programs and that these legislative preferences then induce preferences over assemblies (e.g., Austen-Smith and Banks 1988; Downs 1957). To the extent that there is a definite correspondence between assemblies and the programs they enact, this approach is not in conflict with the approach adopted here. Since preferences over legislative programs induce preferences over assemblies, authors who adopt this approach recognize the primacy of assembly orderings over candidate orderings. However, these authors have been primarily concerned with the fact that the assembly preferences may not be separable. As far as we know, no one has considered the difficulties that arise even when assembly preferences are separable.

Moreover, voters may evaluate assemblies only partially (or not at all) on the basis of the legislative programs that they enact. A voter may prefer an assembly that represents all the relevant constituencies to one that does not. Or, not knowing (and unable to imagine) what substantive issues may arise, a voter may have to evaluate the assembly on other grounds. In each of these cases, the voter's preferences over assemblies will be more fundamental than preferences over legislative outcomes or candidates.

We shall show that unless preferences over assemblies satisfy strong conditions, typical procedures for electing assemblies are inefficient. We identify conditions that ensure the election of a Pareto-optimal assembly but also show that regardless what neutral restriction is imposed upon preferences, an "almost inefficient" assembly may be elected. We also briefly discuss proportional representation from an assembly-based perspective. (All proofs are in the Appendix.)

Candidate-based and Assembly-based Procedures

We classify procedures for electing assemblies along two dimensions: (1) whether candidates must declare which post they contest and (2) the scope of the electorate that votes for each position in the assembly (see Benoît and Kornhauser 1991). We call assemblies in which candidates do not declare which post they contest at-large assemblies. An assembly in which candidates must declare the post they contest is a designated-post assembly. We divide designated-post assemblies into two types: if the entire electorate votes on each post, the assembly is a numbered-post one; if different portions of the electorate vote on specific posts, the assembly is districted.² Note that our definition of designated-post assemblies encompasses not only legislatures but also governments composed of separate houses and an executive.

Let $N = \{a_1, a_2, \ldots, a_n\}$ be the set of candidates. Let \mathcal{A}_m be the set of all possible assemblies. Let v be the number of voters. Let $\Gamma(N)$ be the set of all possible orderings of the elements of N; 3 let $\Gamma(\mathcal{A}_m)$ be the set of all possible orderings over \mathcal{A}_m .

Definition 1. An assembly-based procedure is a function $f: \Gamma(\mathcal{A}_m)^v \to \mathcal{A}_m$.

DEFINITION 2. A candidate-based procedure is a function $g: \Gamma(N)^{\mathbf{v}} \rightarrow \mathcal{A}_{\mathbf{m}}$.

We assume that each voter i has a well-defined preference ordering $R_i(\mathcal{A}_m) \in \Gamma(\mathcal{A}_m)$ over assemblies, with "A > B" meaning "Assembly A is strictly preferred to assembly B" and " $A \ge B$ " indicating weak preference.

We note that while our definitions do not immediately apply to party systems such as party-list proportional representation, our ensuing discussion (esp. theorem 1) can be applied to these systems.

Separability

In some situations, complementarities among the candidates may determine the desirability of an assembly. In such cases, it may be that some voters' favorite assembly consists, for instance, of candidates a_1 and a_{10} but that these voters only like a_1 because of how well a_1 works with a_{10} . If a_{10} is not present, these voters do not want a_1 on the assembly. Other voters may like the way that a_6 and a_8 interact; and so forth.

When preferences over assemblies have this interactive nature, choosing assemblies by voting for individual candidates does not seem sensible; the "wrong" combination of candidates might easily be elected. Indeed, with this type of preferences, there is no immediate way for voters to rank individual candidates.

On the other hand, it may be that the desirability of a candidate can be determined independently of the composition of the rest of the assembly; that is, preferences may be separable so that a voter who prefers the assembly $\{a_1, a_{10}\}$ to $\{a_2, a_{10}\}$ prefers $\{a_1, a_7\}$ to $\{a_2, a_7\}$ and, in fact, prefers a_1 to a_2 on any assembly. In this case, there is a natural ranking of the candidates, and one might think that candidatebased procedures are then appropriate. However, as we shall show, even in this "good" case, common candidate-based procedures fail to have minimally desirable properties.

Formally, we define separability as follows. For any two candidates a_i and a_j , let X_{ij} be a set of (m-1)candidates not including a_i or a_i .

DEFINITION 3. An individual has separable assembly preferences if and only if, for all ai, ai, Aii, and Bii, $\{a_i\}UA_{ii} > \{a_i\}UA_{ii} \text{ implies } \{a_i\}UB_{ii} \ge \{a_i\}UB_{ii}.$

A separable assembly ranking generates a unique candidate ranking in the obvious way: $a_i > a_j$ if and only if there exists an A_{ij} such that $\{a_i\}UA_{ij} > \{a_i\}UA_{ij}$, and a_i is indifferent to a_j if and only if $\{a_i\}UA_{ij}$ is indifferent to $\{a_i\}UA_{ij}$ for all A_{ij} . Many assembly rankings, however, are consistent with any single candidate ranking; that is, a single candidate ranking could be generated from many different separable assembly preferences. The following proposition elaborates this idea:

Proposition 1. Let $a_1 > a_2 > ... > a_n$ be a candidate ranking and A and A' be two assemblies. If and only if there exist i and j such that the ith-highest-ranked candidate in A is higher ranked than the ith-highestranked candidate in A' but the jth-highest-ranked candidate in A' is higher ranked than the ith-highest-ranked candidate in A, then A is ranked above A' by some consistent assembly rankings and below A' by others (see Benoît and Kornhauser 1991).

Nonstrategic and Simple Voting

A strategic voter tries to anticipate the votes of others when deciding how to vote. A nonstrategic voter, on the other hand, votes solely as a function of personal preferences. Of course, voting nonstrategically may not be optimal. Nonetheless, in what follows we often assume that voters act nonstrategically.

We make this assumption for three reasons: (1) in many situations, nonstrategic voting may reasonably describe voter behavior; (2) an understanding of nonstrategic voting is an important first step to the understanding of the candidate-based procedures that predominate in our political institutions; and (3) most importantly, the assumption of nonstrategic behavior simplifies the exposition without undermining the force of the argument, as our results do not hinge upon this assumption.5

When preferences are separable, a salient nonstrategic way of voting is for a voter with k votes to cast these votes for the top *k* candidates in the candidate ranking generated from the voter's assembly ranking. Such voting is akin to sincere voting and maximizes a voter's expected utility with respect to a uniform distribution on the behavior of other voters. Elsewhere, we have termed this simple voting.6

WHY ASSEMBLY PREFERENCES MATTER

Candidate-based procedures ought to be evaluated in terms of preferences over assemblies. Put differently, preferences over assemblies, not individual candidates, are fundamental. Adopting this perspective has significant consequences for an understanding of representative institutions. We shall first offer examples concerning efficiency and proportional representation that illustrate this claim. Later, we shall offer some general theorems concerning the efficiency properties of candidate-based procedures.

An Example

Our first example shows that candidate-based procedures may elect Pareto-inferior assemblies. There are five candidates contesting two seats on an at-large assembly. The voters divide into four groups. The first group ranks the 10 assemblies as follows: $\{a_4, a_2\}$ $> \{a_4, a_3\} > \{a_4, a_1\} > \{a_4, a_5\} > \{a_2, a_3\} > \{a_2, a_1\} > \{a_3, a_1\} > \{a_2, a_5\} > \{a_3, a_5\} > \{a_1, a_5\}.$

Notice that preferences are separable. This assembly ranking generates the candidate ranking $a_4 > a_2 >$

 $a_3 > a_1 > a_5$.

Other separable assembly rankings are consistent

TABLE 1 Example of Foreign Candidate	-	s' Rankin	g of	1
	(ROUPS O	F VOTER	S
	1	2	3	4
RANKING OF CANDIDATES	15 VOTERS	5 VOTERS	5 VOTERS	15 VOTERS
1st	a ₄	a ₁	a ₁	a ₃
2nd	a_2	a_2	a 5	a ₅
3rd	a_3	a_3	a_3	$a_{\scriptscriptstyle 1}$
4th	a_1	a_4	a_4	a_4
5th	a_5	a_5	a_2	a_2

with this same candidate ranking. For instance, reversing the preference between $\{a_3, a_1\}$ and $\{a_2, a_5\}$ in the assembly ordering produces separable preferences that generate the same candidate ranking.

The remaining three groups also have separable assembly preferences, which generate candidate rankings. Table 1 presents the four rankings of candidates. The number at the top of each column gives the number of voters in that group.

Suppose (as is often the case with two-person assemblies) that everybody casts two votes for two different candidates and that the top two vote-getters form the assembly. We assume that everyone simply votes for their top two candidates. The assembly $\{a_2, a_5\}$ is elected with 20 votes for a_2 and 20 votes for a_5 . But *every* voter prefers $\{a_1, a_3\}$ to $\{a_2, a_5\}$! In fact, the last three groups of voters rank $\{a_1, a_3\}$ second, while they rank $\{a_2, a_5\}$ no higher than seventh. (More precisely, in every assembly ranking consistent with these candidate rankings, $\{a_1, a_3\}$ is ranked second by the last three groups, while $\{a_2, a_5\}$ is ranked seventh or eighth.)

The election of the inefficient assembly $\{a_2, a_5\}$ is not due to an assumption of nonstrategic voting or a failure of the voters to recognize the superiority of $\{a_1, a_3\}$. If the voters agreed to vote for a_1 and a_3 , the first two groups (for instance) would have a strong incentive to deviate from the agreement and vote for a_2 , instead of a_3 , since each member of these groups prefers $\{a_1, a_2\}$ to $\{a_1, a_3\}$.

Although the prevailing candidates in Table 1 do not win decisively, it is easy to write down examples in which the winning candidates receive votes from an overwhelming majority of the voters or form a Condorcet set of candidates capable of beating all other candidates pairwise and in which the winning assembly is still inefficient. Furthermore, an elected assembly could rank, say, in the bottom 20% of all possible assemblies for all voters, while all voters agree that a certain other assembly ranks among the top 10%. Hence, the election results themselves provide no indication of the existence or severity of a problem.

Strategic Voting and Endogenous Preferences

In Table 1, the list of candidates is given exogenously. We now present a simple game-theoretic example in which the positions of candidates is determined endogenously, voters use dominant strategies, and the final outcome is inefficient. Thus, the cause of the inefficiency is the use of the candidate-based procedure and not that the "wrong" candidates have been assumed at the outset or that voters are not acting strategically.

The assembly is a numbered-post one in which the three posts consist of different offices (e.g., sheriff, school superintendent, and sanitation chief). Two candidates are running for each office. For each office, a candidate can adopt a position that is described by a real number from 0 to 1.

The voters divide into three equal-sized groups. Group 1's favorite positions on the three offices is given by $\{1, 0, 0\}$. Members in group 1 care most about the first office, so that they prefer the assembly $\{1, 1, 1\}$ to the assembly $\{0, 0, 0\}$. Group 2's and Group 3's favorite outcomes are given by $\{0, 1, 0\}$ and $\{0, 0, 1\}$, respectively. Individuals in groups 2 and 3 care most about the second and third offices, respectively, so that they, too, prefer the assembly $\{1, 1, 1\}$ to $\{0, 0, 0\}$. Preferences are separable.

The game is in two stages. In the first stage, candidates choose a position. In the second stage, individuals cast their votes, one for each office. Each office is decided by majority rule. We look for a subgame perfect equilibrium in undominated strategies.

Since preferences are separable and there are only two candidates per office, it is always dominant for an individual to vote for the candidate that is closest to the voter's ideal position for each office. Since for each office, two-thirds of the voters have 0 as ideal point, in equilibrium, both candidates will enter at 0 (the median). Thus, we have the following:

PROPOSITION 2. The unique, subgame perfect equilibrium outcome in undominated strategies is the election of the assembly $\{0, 0, 0\}$.

The assembly $\{0, 0, 0\}$ is elected although all voters prefer $\{1, 1, 1\}$.

Proportional Representation

At its most basic level, the concept of proportional representation holds that if x% of the electorate has identical preferences, then that group is entitled to choose x% of the members of an assembly. We shall argue that if preferences are only assumed to be separable, this candidate-based concept is suspect. The concept of proportional representation, however, can be defended if assembly preferences satisfy a stronger restriction on preferences. ¹⁰

Consider 10 candidates running for five seats and suppose that each voter has separable preferences over assemblies. Suppose 40% of the voters have identical preferences and that the voting procedure

perfectly realizes the group's entitlement under proportional representation to elect 40% of the assembly. That is, the group can assure the election of its top two candidates. As 56 of the 252 possible assemblies include these two candidates, the voting procedure eliminates 196 possible assemblies.

But these are not just any 196 assemblies. If the coalition were, instead, given the right to eliminate any 196 assemblies, it might well have chosen to exclude assemblies that contained undesirable candidates, rather than those that did not include desirable ones. In other words, although the outcomes have been restricted to 56 assemblies, the resulting assembly does not necessarily rank among the coalition's top 56. In fact, the elected assembly may rank as low as 232d. Even a coalition that finds its top four candidates (80% of the assembly) elected may rank the resulting assembly no higher than 125th.

Thus, allowing a group to include its top candidate(s) in an assembly may accomplish very little. To make sense of proportional representation, an additional assumption is required. Separable preferences are lexicographic when assembly A is preferred to assembly B if and only if the highest-ranked candidate in $A/(A \cap B)$ is preferred to the highest-ranked candidate in $B/(A \cap B)$. An individual with lexicographic preferences always chooses to eliminate first those assemblies that do not include his or her top candidates. Under proportional representation, candidates are also eliminated in this way and the resultant assembly is efficient. In the example, with lexicographic preferences, the coalition of 40% of voters elects an assembly that is ranked no lower than fifty-sixth (or in the top 23% of all assemblies). While it may be too strong to say that the assumption of lexicographic preferences "justifies" proportional representation, at least there is a certain sense to it in this case.

THE EFFICIENCY OF CONSTANT SCORING SYSTEMS

The inefficiency in both Table 1 and the game-theoretic example is due to the candidate-based nature of the voting method used. This inefficiency arises with a wide range of candidate-based voting methods and with a wide range of assumptions about voter behavior. Rather than give the most general possible theorem, however, we offer a fairly broad theorem that is easily stated (and proved).

DEFINITION 4. A constant scoring system for an at-large assembly of size m is one in which each voter is given some number, $k \le m$, of votes to be distributed among k different candidates and the assembly elected consists of those m candidates receiving the m greatest total number of votes.

Assumption 1. The size of the assembly is $m \ge 2$, the number of candidates is $n \ge m + 3$, and the number of

voters is $v \ge max(m, 6)$. When preferences are separable, individuals vote simply for their top k candidates.

THEOREM 1. Under assumption 1, even if voters have separable preferences, a constant scoring system may result in the election of an at-large assembly A although there is an at-large assembly B that everyone prefers.

Pareto-optimality is generally considered to be a rather weak requirement and is satisfied when individuals vote directly for the outcomes, using, for instance, plurality rule, plurality with a runoff, and a Borda count. Theorem 1 shows, however, that when assemblies are being chosen but candidates are being voted upon, this requirement is, in general, too strong.

We now offer a condition on preferences that is sufficient to guarantee that a constant scoring system yields a Pareto-optimal assembly. Let |S| denote the number of elements in S.

DEFINITION 5. An individual has k-block preferences over assemblies if and only if there exists a set B(k) of candidates such that for any assemblies A and A', if $|A \cap B(k)| > |A' \cap B(k)|$, then A is strictly preferred to A'. 11

We will say that an individual whose preferences are k-block for some k has block preferences. Preferences that are block may or may not be separable. In either case, voting for the block set of candidates is simple.

An individual with block preferences has favorite candidates and is most concerned with getting the maximum number of these candidates elected. Thus, someone with 1-block preferences has a most preferred candidate and prefers any assembly with this candidate to any assembly without. A person with 2-block preferences has two preferred candidates and prefers any assembly with one of these candidates to any assembly without either and any assembly with both these candidates to one with fewer than both. No further restrictions are imposed. An individual with strong dislikes who is concerned with keeping certain candidates out of an assembly is not likely to have k-block preferences.

As the next theorem indicates, k-blockness guarantees Pareto optimality in constant scoring systems. In the proof of the theorem, we ignore the possibility of two candidates' receiving the same number of votes. Ties may be broken by using an arbitrary voter as tiebreaker. Breaking ties by randomly selecting candidates, however, may be inefficient.

Assumption 2. A voter with k-block preferences and k votes casts these votes simply for the block set. The electorate is sufficiently heterogeneous that at least m candidates receive votes.

THEOREM 2. Under assumption 2, if every voter has k-block preferences and k votes, then a Pareto optimal assembly is elected.

While this is a positive theorem for voting over candidates, several comments are in order. First, the theorem requires not only that voters have block preferences, but also that all these preferences be k-block for the same k and that this k be the number of votes that each person is to cast. This is a lot to ask. However, if individuals are given either too few or too many votes, efficiency cannot be guaranteed.

Approval voting offers some hope in this regard. Under approval voting, each person can cast as many votes as desired. One reasonable way for a person with k-block preferences to vote in such a system would be to cast k votes for his or her block set. This assumption gives the following:

THEOREM 3. If every voter has block preferences than approval voting (with each individual voting for his or her block set) elects a Pareto-optimal assembly. If preferences are only separable, a Pareto-inferior assembly may be elected.

Blockness is quite a strong assumption. Nevertheless, it is not easily relaxed. If even a few people have nonblock preferences a Pareto-inferior assembly may be elected. Furthermore, as Theorem 4 will indicate, even if all preferences are block or satisfy stronger restrictions, a "nearly inefficient" assembly may be elected. To state the result, we need some preliminaries.

A restriction is on *individual preferences* (rather than across preferences) if it can be verified by checking preferences in isolation. Thus, separability is a restriction on individual preferences, since any single preference ranking can be checked for separability; single-peakedness is a restriction across preferences, since it can only be violated by a set of preferences.

A restriction is *neutral* if it is independent of the labeling of candidates; that is, if a preference ordering P satisfies a neutral restriction, then P' will satisfy the same restriction, where P' is defined as follows: for all assemblies A_i , let A_i' equal A_i , with a_r replaced by a_m and a_m replaced by a_r (if a_r or a_m occurs in A_i ; otherwise, A_i' equals A_i). Then in the ordering P', A_i' is ranked ahead of A_j if and only if A_i is ranked ahead of A_i in the ordering P.

All the restrictions that we have considered so far are neutral restrictions on individual preferences. An example of a nonneutral restriction on individual preferences would be that the assembly A be ranked first

A neutral nonstrategic voting rule for a voter with k votes is a function $h: \Gamma(\mathcal{A}_m) \to C_k$ such that $f(P) = C_k$ implies $f(P') = C_k'$ where C_k is the set of all subsets of size k of the set N of candidates, C_k is an element of C_k , and C_k' is the permuted element.

Theorem 4 will indicate that no matter what neutral restriction on individual preferences is imposed, an assembly that is nearly inefficient may be chosen by a constant scoring system.

Assumption 3. The size of the assembly is $m \ge 2$, the number of candidates is $n \ge 4$ and the number of voters is $v \ge min$ (3, m). All voters use the same neutral nonstrategic voting rule.

THEOREM 4. Consider any neutral restriction on strict individual preferences. Under assumption 3, there exist

preference profiles in which all individual orderings satisfy this restriction, an assembly A is elected, and all but one or two voters prefer an assembly B.

DESIGNATED POSTS

Theorem 1 was stated for at-large assemblies. With suitable modifications, the theorem extends to the case of districted and numbered-post elections. For these assemblies, too, candidate-based elections may produce inefficient assemblies or governments.¹³

Designated-post assemblies are of special interest because they are so common. These assemblies may have two very distinct characters. The designated-posts may be different seats in a legislature, or they may be different offices in a government. One might thus characterize the president of the United States and the members of both houses of Congress as a designated-post assembly with the president elected at-large and senators and representatives elected by different districted procedures.

We now turn to assumptions that guarantee the election of efficient designated-post assemblies. As the ensuing discussion indicates, these assumptions may be more plausible than the assumption of blockness used earlier.

In a numbered-post election, suppose a voter assigns an order of importance to the posts (e.g., the mayoralty is most important, followed by district attorney, and so forth).

DEFINITION 6. Let A and A' be two assemblies, with the posts listed in declining order of importance. Suppose that the two assemblies first differ in the jth position. Then preferences are said to be top-lexicographic if an individual always prefers A to A' when the jth candidate in A is the individual's most preferred candidate for post j.

Assumption 4. Preferences are separable. Each voter gets one vote per post and votes simply for his or her top candidate for each post.

THEOREM 5. Suppose each individual agrees on the order of importance of the posts in a numbered-post assembly and that preferences are top-lexicographic. Then under assumption 4, a Pareto-optimal assembly is always elected.

For simplicity, our analysis of districted procedures considers only those elections in which each individual votes in a single district. In this context, an individual's preferences are 1-block if there is a district and a candidate in that district, such that the individual prefers any assembly with that candidate to any assembly without that candidate.

THEOREM 6. If each individual votes in a single district and has 1-block preferences with respect to that district, then a Pareto optimal assembly will be elected.

If voters in a numbered-post election have top-lexicographic preferences but disagree on the order of importance of the posts, then an inefficient assembly may be elected. Similarly, if voters in a districted election have 1-block preferences but not with respect to the district in which they vote, then an inefficient assembly may be elected.

The assumptions on preferences in theorems 5 and 6 are both nonneutral, because they are tied to specific posts. In a numbered-post assembly where the different posts have different titles and responsibilities, a nonneutral assumption may be reasonable. If the differently numbered posts are identical in duties and numbered merely for electoral purposes, a nonneutral assumption is unwarranted. In such a case, there are neutral assumptions that will guarantee the election of an efficient assembly.14 However, this efficiency can only be assured with respect to those assemblies which can be formed respecting the posts for which each candidate has declared. It cannot be guaranteed with respect to the at-large assemblies that could be formed. In the neutral case, this broader notion of efficiency would seem appropriate.

The nonneutrality in the assumption of theorem 6 may be warranted when voters elect local representatives whose responsibilities are primarily to their district. Again, if all representatives have identical duties, a nonneutral assumption is not justified. In districted elections, no neutral assumption can guarantee the election of a Pareto-optimal assembly, even among the restricted set of assemblies.

When there exists a Condorcet winning assembly in a numbered-post election with only two candidates per post (e.g., a Democrat and a Republican), each candidate in this assembly is a majority-rule winner in his or her individual post election (Kadane 1972). Thus, majority rule will produce this Condorcet winner. In a districted election, however, majority rule may fail to produce a Condorcet winner (see Benoit and Kornhauser 1991).

CONCLUSION

Jurisdictions generally adopt candidate-based procedures—in particular, constant scoring rules—to elect assemblies. This common practice poses a problem, since such procedures do not possess minimal efficiency properties; the elected assembly may be nearly inefficient.

These results suggest two types of inquiry. First, one might design and study fully assembly-based procedures for the election of assemblies. This task faces both practical and theoretical problems. Consider, for example, three different assembly-based procedures for an at-large election with 10 candidates running for five seats. In procedure 1, each of the 252 possible assemblies are listed on the ballot and the assembly that receives the most votes is elected. Procedure 1 presents practical difficulties. In procedure 2, each voter marks five candidates on the ballot, ballots are counted as groups of five, and the group that receives the most votes is elected. Under this procedure (as under procedure 1), the winning assembly might garner a very small percentage of the

ballots cast and thus appear undesirable. Voting for candidates, of course, only gives the illusion of performing well in this respect. Although the winning candidates may receive many votes, the resultant assembly may well have received no votes in a direct assembly election. In procedure 3, the 10 candidates divide into two slates of 5, between which the voters choose by majority vote. Here, the electorate might all prefer some other group of 5 candidates as the assembly. Finally, note that while any procedure in which individuals vote for candidates will be dominated by some assembly-based procedure, there is no presumption that a particular assembly-based procedure is superior to a particular candidate-based procedure.

A second inquiry pursues further the investigation of candidate-based procedures. While no ordinal restriction on individual preferences can justify the use of candidate-based procedures, one might identify restrictions across preferences that assure the good performance of specific procedures. Alternately, one might conjecture that individuals do not perceive much difference between many of the assemblies, so that candidate-based procedures do not perform too badly.

Finally, we underscore our initial observation. Any discussion of preference restrictions must be made on assembly preferences, and the selected assembly must be judged with respect to these assembly preferences. To evaluate an election method, one must start with the recognition that preferences over assemblies, not candidates, are fundamental.

APPENDIX: PROOFS

Proof of Proposition 2. Suppose the two candidates enter at positions x, $y \ne 0$ and suppose y is elected. This is not an equilibrium. If x enters at position 0, instead, two-thirds of the voters will switch and vote for x, since this will be a dominant strategy for them. The unique subgame perfect equilibrium in undominated strategies is for both candidates to enter at 0. Voters then vote at random and each candidate has a 50% chance of being elected.

Proof of Theorem 1. The proof consists of constructing preference profiles such that the assembly $\{a_1, a_2, \ldots, a_{m-1}, a_m\}$ gets elected even though everyone prefers $\{a_1, a_2, \ldots, a_{m-2}, a_{m+1}, a_{m+2}\}$. Assume that all preferences are separable. The proof considers two cases:

Case 1. k < m. Let the population be divided into m equal-sized groups (plus or minus 1, since all groups must be integral numbers) with generated candidate rankings as shown in Table A-1. Note that for k < m, candidates $a_1 - a_m$ each receive at least one vote, and no other candidate receives any votes. Therefore the assembly $\{a_1, a_2, \ldots, a_m\}$ is chosen. However, we can assume that everyone would like to replace $\{a_{m-1}, a_m\}$ by $\{a_{m+1}, a_{m+2}\}$; that is, these candidate rankings are consistent with assembly preferences

IADLE A-I			
Example of m	Groups	Ranking A	V Candidates

			GROUP		
RANK	1	2		m-1	m
1	a ₁	a ₂		<i>a</i> _{m-1}	a
2	-				
m – 2*					
m - 1	a_m	a_m		a_1	a_1
m	a_{m+1}	a_{m+1}		a_{m+1}	a _{m+1}
m+1	a_{m+2}	a_{m+2}		a_{m+2}	a_{m+2}
m + 2	a _n	a _n		a_n	a_n
	{tne	remaining	candidat	es}	**
					^^
n	a _{m-1}	a_{m-1}	*	a_m	a_{m-1}

*In positions 2 through m-2, the first m-2 groups rank candidates from a_1 through a_{m-2} arbitrarily (making sure no one lists the same candidate twice, of course). Groups m-1 and m arbitrarily rank candidates a_2 through a_{m-2} .

**The remaining candidates.

that rank $\{a_1, \ldots, a_{m-2}, a_{m+1}, a_{m+1}\}$ over $\{a_1, \ldots, a_{m-1}, a_m\}$.

Case 2. k = m. Let the population be divided into four groups of two different sizes (plus or minus 1), p and q, p < q, as in Table A-2. Again, $\{a_1, a_2, \ldots, a_m\}$ is elected even though we can assume that everyone would prefer to replace candidates a_{m-1} and a_m with a_{m+1} and a_{m+2} . Q.E.D.

Proof of Theorem 2 (*Reductio*). Suppose that A is elected but B is Pareto-superior. Let $A^* = A/(A \cap B)$ and $B^* = B/(A \cap B)$. Consider any individual. Since the individual likes B at least as much as A, B^* must have at least as many elements that are in the individual's block set as A^* does. Since the individual has k votes, he or she casts at least as many votes among the elements of B^* as among A^* . This is true for all individuals, so the total number of votes received by the members of

TABLE A-2

Example of Four Groups of Two Different Sizes (p and g) Ranking N Candidates

	GROUP SIZE			
RANK	p	p	q	q
1	a ₁	a ₁	a ₁	a ₁
2	a_2	a_2	a_2	a_2
$\dot{m}-2$	 a -		 .a .	
m – 1	a _{m-2} a _{m-1}	a _{m-2} a _m	а _{т-2} а _т	a _{m-2} a _{m-1}
m	a_{m+1}	a_{m+1}	a_{m+2}	a_{m+3}
m+1	a_{m+3}	a_{m+3}	a_{m+3}	a_{m+1}
m + 2	a_{m+2}	a_{m+2}	a_{m+1}	a_{m+2}
m + 3	a_n	a_n	a_n	an
n	 а _т			· · · · · a _m
Note: Group s	size p < q.			

 B^* is at least as great as the total received by the members of A^* . In particular, the maximum number of votes received by an element of B^* must be greater than the minimum number received by an element of A^* (assuming no ties), so that an element of B^* must have been elected, a contradiction. Q.E.D.

Proof of Theorem 3. The proof is virtually identical to the proofs of theorems 1 and 2.

Proof of Theorem **4**. First, it is easy to see that if the voting rule does not involve voting for a subset of a voter's top assembly, an inefficient assembly can be elected. For instance, if k = m and the preferences of all votes are identical, an assembly different from the voters' unanimous first choice will be elected. Therefore, assume that the voting rule involves voting for a subset of a voter's top assembly.

Suppose that k = m. The population divides into three groups, and each individual preference ordering satisfies some neutral restriction. Without loss of generality, assume that each individual in the first group has as favorite assembly $\{a_1, a_2\}UB(m-2)$, where B(m-2) is a set of m-2 candidates disjoint from $\{a_1, a_2, a_3, a_4\}$. Furthermore, we can assume that $\{a_1, a_3\} \cup B(m-2)$ is ranked above $\{a_2, a_4\} \cup B(m-2)$. (Suppose, instead, that $\{a_2, a_4\} \cup B(m-2) > \{a_1, a_3\} \cup B(m-2)$. By neutrality [interchanging a_1 and a_2], a group with favorite assembly $\{a_1, a_2\} \cup B(m-2)$ and $\{a_1, a_4\}$ $UB(m-2) > \{a_2, a_3\}UB(m-2)$ also satisfies the restriction. Again by neutrality [interchanging a3 and a_4], a group with favorite assembly $\{a_1, a_2\} \cup B(m-2)$ and $\{a_1, a_3\} \cup B(m-2) > \{a_2, a_4\} \cup B(m-2)$ also satisfies the restriction.) Similarly, we assume that the second group's favorite assembly is $\{a_3, a_4\}UB(m-2)$ and that they all also rank $\{a_1, a_3\} \cup B(m-2)$ above $\{a_2, a_4\} \cup B(m-2)$. Finally, the third group's favorite assembly is $\{a_2, a_4\} \cup B(m-2)$.

We assume that the three groups are, respectively, of size (V-1)/2, (V-1)/2, and 1 if V is odd, and (V-2)/2, (V-2)/2, 2 if V is even. The assembly $\{a_2, a_4\}UB(m-2)$ is elected, but all but one or two people prefer $\{a_1, a_3\}UB(m-2)$.

Now suppose that k < m. The population divides into m groups. The first m-1 groups are of (approximately) equal size. All members of these groups have $\{a_1, a_2, \ldots, a_{m-1}, b_m\}$ as their favorite assembly. Afterward, their preferences differ in such a way that each element of $\{a_1, a_2, \ldots, a_{m-1}\}$ receives at least one vote and only these candidates receive any votes. (This can be done because each voter uses a neutral nonstrategic voting rule.) The last group has one member who votes for a_m and candidates drawn from $\{a_1, a_2, \ldots, a_{m-1}\}$. The assembly $\{a_1, \ldots, a_m\}$ is chosen; but all save one voter (possibly) prefers $\{a_1, \ldots, a_{m-1}, b_m\}$. Q.E.D.

Proof of Theorem 5. Let A be the elected assembly and let A' be a different assembly. Suppose that the two assemblies first differ in position j. The candidate chosen to fill that post must be someone's most preferred candidate for that post, and that individ-

ual prefers A to A', since preferences are top lexicographic. Q.E.D.

Proof of Theorem 6. Theorem 6 is obvious.

Notes

We thank Steven Brams and John Ferejohn for their comments on an earlier draft. While this paper was being written, Benoît spent two weeks at the Instituto Autonomica de Barcelona, whose support he gratefully acknowledges. The technical support of the C. V. Starr Center for Applied Economics is also acknowledged.

1. Separable preferences exhibit a form of independence;

separability is defined formally below.

2. In the United States, school boards and the legislative bodies of many cities are elected at-large. The United States Congress, as well as the legislatures of the states, are elected using districted procedures. The election of the executive in many cities and states uses a numbered-post procedure: each candidate declares which executive post—governor, attorney general, or treasurer—he or she seeks, and then the entire electorate votes on each post.

3. When candidates are running for different posts, $\Gamma(N)$ is the set of all possible orderings of the candidates grouped

by their respective posts.

4. For designated-post assemblies, a_i and a_j must contest the same post, and X_{ij} draws its (m-1) candidates from each of the remaining posts.

5. For instance, see the discussion of Table 1. Also see the game-theoretic example, where voters use dominant strate-

gies.

6. See Benoît and Kornhauser 1991, where we also discuss simple voting more fully. Separability is a sufficient but not necessary condition for simple voting. A necessary and sufficient condition is "top-separability."

7. This reasoning is in the spirit of Nash equilibrium analysis. Any assembly, however, could be elected in a Nash

equilibrium.

- 8. If there exists a Condorcet assembly, however, then those candidates also form a Condorcet set; in this case, a Condorcet set is necessarily efficient. As the text implies, the existence of a Condorcet set of candidates does not imply the existence of a Condorcet assembly (Benoît and Kornhauser 1991; Kadane 1972). Note also that even when there is a Condorcet assembly, common voting procedures do not guarantee the election of the Condorcet candidates, and these procedures may still be inefficient.
- 9. The preferences described here model some actual situations reasonably well. For example, from 1964 through 1986, the four resident commissioners of Etowah County, Alabama, were elected by a countywide vote even though each resident commissioner had to reside in his or her district. The county commission, as a whole, allocated money among districts, but each commissioner controlled funds allocated to his or her district. Residents of a district presumably had preferences like those described in the text. The commission is described in *Presley v. Etowah County Commission* 1992.

10. Our discussion abstracts from many of the issues that have prompted most discussions of proportional representation. For example, we ignore problems of implementation as well as problems that arise when voters have heterogeneous preferences.

11. Fishburn 1981 assumes that A is strictly preferred to A' if and only if $|A \cap B(k)| > |A' \cap B(k)|$. This implies blockness and is, in fact, a much stronger assumption. For instance, suppose $B(k) = \{a_1, a_2\}$. Let $A = \{a_1, a_3\}$ and $A' = \{a_2, a_4\}$. Fishburn's definition requires indifference between A and A' but our definition permits A > A', A < A', or A = A'. Fishburn restricts the assembly ordering to one with k+1 indifference classes.

12. Voting in this manner is simple, but it is not the only simple way to vote. Fishburn 1981 shows that under a stronger assumption than blockness, voting in this manner is

a dominant strategy.

13. See Benoît and Kornhauser 1991. The Ostrogorski paradox can be understood as a special instance of our inefficiency results for numbered-post assemblies (see Anscombe 1976; Bezembinder and Van Acker 1985; Daudt and Rae 1976; Deb and Kelsey 1987).

14. For instance, if assembly A has more first choices than

assembly B, than A is preferred to B.

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INDIVIDUAL AND CONTEXTUAL VARIATIONS IN POLITICAL CANDIDATE APPRAISAL

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In this note we elaborate on the conditions under which on-line and memory-based strategies of political candidate evaluation can be implemented. We suggest that the structure of information may be an important contextual variable affecting the voter's choice of these strategies. In addition, we propose that citizens with less political sophistication are particularly sensitive to structural differences in the political information environment. We use an experimental design that manipulates the information-processing context to test these ideas. Our results suggest that the context in which information is presented plays a critical role in moderating the influence of individual differences in political sophistication.

Recent theories of vote choice have focused on the role of information in voters' decision making (see, e.g., Popkin 1991). Today's candidate-centered campaigns supply citizens with ample information. When presented with these data, how do voters use them to assess the candidates? Current cognitive models of candidate evaluation suggest that the answer to this question may depend on whether the raw data are processed on-line, as the voter acquires it, or whether the information is stored away, perhaps to be retrieved sometime later (see Lodge, McGraw, and Stroh 1989).

The timing of the voter's production of candidate judgments is the key element differentiating on-line and memory-based modes of processing. If voters begin evaluating candidates at the time of information acquisition, judgments are said to be made on-line. In this mode of processing, each new piece of information that voters receive about a candidate is used to update the judgment, similar to the way in which Fiorina (1981) envisions a "running tally" of partisan evaluations. When voters need to produce an overall judgment about the candidate (whether in the voting booth or in order to answer a "feeling thermometer" question on a national survey), they would simply retrieve the tally without reviewing the information upon which it is based. By contrast, in memory-based judgments, voters have not constructed a running tally; instead, they pull together pieces of information they retrieve from memory, integrating them into an overall evaluation at the time that the judgment needs to be produced. The terminology used to describe these processes is somewhat misleading, because both modes are memory-based (see Hastie and Pennington 1989). However, if processing has been on-line, what is retrieved from memory is the previously constructed summary judgment. With memory-based judgments, on the other hand, this overall evaluation has not been made. Therefore, the voter must retrieve specific items from memory in order to construct it.

The difference in memory retrieval in the two modes leads to a critical prediction. If voters have computed a judgment on-line, then there is no need for them to review the specific information upon which it is based in order to make most decisions. The evaluation has already been constructed and stored in memory, so that there is no need to review the information from which the global summary was abstracted. The voter simply retrieves the summary judgment. Empirically, this translates into a prediction of the relative independence (in a correlational sense) of summary candidate evaluations and the evaluative implications of the specific information that individuals can recall about a candidate. Conversely, if voters have not been engaged in on-line processing, then when asked for a summary assessment, they must construct one at that time, based on the specific information they can recall about the candidate. When the judgment is memory-based, then, we can expect a strong correspondence between the specific information that individuals can recall about a candidate and their general assessments of the candidate.

CONTINGENCIES IN THE PROCESS OF CANDIDATE APPRAISAL

Both motivation and cognitive capacity appear to affect individuals' choices about which processing strategy to employ. If individuals approach an information acquisition situation with the explicit goal of forming an evaluation of someone, then they appear to adopt an on-line strategy (Srull and Wyer 1986). Indeed, on-line processing of information is so pervasive that investigators have had to go to extraordinary lengths to disrupt what appears to be a natural inclination to evaluate other people spontaneously (see Hastie and Park 1986). Motivation, however, may not be sufficient under some circumstances.

Bargh and Thein (1985), for example, suggest that individuals must have adequate processing capacity to perform the information integration activity that is required in the on-line processing mode. If individuals' capacities are overwhelmed, they may not be able to implement an on-line strategy, even if they are specifically motivated to form an impression of another person. If cognitive capacity is overloaded during information acquisition, individuals are not able to construct-and subsequently store-an online evaluation. When these individuals are later asked to make an overall evaluation, they must perforce rely on the relatively "raw" information they were able to encode in order to construct their judgments (i.e., memory-based processing). Under these overload conditions, then, we can expect to see a strong correlation between the information that is recalled and the summary judgments that individuals generate.

Both motivation and ability to process may be influenced by individual differences. For example, Bargh and Thein (1985) found that for individuals with "efficient processing structures," information overload did not appear to affect their ability to implement an on-line strategy. McGraw, Lodge, and Stroh (1990) obtained similar results in their study of political candidate evaluation. Political sophisticates were much more likely to process information online, a finding that they attribute to political sophisticates' more efficient processing capacities.

We shall contribute to the growing recognition of the heterogeneity in information processing by suggesting that the information context may also influence individuals' ability to implement an on-line appraisal strategy. We present some results from an experiment designed to investigate the role of context in candidate evaluation processes. Our results suggest that the structure of the political environment may compensate for individual differences in cognitive resources under some circumstances.

CONTEXT AND CANDIDATE EVALUATION

Scholars have recognized that variations in the quantity and quality of information available affect how voters make up their minds (see, e.g., Bartels 1988; Popkin 1991; Westlye 1991). A more subtle constraint imposed by the context of candidate evaluation may inhere in the structure of information. Some information environments are organized rather simply. For example, a presidential nominee's acceptance speech or a candidate "infomercial" is a simple information structure, at least in terms of forming an evaluation of that candidate. While a lot of information may be presented at one time, the structure is nonetheless highly person-centered. By contrast, a debate between candidates is a more complex setting. It features two or more individuals, each of whom presents information on a number of different dimensions, usually in some alternating format. Early televised debates among the contenders for the 1992 Democratic nomination, for example, included as many as five participants talking about issues that ranged from nuclear power to tax cuts.

These two kinds of information structures—person-centered and dimension-centered—capture a wide variety of political communication settings. Newspapers and news magazines, for example, often present long profiles of the candidates one at a time. Candidates' stump speeches are also person-centered forms of communication. These same candidates, however, will also appear in political debates, and the news media often compare the candidates along specific issue dimensions. These two types of formats are perhaps no better epitomized than by the two vehicles through which Ross Perot introduced himself to the public in the 1992 campaign: "infomercials" and participation in the presidential debates.

We believe that information structure can affect voters' processing strategies by influencing both the motivation to process and the ability to process the information presented. Dimension-centered formats, such as political debates, may attract viewers' attention (all else equal) because of the inherent conflictual nature of such presentations. The proposals by the Bipartisan Commission on Debates to eliminate the journalist panel of questioners from the 1992 presidential debates, for example, were justified largely on the grounds that this change would force the candidates to challenge each other directly, thus making the confrontation more interesting to the viewer. Furthermore, the added complexity of a multipleperson stimulus environment may arouse viewer attention, in part because the requirements of television as a medium necessitate more complex structural features (e.g., frequent cuts from one candidate to the other) in order to capture the ongoing communication in a debate context. The structural features of television (e.g., cuts, zooms, and edits) have been shown to affect viewers' attention involuntarily (see Lang 1990; Thorson, Reeves and Schleuder 1985).

Although person-centered structures may be less involving than debatelike confrontations, we believe they may require less effort and resources to process the information. For the same reasons that debates capture viewers' attention, they may also interfere with other processing tasks if individuals' processing capacities are exceeded. The interference seems the most pronounced when the material is unfamiliar to the viewer (Thorson and Lang 1992). Unfamiliar information requires more capacity to process. This may explain why Just, Crigler, and Wallach (1990) found that levels of learning about a candidate's issue positions were actually lower when this information was conveyed in a political debate than when it was presented in the form of a political commercial. For familiar information, on the other hand, emotional arousal and complicated structural features may enhance processing, because viewers are less bored.

These findings suggest that the relationship between motivation and ability to process may be complex. At high levels of viewer involvement, the resources necessary to attend to the information may compete with the resources necessary for other tasks such as comprehension and information integration that are required in on-line processing. At low levels of involvement, however, viewers may not be as interested in attending to the messages and therefore may not pay enough attention to the information to absorb its implications.

Different types of voters may be affected differently by the structure of information presentation. We hypothesize here that individuals with less political sophistication may be especially affected by contextual features. The politically sophisticated know more than the unsophisticated. This knowledge enables them to handle new political information with greater facility, for any new morsel can be integrated more easily into what is already known. This greater cognitive capacity gives sophisticates more flexibility in their use of cognitive strategies (Fiske, Kinder, and Larter 1983; Showers and Cantor 1985). Given this cognitive dexterity, they should be less constrained by the structure of information imposed on them. For individuals less well versed in politics, on the other hand, following public affairs is more strenuous. Nonsophisticates may find their capacities overwhelmed by the attentional demands of a more complicated, debate-focused presentation format. Thus our central hypothesis is that there is an interaction between the type of information environment and the individual's skills, a contingency that should moderate the importance of sophistication in the use of on-line or memory-based strategies.

RESEARCH DESIGN AND PROCEDURE

In a real campaign, it would be virtually impossible to isolate voters before they have had any exposure to the candidates. Even if we could, we could never measure all the information to which they subsequently would be exposed. We believe, therefore, that an experimental design is necessary to test our hypotheses about information structure. By employing fictitious candidates as stimulus material and manipulating information structure, we are able to have control over the voters' information environment

With the assistance of a local playwright, we crafted a "political campaign" that pitted two candidates against each other in a race for a state legislative seat. Our candidates, Ron Vanderkenning and Larry Drake, were actually professional actors we secured through a local talent agency. We filmed the "candidates" at a university television studio in a setting resembling a public affairs program.

Research participants viewed a 15-minute campaign composed of three distinct types of information: personal background, policy positions, and political party membership. In the personal background component, each man spent almost two minutes providing the audience with details of his upbring-

ing, education, occupation, family life, previous political experience, and religious affiliation. The candidates were evenly matched on each feature. For example, both were married and each had an uppermiddle-class profession.

In the policy portion, each candidate described his position on six issues: family farm policy, education, the business climate and taxes, social service spending, defense spending, and trade policy. Each issue statement lasted from 30 to 45 seconds. Ron's platform was moderately conservative, and Larry's was moderately liberal.

In the third informational component, each candidate paid tribute to his respective political party. In this segment, the candidates extolled the virtues of past party heroes and highlighted established party cleavages but did not mention any specific issues.¹

In the present study, we varied the focus of comparison by manipulating the videotaped debate. In the high-person focus, or candidate-centered condition, subjects viewed Ron's exposition in its entirety before they were introduced to Larry. In the debate version of this manipulation, a dimension-centered presentation was created by having the two candidates take turns delivering information in response to prompting from an off-camera narrator. Importantly, the information conveyed by the candidates was precisely the same in the debate condition as in the candidatecentered condition, but the addition of the narrator's voice and the intercandidate shifts transformed the context from one focused on each candidate into one more centered around a comparison of the various political characteristics of the two men.

In exchange for a cash payment, 126 individuals participated in our study. We recruited participants through advertisements in a daily newspaper. Approximately half of the subjects were undergraduates at a large midwestern university. The age of participants ranged from 18 to 79, with a mean of 26 years. Most of the subjects (75%) were political independents. Of the independents, 16% leaned toward the Republican party, and 37% leaned toward the Democrats. The high degree of independence may be an artifact of how we ascertained partisanship, a single seven-point scale, rather than the conventional three-part National Election Study (NES) question.²

Upon arriving for their appointments, subjects were presented with a letter of instruction to read. They also listened to a tape-recorded version of the letter in order to reinforce the instructions. This letter was printed on official-looking letterhead stationery bearing the name of a fictional public relations firm, BARK Associates, Inc.

Subjects watched the video after receiving the instructions. In the debate condition, the opening shot of the video showed both Ron and Larry seated around a small table. An off-camera voice welcomed the men to "Inside Scoop." The narrator then told the candidates that the audience would like to hear a little bit about their backgrounds and invited Mr. Vanderkenning to go first. The camera then cut to Ron, who provided the biographical information con-

tained in the personal background vignette. After Ron finished, the camera focus shifted to Larry, who then proceeded to share his biography with the audience. This back and forth between the two men was repeated for each of the six policies and the political party segment, with the narrator's voice providing brief transitions between each area. During each of these candidate alternations, the camera came back to the two men seated around the table. In the person-centered condition, the two candidates were never pictured together and a narrator was not used.

Immediately following the presentation, subjects were given eight minutes to complete a distractor test. It contained questions of general knowledge in history, science, geography, and literature. Subjects then filled out a questionnaire testing their memory for the material presented, asked for their evaluations of the candidates, measured their perceptions of the candidates' stands on the issues, surveyed their knowledge of politics, asked for their political predispositions and basic demographic characteristics.

MEASURES

Sophistication. The measurement of political sophistication has been contentious (see, e.g., Luskin 1987; Krosnick 1990). We followed the common practice of building a composite index from several different measures. To form our index, we standardized and then summed four variables: self-reported interest in politics, exposure to television news, behavioral participation in political activities, and the number of correct answers to a five-question political information test. Coefficient alpha for the scale is .62. A median split was used to create two sophistication groups: political sophisticates and nonsophisticates.

Party Identification. Subjects' party identification was measured with a single seven-point scale ranging from strong Democrat to strong Republican.

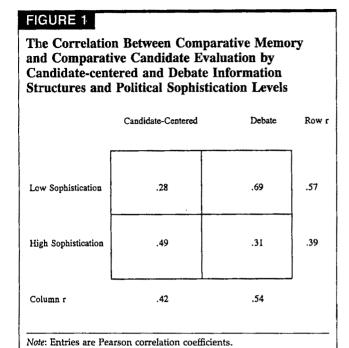
Policy Similarity. We created a measure of policy similarity in order to assess the degree to which a subject's issue preferences matched our candidates' platforms. Subjects indicated their attitude toward several public policies on seven-point scales. For each issue, subjects were given a policy similarity score. If their attitude fell on the Republican side of the scale, subjects received a score of 1, indicating similarity to Ron. If a subject's attitude rating was closer to Larry's (the Democratic) position, on the other hand, the subject received a similarity score of -1. And if subjects rated their attitude at the middle of the issue scale, at position 4, or did not have an attitude on the issue, they received a similarity score of 0 for that issue. To obtain an overall measure of policy similarity, the policy similarity scores were summed across the five issues areas for which we gathered subjects' attitudes. The resulting measure of policy similarity variable had a range of -5 to 5, with 5 indicating maximum similarity to the Republican, and -5 denoting maximum similarity to the Democratic, candidate.

Comparative Candidate Evaluation. Comparative candidate evaluation is the dependent variable in our regression equation. Overall evaluation of each candidate was assessed by a seven-point semantic differential with anchors of dislike and like. A comparative candidate evaluation variable was constructed by subtracting subjects' rating of Larry on the liking scale from their rating of Ron. The resulting variable ranged from -6 to +6, with higher scores indicating greater liking for Ron.

Comparative Candidate Memory. Subjects were given a free recall task after viewing the video and completing a distractor test. They were told to write down everything they could remember about what they had heard and seen on the video, numbering their thoughts as they went. They were given as much time as they needed to do the task. The next page of the questionnaire instructed them to go back through their entries, indicating whether what they had remembered was positive, negative, or neutral about the candidates. We used positively and negatively valenced recall memory to construct a measure of comparative candidate memory. For each candidate separately, a "net" memory score was obtained by subtracting the number of negative items subjects recalled about the candidate from the number of positive items recalled. If subjects recalled more positive than negative attributes about Ron, for example, their net memory scores for Ron would have been positive. A comparative candidate memory variable was constructed from these two net scores by subtracting the net score for Larry from the net score for Ron. If the net scores for Ron and Larry were the same, the subject's comparative memory score would have been 0. If Larry's net score was higher than Ron's, then the comparative memory variable would have been negative, and if Ron's net score was higher than Larry's, the comparative memory variable would have been positive.

EXPECTATIONS AND RESULTS

To examine our expectations about the interaction of sophistication with information structure, we correlated the measure of comparative candidate memory with the comparative candidate evaluation variable within the four groups of subjects defined by the combination of the two levels of sophistication and the two different information formats. As can be seen in Figure 1, consistent with our reasoning, the correlation is quite a bit stronger for the debate—low sophistication group, indicating greater memory-based processing. Furthermore, the difference between the correlation in the two different structures is much smaller for those individuals defined as high in



sophistication than the corresponding difference for those low in sophistication, also supporting our contention that the low sophisticates would be more sensitive to the different information environments.

To test the statistical significance of these differences, we use a regression-based model. It assumes that comparative candidate evaluation is a function of subjects' partisanship, policy predispositions, and valenced recall memory of the information presented on the videotape. By construction, party identification, policy similarity, and comparative candidate memory should be positively related to the dependent variable. We test our hypothesis by using twoway and three-way interaction terms. The candidatecentered information structure is coded as 0 on a dummy variable and the debate condition as 1. Political sophistication is also a dummy variable, scored 1 for sophisticates and 0 for the nonsophisticates. If sophistication moderates the relationship between memory and judgment, as others contend (McGraw, Lodge, and Stroh 1990), then the coefficient for the interaction of the sophistication dummy variable and the comparative memory variable should be statistically significant and negatively signed. If the debate structure interferes with on-line processing, as we argue, then the coefficient for the interaction of the structure dummy variable and the comparative memory variable should be positively signed, indicating that in the debate condition, the relationship between memory for information and overall candidate judgments is significantly stronger. Finally, if structure and sophistication interact as we hypothesize, then memory-based processing should be significantly greater for the nonsophisticates in the debate condition. If our key hypothesis is correct, then the coefficient for the three-way interaction term formed by multiplying the memory variable by the sophistication and structure dummy variables should be significant and negatively signed. This result would indicate that in the debate condition, the relationship between memory and overall candidate judgments was significantly weaker for political sophisticates.

These expectations produce the following equation:

Comparative candidate evaluation = b_0

- + b_1 (party identification) + b_2 (policy similarity)
- $+ b_3$ (comparative memory)
- $+ b_4$ (sophistication dummy)
- $+ b_5$ (structure dummy)
- $+ b_6$ (sophistication \times structure)
- + b_7 (structure dummy × sophistication dummy)
- $+ b_8$ (memory × structure dummy)
- $+ b_9$ (memory × sophistication dummy)
- + b_{10} (memory × sophistication dummy
 - × structure dummy).

Given our a priori expectations, our key hypothesis tests are one-tailed.

The results of our estimation are displayed in Table 1. Recall memory, other things being equal, appears to bear little on subjects' overall evaluations of the two candidates once partisanship, policy similarity, and the interactions are controlled. Contrary to recent research on on-line processing (McGraw, Lodge, and Stroh 1990), sophistication per se does not seem to influence the strength of the relationship between memory and judgment. The interaction term does not have a significant coefficient and its sign is in the wrong direction. Structure, on the other hand, has the hypothesized impact on the ability to implement an on-line strategy. The significant interaction coefficient is consistent with the interpretation that a debatelike format overloads individuals' capacities, forestalling the kind of integrative activity that is required in an on-line strategy. This forces individuals to rely on the information they can recall at a later time to make their candidate judgments, producing a significant memory-judgment relationship. Finally, the strongly significant three-way interaction supports our central hypothesis handsomely. For the sophisticates, information structure matters little. The resulting memory coefficient in the debate condition is virtually zero (i.e., .02 + .16 + .32 .47 = .03), and in the candidate-centered condition it is just slightly higher (i.e., .02 + .16 = .18). For the nonsophisticates, on the other hand, the candidatecentered condition results in no linkage between memory and judgment (B = .02), an indication that they were able to implement an on-line strategy in

TABLE 1

The Relationship between Comparative Memory and Comparative Candidate Evaluation by Information Structure and Political Sophistication

	*
INDEPENDENT VARIABLES	UNSTANDARDIZED COEFFICIENTS
Party identification	.28* (.16)
Policy similarity	.18* (.09)
Comparative memory	.02 (.11)
Comparative memory × sophistication	.16 (.14)
Comparative memory × structure	.32** (.12)
Comparative memory × structure × sophistication	47** (.17)
Sophistication dummy	68 (.5 6)
Structure dummy	−.18 (.54)
Structure dummy × sophistication dummy	37 (.81)
Adjusted R ² N	.33 104

Note: Structure = 1 if debate, 0 if candidate-centered. Sophistication = 1 for high, 0 for low. Standard errors are in parentheses. The dependent variable is comparative candidate evaluation.

*p < .05.
**p < .01.

the less cognitively demanding person-centered condition. However, the strength of the memory–judgment relationship is considerably higher in the debate condition for the nonsophisticates (.02 + .32 = .34).

Table 1 strongly demonstrates the role of context in the cognitive processing of political information. The candidate-centered condition seems to have encouraged an on-line appraisal strategy among the nonsophisticated. Consistent with our argument about the conditional impact of sophistication, there is no significant relationship between recalled information and summary candidate assessments for the nonsophisticated in the less cognitively demanding environment. Under this condition, in fact, it is experts who appear slightly more dependent on memory for their judgments, suggesting that the candidate-centered format was not as motivating to them as the debate format. The debate format, however, was not conducive to on-line processing among those with less well developed political skills.

CONCLUSION

Political candidate information comes packaged in different ways, particularly as candidates look for ways to exploit new technologies. Presidential candidates in the 1992 campaign were responsible for many information innovations, including videocassette distribution, talk show appearances, and "infomercials." Interestingly, many of these new ways of communicating with voters featured the candidate in a structure that we have been describing as personcentered. Indeed, the Discovery Channel took cues from the candidates when it sponsored a very nontraditional presidential candidate "debate" during the primary season that featured each of the remaining contenders (except the president) giving a 20minute "talking head" presentation. If we are to discover what voters know and when they know it (to borrow the Watergate phrase of former Tennessee senator Howard Baker), it is essential to understand how these different types of information environments affect the processing of information. Our results suggest that information structure does matter, particularly for those voters who are more sensitive to the information-processing demands of the environmental setting. Our results, therefore, may help us understand why candidate commercials have been found to be particularly important sources of information for voters as they consider their choice. Our results also suggest that some contextual tinkering with traditional debate formats may be necessary if they are to be more useful for voter learning about the candidates.

Our results point to the conclusion that citizen differences in political sophistication may be less consequential in some circumstances than political cognition research typically suggests. According to our findings, an on-line type of information-processing strategy is equally available to all types of voters—as it should be, given that we employ it so routinely in everyday life (see Rahn et al. 1990)—but the sophisticated, perhaps because of their greater skills, can implement it even in a cognitively demanding environment. Thus the nature of the political environment, through its influence on both motivation to process and ability to process, may be as important as individual differences in understanding the heterogeneity of political information processing.

Notes

A version of this note was presented at the 1991 annual meeting of the Midwest Political Science Association. We are grateful to Susan Klein for her able research assistance and John Richardson for his deft scriptwriting. We also thank John Bruce, Greg Diamond, Jack Dennis, Norman Luttbeg, Richard Merelman, and John Sullivan for their comments on earlier versions. Jon Krosnick provided us with especially helpful insights. The research reported here was funded, in part, by grants from the University of Minnesota Graduate School, Duke University, and the National Science Foundation (SES 92-96046).

1. Transcripts are available upon request.

2. Aldrich and his colleagues (1982) found similar results in their report of experimental question designs in a 1979 NES pilot study. In a split-half design, half of the respondents were asked the usual NES partisanship questions, while the other half were presented with a seven-point-scale format for measuring partisanship, similar to the seven-point-issue-scale design. They found that one in six respondents were "pure" independents when the traditional measure was used but that one in two were "pure" independents (i.e., chose the midpoint, or 4, response) when presented with the seven-pointscale measure of partisanship.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN JUNE, 1994

The following articles, research notes and controversies have been tentatively scheduled for publication in the June 1994 issue:

Courtney Brown. "Politics and the Environment: Nonlinear Instabilities Dominate."

Darrell Dobbs. "Choosing Justice: Socrates' Mythical City and the Practice of Dialectic."

James D. Fearon. "Domestic Political Audiences and the Escalation of International Disputes."

Morris Fiorina. "Divided Government in the American States: An Unintended Consequence of Legislative Professionalism?"

Andrew Gelman and Gary King. "Enhancing Democracy through Legislative Redistricting."

Micheal W. Giles and Kaenan Hertz. "Racial Threat and Partisan Identification."

Ronald Inglehart and Paul R. Abramson. "Economic Security and Value Change."

Lawrence R. Jacobs and Robert Y. Shapiro. "Issues, Candidate Image, and Priming: The Use of Private Polls in Kennedy's 1960 Presidential Campaign."

Donald S. Lutz. "Toward a Theory of Constitutional Amendment."

Richard Nadeau, Richard G. Niemi, and Timothy Amato. "Expectations and Preferences in British General Elections."

Melissa A. Orlie. "Thoughtless Assertion and Political Deliberation."

John E. Roemer. "The Strategic Role of Party Ideology When Voters Are Uncertain about How the Economy Works."

Leslie Paul Theile. "Heidegger on Freedom: Political, Not Metaphysical."

Alexander Wendt. "Collective Identity Formation and the International State."

Arthur H. Miller, Vicki L. Hesli and William M. Reisinger. "Reassessing Mass Support for Political and Economic Change in the Former USSR." A Research Note.

Bruce Western and Simon Jackman. "Bayesian Inference for Comparative Research." A Research Note.

James Johnson; Dana Villa. "Public Sphere, Post-modernism, and Polemic." A Controversy.

ERRATUM

"Electoral Competition in the American States" by Thomas M. Holbrook and Emily Van Dunk, December 1993, pp. 955–62. On p. 956, column 2, the sentence beginning on line 8 should read, "Though there is some disagreement on this matter, we define a safe seat as one that is won by a margin of victory of five or more percentage points."

BOOK REVIEWS

American Political Science Review

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THE NATURE OF LAW TODAY

Hard Cases in Wicked Legal Systems: South African Law in the Perspective of Legal Philosophy. By David Dyzenhaus. New York: Oxford University Press, 1991. 289p. \$69.00.

Natural Law Theory: Contemporary Essays. Edited by Robert P. George. New York: Oxford University

Press, 1992. 371p. \$39.95.

Law and Objectivity. By Kent Greenawalt. New York: Oxford University Press, 1992. 288p. \$45.00. Playing by the Rules: A Philosophical Examination of Rule-Based Decision-Making in Law and Life. By Frederick Schauer. New York: Oxford University Press, 1991. 254p. \$39.95.

Norm and Nature: The Movements of Legal Thought. By Roger Shiner. New York: Oxford University Press, 1992. 348p. \$49.95.

The legal system is a major political institution; and it would be a poor theory of politics, whether normative or descriptive, that lacked an account of the law. Most legal theorists writing in English have thought that any such account will need to accommodate the following truths: that to claim to be governing by law sounds like a good thing, and yet laws and legal systems can be more or less just. One strand of thought, sometimes called natural law, explains it by saying that law is an essentially moral enterprise, of which unjust legal systems are defective specimens—in some cases so defective as to be denied the title law save in some ironic sense. A competing strand, legal positivism, denies that, insisting that there is no necessary connection between law and morality, at least none that guarantees that every full-blooded legal system will have some positive moral worth. Law is just an institutionalized mode of rule application, rules being identified by considerations of social fact and without recourse to moral arguments. Under typical conditions, law is normally a valuable institution; but that—and how valuable it is—is contingent on its content.

Such is the core of one important dispute among legal theorists today. Obviously, natural law theory in this sense need not be committed to the existence of a law of nature; nor need legal positivism deny it. And legal positivism need not be committed to a sociologically "positivistic" account of the nature of social facts: on that point, they can be just as interpretative or hermeneutic as you please. So the terminology is bound to leave some in honest confusion (and provide others with a chance to mislead the sophomore); but we are probably stuck with it—and thus with the natural-law-versus-positivism debate.

The five books considered here aspire to join, or intersect with, that famous debate. Kent Greenawalt and Frederick Schauer treat different aspects of the notion of ruling; the collection edited by Robert P. George and the books by Roger Shiner and David Dyzenhaus examine the relationship between law and morality. It is of interest that a number of these

authors think that the debate is now fruitless, although—characteristically in political theory—they offer conflicting accounts of why that is.

Kent Greenawalt's Law and Objectivity ranges widely over three questions about whether law is, or should be "objective": (1) the problem of determinacy, that is, whether legal rules can, if properly interpreted, uniquely determine the answer to a case; (2) how law treats people, for example, whether it should appraise their behavior by an individualized standard tailored to the situations and capacities of each, by some uniform external standard of what might properly be expected of the reasonable person, or by abstract classifications that are fair and reasonable; and (3) the relationship between law and other standards, such as those of custom, morality, and economics.

Greenawalt is reliably clear and interesting on most of these topics; the first section, in particular, provides a nice summary and critique of the debate around indeterminacy, one that will prove useful to many students. But there is more coherence within his treatment of each topic than there is among them. In part, that is inevitable; for valuable as it is to collect, distinguish, and assess the ways in which law may be called objective, the topics treated here are but homonyms; it is as if one opened a book on banks and found chapters on central banks, snow banks, and river banks. No doubt, when the objectivity of law is praised or criticized, all sorts of things might be in issue; and this clearly written and accessible text touches on most of them. At the same time, it is hard to get to the bottom of any one of these disparate issues in the short compass that a chapter or two

With Frederick Schauer's Playing by the Rules, we have a clear and interesting account of one type of social rules (i.e., prescriptions) that are of special relevance to legal systems. His core idea is that such rules are "entrenched" generalizations—middlerange principles that stand as instruments for achieving their deeper justifications but instruments that inevitably involve some suboptimal decisions. To favor rule-based decision making is thus to be committed to some kinds of error. A justification for such a seemingly conservative procedure is to be given in terms of the benefits of allocating jurisdictional authority to some people, rather than others.

This excellent book shows that a work can be both compact and important. I hesitate, therefore, to note that it leaves out a significant possibility. No doubt some prescriptive rules do have the function of entrenching generalizations as instruments to further certain goals. But is that true of all prescriptions? What, for example, is the underlying justification of the rule "Honour thy father and thy mother"? One possible explanation is that it enjoins children to a certain behavior as a fitting expression of a valued

relationship. From Weber to Wittgenstein, many have suggested that rules may have not only instrumental but also expressive, or communicative, functions. To see them as aiming, indirectly and perhaps imperfectly, at certain underlying justifications is to distort their actual role in practical thought. This suggests that Schauer's account may, in fact, be something other than a general theory of what rules are; for a noninstrumental prescription is still a rule. To say that rules are not ultimate justifications in practical reasoning is not to say that they are always instrumental, although, to be sure, many legal rules are. This may limit the generality of Schauer's account. Still, his work now joins that of David Lyons and Joseph Raz as indispensable treatments of the subject.

Greenawalt's and Schauer's treatments, in their different ways, focus on the first part of the problem: What is so good about ruling by laws? The remaining three books tackle the relationship between law and morality. The oddly conceived volume, Natural Law Theory, offers, as the editor, Robert George, fairly warns, a "remarkable assortment" of theories (p.v). It ranges from Hadley Arkes on Cicero to Jeremy Waldron on metaethics and adjudication. Many of the pieces (those by Joseph Boyle, Robert George, Russel Hittinger, Jeffery Stout, Hadley Arkes, and Lloyd Weinrib) will be of interest only to philosophers working on moral realism or to historians concerned with the classical or Thomistic moral traditions. The social theorist interested in the bearing of these ideas on legal systems will need to turn to the other contributions, some of them excellent, to find out anything about law.

Consider the question of moral realism—the idea that there is a fact of the matter (an "objectivity" if you like) in morality. Contributors John Finnis and Michael Moore have done more than any other contemporary writers to explore the relevance of this for legal theory. Finnis, in his landmark Natural Law and Natural Rights (1981), updated the Aristotelian-Thomistic line in a way that made it join squarely with contemporary social theory. Michael Moore's rejection of positivism begins at a different point, drawing on recent work in the philosophy of language and in metaethics. He makes explicit and rigorous an argument that has been drifting around since Lon Fuller's famous interchanges with the late H. L. A. Hart—that only its purpose, goal, or function makes law what it is; and since it is trivially true that a thing ought to fulfil its proper function, positivists must be wrong to think that there is a difference between law as it is and law as it ought to be.

One might ask what such functionalism has to do with any natural law thesis. After all, "credit card" is a functional kind because all and only those things that have the purpose of providing credit are credit cards. But one can still say whether something is a credit card and identify its credit limit, expiry date, and so on without thinking that all credit cards are morally good and even while thinking that all credit facilities, being usury, are immoral.

Functionalism helps natural-lawyers only if the function of legal systems is a moral one. Moore says, "If law is a functional kind then necessarily law serves some good and thus, necessarily, law is in that way related to morality" (p. 221). But that does not follow. Suppose, as some Marxists think, law is a functional kind whose purpose is to legitimate class domination. Good law is law that does that well and to be a legal system is to have some threshold ability to do that. Does this establish a necessary connection between law and morality? Certainly not. Moore tries to advance the case by saying that for law to be necessarily moral, it must promote some goal that is related to all human goods. Yet he leaves to another occasion the demonstration that there is such a goal and that it distinguishes law from other social insti-

Similarly, Roger Shiner ducks at the very last minute in his *Norm and Nature*. Acknowledging that some functional kinds have nonmoral functions he says, without argument, that law is different, that "the judgement of some system of norms that it is a legal system is at one and the same time a candidate judgement of critical morality that the system fulfils well some moral purpose" (p. 129). Such remarks are puzzling, and I am unsure what Shiner means by a "candidate judgement" (one that might turn out to be false?). But things are even worse than these functionalists imagine, for two reasons.

First, some moral functions of law are compatible with the separation of law and morality. Suppose that the distinctive function of law is to *make justice possible*, by giving people control over their lives and the capacity to coordinate the common activity on which justice depends. Although law therefore necessarily makes justice possible, it is still possible that a legal system does not make justice actual. Indeed, it is consistent with this that *all* actual legal systems are unjust. If that is natural law, then any positivist may accept it.

Second—and worse—law is simply not a functional kind in the requisite sense. Theorists as diverse as Weber, Kelsen, and Pashukanis have all cogently argued that it is impossible to give a nontrivial functional account of law, in part because no function is unique to, or universal among, those forms of social order that we identify as legal systems. Law is in fact a modal kind; it is a specific means of doing things-in Kelsen's language, a "specific social technique." Note that this is not the objection that there is no universal agreement on an end for law. As Moore rightly says, that is beside the point. The question is whether there is a specific function of law, not whether theorists agree about it. But that is where the case fails. There just is no interesting and distinctive function of all legal systems.

In his perceptive and helpful essay in *Natural Law Theory*, Jeremy Waldron sets out the case against the relevance of metaethics to legal theory (a case also made by both Hart and Dworkin). It is a excellent piece for students needing both a reliable sketch of modern debates about the objectivity of ethics and a

compelling argument for their irrelevance to jurisprudence. In brief, Waldron says that our concerns focus not on whether there are, in some sense, objective values but on whether and how these might be identified by courts and guide their judgments. Without an epistemology (and maybe even a methodology), all the talk about ethical objectivity has no cash value. Thus, our political worries about the unpredictability, irrationality, or illegitimacy of judicial decisions are not allayed by adopting moral realism.

At the same time, Waldron dismisses the whole debate about the nature of law. In this respect a good Dworkinian, he thinks that the central problem of legal theory is one of articulating the sort of standards on which judges should base their decisions. These obviously include moral considerations, particularly in countries like the United States, England, and Canada, where some laws direct adjudicators to apply moral judgment in deciding cases and others leave enough indeterminacy in meaning that they cannot sensibly be applied in any other way. Thus, Waldron confidently declares that positivism is "almost certainly false."

Yet all positivists agree with his observations about judicial decision making. They insist that a lot of what goes on on the bench is moralizing, or politics in the broad (and sometimes even the narrow) sense. But they explain this by a theory that allows that law is not everywhere dense, that it has gaps that leave room for discretion. To a positivist, a theory of law and a theory of proper adjudication are different enterprises. Law often gives judges the power to decide whether a certain delay is "unreasonable," whether a wage rate is "fair," whether procedures accord with "fundamental justice," and so on. And these powers may also include a duty to exercise them in accord with certain moral considerations. In other cases, discretion arises from the inherent, though marginal, indeterminacy of every rule. (As Kelsen rightly argued, this is true not merely in hard cases: every case contains both rule application and rule creation.)

Waldron thinks that this reply is just sophistry: "As far as I can tell, the motive behind such moves is to secure a victory . . . for a position called 'legal positivism', no matter what that position turns out to be" (p. 160). But that is not the motive. Positivists are simply seeking an account that is properly sensitive to the different ways in which rule application and rule creation engage our theoretical and practical interests. For example, we tend to hold decision makers more responsible when they have more power to create rules; one who is merely applying rules laid down by others attracts criticism of different kinds.

None of this is a matter of semantics. A view of where to draw the line between law and moralizing, between applying and creating rules, is linked to a whole family of other evaluative and theoretical judgments. Perhaps it is possible to accommodate these within a moralistic view of law. (One might say that judges can be blamed for bad decisions even if the

rules are not of their creation [if things get bad enough, they should break the rules] or that provided they act in good faith, they should not be blamed even for bad moralizing, because the job is a difficult one.) My point is not that there is no other way to get at the substantive issues. That is hardly ever the case in political theory: our idioms are usually both flexible and dispensable. Rather, it is that the distinction between law and discretion has natural roots and cannot be dismissed as a quibble.

In any case, consider the alternative, first proposed by American legal realists like Karl Llewellyn: the law is whatever can be done about a case. If this is right, then not only does law fuse with morality but we cannot even distinguish the law of the United States from the law of Mexico (since conflict-of-laws rules may direct United States courts to apply Mexican law) nor from accounting practice (since courts may be bound to apply this in figuring damages in a civil case)—and so on. Some doctrine of the limits and unity of a legal system therefore appears indispensable.

Waldron is not the only contributor to think the debate superseded. Neil MacCormick (whose Legal Reasoning and Legal Theory remains the best account of its subject matter from a broadly positivist perspective) has been persuaded by reading Finnis that "the issue of the mutual opposition [between positivism and natural law is] . . . now closed and unfruitful. There are elements from works in both schools which any sound theory of law should embrace" (p. 130). He finds that Finnis, at any rate, does not deny the separation thesis and that positivists should accept that both law and morality are species of practical reasoning, that certain moral aspirations (if not achievements) are intrinsic to law, and that the rule of law is, at least prima facie, always of moral value.

In his own essay, Finnis himself says, for example, that the law of England, being a "cultural object, constructed or (as we say) posited by creative human choices, is an instrument, a technique adopted for a moral purpose, and adopted because there is no other available way of agreeing over significant spans of time about precisely how to pursue the moral project well" (p. 141). Thus, legal reasoning is largely technical reasoning, "not moral reasoning," though it is in some ways structured by "moral absolutes" such as the prohibition on intentional killing or deliberate deceptions (p. 142). Finnis now thinks that it is the value of impartiality that drives the law toward the artificial technique of deciding disputes by positive norms identifiable "as far as possible" on the basis of their sources.

While there is something undeniably genial at promise of such a peaceful coexistence between natural law and positivism, others will have nothing of it. The volume concludes with a pair of essays by Joseph Raz and Ernest Weinrib, testing the power of Weinrib's no-compromise, naturalistic essentialism in jurisprudence. Weinrib offers a sketch of his high conceptualist position: a solution of Kant plus Aristotle in Hegel. (This piece is mostly about tort law.

Weinrib sometimes writes as if the nature of law itself can be decoded from certain tort doctrines, rather like making a complete dinosaur from a couple of DNA sequences preserved in amber.) Raz's piece is an effective demolition, though it will be of interest mainly to readers of Weinrib. In a few paragraphs of reply, Weinrib complains of being misread when he writes things like this: "By suggesting that the rationality of law lies in a moral order immanent to legal material, formalism postulates that juridical content can somehow sustain itself from within" (p. 310). My own sympathies lie with those who struggle to construe such passages.

A very different form of antipositivism, one more agreeable to the more empirically or institutionally minded reader, is contained in David Dyzenhaus' book, *Hard Cases in Wicked Legal Systems*. He sets out to test the merits of positivism by considering how it fared in South African legal practice. The book is a rich resource for its illuminating discussions of the style of judicial reasoning that the South African judiciary brought to the apartheid laws and related apparatus of oppression. Unsurprisingly, the judges were not revolutionaries; with a few heroic exceptions, they deferred to an oppressive regime. What is, perhaps, surprising is that Dyzenhaus lays the blame at the door of positivism.

Can the issue finally be resolved by such a pragmatic test? There is a deep-threshold issue that Dyzenhaus does not quite surmount. Legal positivism does not have a distinctive theory of adjudication. It has a theory about the nature of law, according to which judges often have discretion. To decide how such discretion should properly be exercised requires a normative theory of justice and political responsibility. If South African judges failed to do justice, then either this theory failed them, or they failed to act on it. If they were blind to discretionary powers that they, in fact, had, then their theory of law may have failed them; but by the same token, it was surely not a positivist theory.

At best, Dyzenhaus may have shown that certain judicial interpretations of positivism gave rise to bad judgments, primarily through excessive deference to an unjust legislature and executive. There is no guaranteeing that any complex and abstract theory will find itself fairly employed in practice. But this truth is quite general. Dyzenhaus apparently believes that a different theory would, in the South African context, have given rise to better judgments. In particular, he would have preferred the judges to give more weight to a "common law" style of adjudication more attentive to individual rights. I am not sure that "common law reasoning" is, in fact, so sensitive to rights, as opposed, for instance, to some conception of the public good. Nor is it clear that a rhetoric of rights would have made a substantial difference (though here I am only speculating). The evils of the unreformed South African state were probably sufficient to withstand almost any judicial rhetoric. Would it have been safer, for example, for the oppressed African majority if the judges had gone all the way and become natural-lawyers, enforcing in their courts not only the law of the land but the law of nature as they saw it, or if they had become Dworkinians, seeking to extend to hard cases the underlying spirit of law, as they saw it? I am not confident.

Behind this, there is a still-larger question. Should legal theories be accepted or rejected according to the political payoffs of endorsing them in a particular jurisdiction? It seems likely that the consequences of judges' coming to endorse one view or another depends on the local context and political culture. Yet it seems odd to suppose that naturalism is a good account of the nature of law for South Africans but a poor one for, let us say, the French.

The ideas that the debate between natural-lawyers and positivists does not matter, has been superseded, or is settled by history are all very remote from Roger Shiner's approach to the issue. His fascinating book, Norm and Nature: The Movements of Legal Thought, argues that there is deep significance to a feeling that every undergraduate must have had in first considering the modern classics of legal theory: that there is something good to be said on each side yet not enough to promise a final and conclusive argument. Shiner's original and provocative claim is that this is itself the nature of law. He complains that most think of legal theories

as careful mappings, with various degrees of accuracy, of the legal landscape. The model presupposed by this study is, on the contrary *dynamic*—that is, one sees past the different theories to the entities they illuminate, not by seeing the different theories as partial pictures juxtaposed, but as partial theories *interacting* with each other, and by that interaction providing the deeper understanding sought in legal theorizing." (p. 13)

Shiner's thesis is thus metajurisprudential: law is that thing which sets up the dynamic between positivism and antipositivism in legal theory.

This complex idea is more exemplified than defended in his book, as it moves through a discussion of the "simple" positivism (Bentham or Austin), to its more sophisticated variants (Hart and Raz), to the replies of antipositivists (Finnis and Dworkin). Shiner's heart is clearly on the side of the antipositivists, though, at the end, he honestly points up their weaknesses, too. There is much that is worth considering here and much of value in his deft reconstruction of the arguments of the various sides (though it is sometimes hard to think of writers who endorse the composite positions he describes). But most challenging for present purposes is his metajurisprudential thesis: Is it sound?

Since there might have been legal systems and yet not any legal theory, we should refine it somewhat. Let us say that Shiner takes law to be that thing which, when presented to philosophic reflection, calls forth such debates—or (since the writers Shiner considers are mostly English or American) that thing which, when presented to the Anglo-American philosophic mind, calls forth the natural-law-versus-positivism debate. Is that what law is? Is the structure of challenge and response in a theoretical debate like

a piece of litmus paper that tells us something about the nature of thing being debated?

I have no doubt that for almost any x, if asked to choose between a static and a dynamic theory of x, we would all prefer the latter. (Who would want to miss out on the action?) But that does not support Shiner's thesis. We need to notice an important distinction between a theory of the dynamics of x and a dynamic of theories about x. Legal systems are historical entities: laws and legal systems come into being, are enforced, are repealed, fall in disuse, and so on. So any theory of law must cope with the dynamic character of law in this sense. But Shiner has something different in mind. He moves from an observation about the inherent dynamic of theories about law to a conclusion about the nature of law itself. That is a more exotic—and original—claim.

It is also hard to interpret and—I think—to defend. The structure of much theoretical debate in the social studies involves the kind of movement of hypothesis and attempted refutation that Shiner traces. The debate between positivists and their critics resembles many other enduring debates in philosophy, such as those between realists and nominalists, libertarians and determinists, and so on. In all of these cases, we find a dynamic interplay of debate, a shifting and refining of positions, and (often) an enduring sense that no one has got it quite right. Where the disputes are conceptual, rather than empirical, replies and

reformulations are normally forthcoming. Does this suggest further metalevel theses of Shiner's sort?

There is reason to be skeptical. What Shiner needs to show is that it is proper to impute such theoretical (not to say literary) dynamics back to the object level and thus to say, "That is what law is like." And to show that, we would need to be confident that the movements in legal thought reflect only the dynamic of the subject matter. That is doubtful. It seems much more likely that the structure and content of the debate is conditioned by a number of different factors including, for example, political ideologies, professional ambitions, cultural biases, and (of course) chance. The movements of legal thought lie within the province of the historian of ideas. I am not denying that there may sometimes be fully adequate "internal" explanations for growth in any branch of knowledge; but it is obvious that there are also "external" explanations, and I can see no way to disentangle their effects in the present case. That being so, the movements of legal thought may not be a significant guide to the nature of law itself.

So the debate seems here to stay, at least for the moment. That it continues to attract such imaginative and fertile minds is surely encouraging.

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POLITICAL THEORY

Moments of Decision: Political History and the Crises of Radicalism. By Stephen Eric Bronner. New York: Routledge, 1992. 164p. \$49.95 cloth, \$18.95 paper.

With the emergence of the New Left in the 1960s, the recent fall of communism, and the disconcerting rise of religious fundamentalist and ethnic-based movements around the world, it has become something of a truism to speak of a theoretical crisis on the Left. Stephen Eric Bronner, who has given us insightful works on Rosa Luxemburg and on the history of socialist political theory has again offered a thoughtful contribution to this necessary rethinking in Moments of Decision. Lest there be any mistake deriving from the use of the word radicalism in the book's subtitle, Bronner writes from within the tradition of democratic socialism. His goal is to reappropriate this tradition from communist totalitarianism, the Marxist legacy from Marxism-Leninism, and (more generally) the Enlightenment values and aspirations that made socialism possible from the recent postmodern derogation of that heritage.

Though this vantage point is hardly unique today, it is the approach that makes this book uniquely valuable. Bronner seeks nothing less than a radical rethinking of the history of the Left so as to establish a usable legacy for constructing a viable Democratic Socialist movement today. The book's central argument is that the socialist movement took a wrong turn on the eve of World War I, when it reached a fateful fork in the road and split into Communist and Social Democratic movements. Bronner seems to want us to believe that under slightly different circumstances during these crucial moments, a revolutionary but still democratic socialist movement could have survived and prospered. But the book is not an exercise in counterfactual speculation. Bronner recognizes that at key moments in the twentieth century, structural constraints prevented the forces of human emancipation from being realized. By laying out in excruciating detail the relationship between these constraints and the possibilities not realized, the former take on the same contingent character as the human responses to them, and the path not followed begins to look like a real possibility.

It is impossible to do full justice to the subtleties of argument that are woven tightly within a dense, analytical narrative; but some of Bronner's arguments can be briefly summarized. In his account of the Left's response to World War I, he absolves the socialists of responsibility for causing the war. Yet pacifism, too, was an unrealistic response when each country was faced with a looming invasion. Bronner argues that the socialists could have taken a more independent position within their respective governments, including support for the emerging decentralized participatory democracy inherent in the council movement, which survived the war.

The book's strongest arguments are in two chapters narrating the history of attempts to form a popular front of socialists and communists against fascism. Hitler's ascent to power in Germany could have been prevented. It was not an irresistible imposition by the bourgeoisie, nor was it ordained for political economic reasons. Both

socialists and communists made grave misestimates of their opponents, which prevented antifascist collaboration, while Hitler confounded "instrumental reason" with his daring gamble to seize power. Bronner reminds us of the incredible fact that the combined vote of the socialists and communists would have been sufficient to overcome Hitler's plurality.

Turning to France, the book examines with great sensitivity the valiant efforts of Leon Blum to hold back the fascist tide through a coalition of liberals, communists, and socialists, reflecting what Bronner calls "the last great expression of fin de siecle Marxism" (p. 59). Blum is usually dismissed as vacillating, aloof, and elitist; but Bronner convincingly argues that he faced unresolvable political dilemmas, considering the irresponsible role played by both communists and liberals. Though Blum's Popular Front government fell in 1938, its effort to reconcile mass mobilization from below with democratic procedures from above and to establish a European antifascist resistence stand as a legacy that the present can honor and draw upon.

With the discrediting of the parties representing Europe's established order in the fight against fascism, one might have expected that democratic socialism's time had come. But the structural constraints defined by the elimination of the fascist enemy, the rise of the Cold War, and the devastation of Europe which made the American blandishment of the Marshall Plan irresistible, again prevented Western socialism from coming into its own. The Cold War solidified the popular identification of socialism with communist authoritarianism, rehabilitated the Western bourgeoisie's democratic credentials, and left the socialists increasingly aloof from their working-class base as they embraced a strategy of partnership with their capitalists. But Bronner does not view this as a sell-out; for he gives full credit to the European capitalists not only for accepting the liberal welfare state but for establishing their own form of democratic internationalism-both of which gripped the popular imagination far more than the old proletarian international-

The book's odyssey through modern history only arrives in the United States in the 1960s, thereby missing the chance to assess the possibilities in Eugene V. Debs's pre-World-War-I-era Socialist party or the New Deal. This part of the book, however, is the most difficult and problematic for Bronner's project of reconstructing and inspiriting a democratic socialist tradition. He tries to steer a perilous course between the Scylla of poststructuralist endorsement of the particularism and relativism manifested in the fragmented "new social movements" that survived the 1960s and the Charybdis of excoriating the new social movements for abandoning universal values and ignoring the working class. Bronner finds his alternative path in the leadership that Martin Luther King, Jr. sought to provide to the Civil Rights, antiwar, and poor people's movements in the critical moment between 1965 and 1968. Bronner writes that King "objectively represented the most radical socialist possibility in America since the start of the Second World War (pp. 119–20). Though Bronner is sympathetic with the

cultural critique and accomplishments of the 1960s, he emphasizes the unwillingness of new leftists to do the tough intellectual and political work to build a movement to confront capitalist hegemony. The book concludes with a spirited rejoinder to Francis Fukuyama's contention that with the fall of communism and the uncontested rule of liberalism, we have entered a postheroic, posthistorical age.

At times in this book, the reader cries out for a more in-depth portrayal of the historical paths not taken. This is particularly the case with King's leadership, but this book should not be judged on whether his alternatives were realistic possibilities. It is enough that Bronner's historical imagination compels us to pause and reconsider the inevitability of what we know happened, thereby contributing to the task of breaking free of the present's chains.

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RICHARD SCHNEIROV

Virtue Transformed: Political Argument in England, 1688–1740. By Shelley Burtt. New York: Cambridge University Press, 1992. 182p. \$42.95.

One of the more remarkable developments in political theory in recent years has been the rediscovery of what J.G.A. Pocock terms the "Atlantic republican tradition." And central to-indeed, constitutive of-republican political thought is the ideal of the virtuous citizen. Public-spirited, self-abnegating, solicitous of the common good, eschewing wealth, luxury and ease, and prepared to fight for his and others' liberties, the virtuous citizen is a veritable paragon. On Pocock's telling, this republican ideal, as espoused by James Harrington and others, flourished briefly but brilliantly as an opposition ideology but fell victim to the prosperity stemming from English commercial expansion in the late seventeenth and early eighteenth centuries. Except for a few dissidents-Bolingbroke and the Country Whigs, most notably-civic virtue was eclipsed if not erased entirely, and the republican citizen was replaced by the self-interested but "sociable" man of "manners" and

Shelley Burtt attempts to show how "virtue," far from being abandoned, was in important ways transformed in late seventeenth and early eighteenth century England. No brief review can do justice to her intricate, detailed, and well-told tale. But, very roughly, the story she tells is this. "Virtue" represented the moral high ground, and those who (like Mandeville in The Fable of the Bees) dismissed or actively argued against it were themselves dismissed. Every political faction, whether "Court" or "Country," had good reason to lay claim to what was quite clearly a key concept in the political discourse of the day. He whose (re)definition of "virtue" seemed most sensible had an advantage in the rhetorical combat that characterized English politics in this period. But the older ideal of public virtue suffered from a singular liability: it was too restrictive, strenuous and demanding an ideal to have widespread appeal in practice. And yet, at the same time, to envision a Mandevillian world without virtue was well-nigh unthinkable (it remained for later Scotsmen to think the unthinkable). If virtue could not be abandoned, then it must be amended. "Virtue" was accordingly transformed, and from two sides.

On the one side were those "Old" or "Country" Whig ideologists and pamphleteers who engaged in a war of words with the "New" or "Court" Whigs who were allies of and apologists for the Walpole government. The former included such founts of republican rectitude as Trenchard and Gordon's Cato's Letters and Bolingbroke's Crafstman essays. But, Burtt notes, this Country ideology did not simply return to or revive the older ideal of public virtue. Far from being backward-looking or (in Isaac Kramnick's term) "nostalgic," they were hardheaded enough to understand that any ideal of strenuous self-denial was unlikely to have much appeal in a prosperous commercial society. And so they subtly trimmed and qualified "virtue" to make it more palatable, and, in so doing, transformed its meaning in ways and with consequences that they did not always wish or intend. For example, Cato's Letters cleverly intertwined appeals to self-interest with those of civic virtue and attentive citizenship, thereby helping to legitimize the former and transform the latter.

At the same time, and from the other side, Court apologists and ideologists were attempting to counterattack, not by eschewing but by embracing and redefining "virtue" and redescribing the sorts of people who could possess it. The best and most virtuous sort of citizen, on their telling, is one who attends to the public business by minding his own.

Out of these contending arguments came a new, non-public conception of virtue that Burtt calls a "privately oriented civic virtue" which viewed considerations of self-interest as compatible with and complementary to attentive citizenship. One can of course quibble about her choice of phrase-why not a "publicly oriented self-interest" or an "enlightened self-interest" or even "self-interest rightly understood"? But her larger point remains: A supposedly antiquated republican (read "communitarian") politics of virtue was not defeated and replaced by a new and altogether alien "individualist" competitor called liberalism, but was instead transformed in the course of argument and debate, and for good reason. We should not, she concludes, mourn the passing of "public" virtue, nor should we welcome and embrace a more Mandevillian alternative. Perhaps the time is ripe for reconsidering the current liberalism-communitarianism debate in light of this earlier episode.

This is not a book for those who like their history of political thought served up simply and *seriatim*, as a succession of disembodied ideas or hermetically sealed and discontinous "discourses." But it is a book for those who like their histories interesting, riveting, and richly textured.

University of Minnesota

TERENCE BALL

Political Psychology. By Jon Elster. New York: Cambridge University Press, 1993. 204p. \$44.95 cloth, \$14.95 paper.

The Restless Mind: Alexis de Tocqueville on the Origin and Perpetuation of Human Liberty. By Peter Augustine Lawler. Lanham, MD: Rowman & Littlefield, 1993. 200p. \$52.50 cloth, \$21.95 paper.

The rise and fall of totalitarianism and the renewed appeal of democracy have made Tocqueville seem the most prophetic of the nineteenth-century "grand theorists." He predicted that autocratic Russia and libertar-

ian America would each "hold in its hands the destinies of half the world"; but more striking still is his delineation of the underlying forces of modern history: revolutions of rising expectations; egalitarianism leading to communism; mass society turning totalitarian; democracy, if protected against its diseases, restoring and universalizing the liberty lost when centralized monarchy replaced feudalism.

Such prescience has inspired close study of Tocqueville in the past few years alone, including a biography by André Jardin, an interpretive study by Saguiv Hadari, and retrospectives on *Democracy in America* in a collection edited by Abraham Eisenstadt and in books by John Koritansky and Henry Steele Commager. The two volumes under review show that with heightened interest comes more critical scrutiny and more questionable attributions.

Both authors use Tocqueville's work for purposes that he himself did not explicitly pursue. Elster construes him as a behavioral psychologist, turning Tocqueville's aperçus into "psychological mechanisms." Lawler sees him as a moral philosopher. Tocqueville's oeuvre consists of the study on the old regime and the French Revolution, Democracy in America, an uncompleted study of the Revolution, a book of memoirs, and correspondence. The letters contain interesting clues to his own beliefs, as in the quarrel with Gobineau over the latter's infamous Essay on the Inequality of Human Races; but the ensemble hardly amounts either to a systematic account of social psychology or to a philosophical treatise on human nature and morality.

True, Tocqueville announced that a new world needed a new political science, but he used this term rather as the authors of the Federalist had when they dressed up pragmatic arguments with historical examples in support of a new constitution. In discussing Machiavelli, who made a similar pronouncement, John Plamenatz, in Man and Society (1963, I, 3) entered a memorably tart caveat against treating him either as a political philosopher in the traditional sense, "which he is so clearly not," or as a political scientist: "A writer on politics is not scientific merely because he is interested in facts rather than ideals, and rests his advice on what he takes to be the facts." Tocqueville deals with ideals, as well as facts; but the same caution applies.

If he can be neatly characterized at all, it is as a student of historical change and comparative political sociology like Montesquieu, but with a nineteenth-century historicist bent. Compelled to accept the rise of egalitarianism by what he described as "religious dread," he set out to account for the transition to democracy by identifying the universal tendencies that seemed to make it inevitable. He also sought to show that the laws, mores, and institutions of democracy generated patterns of behavior, opinion, and expectation different from those of aristocratic times. Skeptical that any deterministic principle could account for everything or predict more than tendencies, however, he twice notes in his memoirs (echoing Machiavelli) that accident has as much to do with history as necessity and explicitly rejects deterministic theorizing: "For my part, I hate all those absolute systems that make all the events of history depend on great first causes linked together by the chain of fate and thus succeed, so to speak, in banishing men from the history of the human race."

But does his description of American democracy amount to an analysis of behavior in a state of equilib-

rium? Elster devotes two chapters to showing that it does, relying mainly on Democracy in America. (Earlier chapters sift the classicist Paul Veyne's study of Roman patronage and the novelist Alexander Zinoviev's satirical fiction for clues to psychological mechanisms in oligarchical and totalitarian settings.) Mixing praise with criticism, he begins by commending Tocqueville for appreciating that democracy compensates or neutralizes its own excesses. Thus, in politics, bicameralism, indirect elections, and guarantees of individual rights (all constraints foreign to the nature of democracy) are imposed to correct excesses. These prescriptions, however, amount to a "repertory of democracy-stabilizing mechanisms, not a theory of democracy" (p. 105). Otherwise, the analysis suffers from "prejudices and moral clichés" (e.g., biases for aristocracy, monarchy, and masculinity) and from "many ambiguities and even flagrant contradictions" (pp. 107, 112). The indictment presses a lengthy bill of contradictions but is followed by a final chapter praising Tocqueville's "path-breaking analyses of the interaction among desires and opportunities" and setting out to "reconstruct" his thinking to bring out several motivational mechanisms: spillover, compensation, and crowding-out effects (pp. 139-40).

Elster's findings and procedure, while stimulating, are themselves open to question. One of the contradictions he cites is Tocqueville's observing, in volume 1, that newspapers proliferate in America because they are cheap and easy to found and in volume 2, that they "do not multiply because they are cheap" but because groups of people feel a need to communicate with each other. This is, indeed, a contradiction but one that results from the character of Tocqueville's work. He did not set out to write a systematic treatise that would avoid ambiguity and inconsistency. The first point occurred to him while fresh from his visit to the New World. Five years later, immersed in French experience, he evidently realized that the degree of political and administrative fragmentation was, as we might say, a better predictor of why newspapers were more numerous in federal America than in centralized France. Perhaps he should have alerted his readers that he had come to this new understanding, as Elster notes; but rather than tax Tocqueville for inconsistency, we would do better to profit from his second thoughts.

A more serious problem with Elster's approach is that while glossing various texts to compile a catalog of psychological mechanisms, he becomes entangled in an apparent inconsistency of his own. In explaining his examination of observed behavior in three different social settings, he argues that they exhibit a "plurality of mechanisms" at the same time that he endorses rational choice theory, which not only postulates "methodological individualism" but denies that different settings produce different modes of behavior (p. 7). The exegeses provide materials for a sharp critique of this sort of decontextualized theorizing, but no alternative is offered. The book might have addressed or clarified this apparent inconsistency in a closing chapter.

Lawler's Tocqueville is a moral philosopher who introduced a different liberalism, said to be remarkably similar to that of the classical thinkers identified as ancient, rather than modern, liberals by Leo Strauss (p. 101). Lawler seems to mean that while Tocqueville had a Pascalian sense of the human instinct for restlessness and self-doubt, he rejected the dubious solace of bourgeois (modern liberal) hedonism and material-

ism—in Strauss's nice phrase, "the joyless quest for joy." Instead, he advocates a Christian conception of equality and liberty (both restrained by moral law) as the proper foundation of the democratic ideal.

There is much to be said in favor of this reading of Tocqueville's frame of mind, even though he did not write a systematic moral or political philosophy. But Lawler's work has serious flaws. Although he rashly purports to offer the "most comprehensive account" of Tocqueville's life and thought (p. v), he ignores Tocqueville's historical theories and empirical analysis (as well as the details of his marital, religious, and political experience) in order to make him more of a moral philosopher than he was or claimed to be. The presentation also suffers from the Straussian penchant for reading too much meaning into too little text and dismissing whatever is inconvenient as exoteric dross. Although there is good evidence for Tocqueville's affinity with Pascal, for example, he is cited only a few times by Tocqueville, fleetingly, which does not deter Lawler from expatiating on the affinity over some 50 pages. And while he goes into contortions in admitting that Tocqueville "understood himself not to be fortunate enough to have faith" (p. 157), he does not mention that on his death bed Tocqueville stubbornly refused the last rites of the church, leading analysts like Hadari to conclude that he did not believe in God.

These books make clear anew that Tocqueville is worth careful study but remind us of the need to keep in mind what he set out to do and how he did it. He was not a scientific theorist or a systematic philosopher but an extraordinarily gifted enquirer fascinated by history and social and political life. Admirably frank about his biases, he admitted having only a goût de tête for equality but a heartfelt concern for the fate of liberty. What is especially exemplary about his work is that even as he tried to account for the interplay of social forces and free agency, he remained open to possibility and sensitive to the dialectical—even paradoxical—character of thinking and behavior.

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Political Theory and the Displacement of Politics. By Bonnie Honig. Ithaca: Cornell University Press, 1993. 269p. \$35.00 cloth, \$15.95 paper.

Ever since Plato chose to rid his Republic of disruption and dissent, political theorists have attempted to strike a balance between political contestation and the administration of justice. Bonnie Honig's Political Theory and the Displacement of Politics also concerns itself with "negotiating positions" between politics and justice. Honig formulates this negotiation as a confrontation between theories of virtue and theories of virtù. The former is oriented to the promotion of civic virtues best described as hybrids of Aristotelian means and Rawlsian reflective equilibria. The job of the virtue theorist is to chart the oscillations of human interests and reason and to promulgate a set of principles, judgments, laws, and institutions that might fairly accommodate these oscillations and perhaps narrow their arc. On the other hand, theories of virtù are oriented neither to the administration of means nor the coordination of equilibria but to virtuosity, to greatness in striving, and to a celebration of the excesses and unconformities that necessarily accompany such agonism.

Virtue theorists within "republican, liberal, and communitarian" positions, Honig claims, attempt to eliminate all dissonance, resistance, conflict and struggle from their regimes (p. 2). They attempt to wedge the amorphous, polyvalent, and unstable subjectivities of citizens into determinate, stable institutions. Honig accuses them of obscuring and concealing the "remainders" (a term used by Jacques Derrida and Bernard Williams) that writhe and linger after their Procrustean work is done. These remainders might be the "criminal" individuals we would rather ignore, hide away, or blamelessly punish. Alternatively, the misfits might be the agonistic pieces of our own multifaceted souls. In either case, a deconstructive virtù theorizing is proposed as the needed recourse. Despite the obscurantism of many such deconstructive approaches, a straightforward logistics may often be discerned. This logistics is also evident in Honig's work: where there is truth, she uncovers (con)textuality; where there is putative essence, she discloses exclusion; where there is suggested identity, she displays difference; where there is metaphysics, she contaminates with psychology; and where there is consolidation and stability, she exposes subjectification and violence. Like William E. Connolly's Identity\Difference (1991), which offers similar "democratic negotiations of political paradox," Honig proves very much up to her deconstructive task.

Honig's encomium of virtù, however, ends by "renegotiating" the same "positions" she at first had diametrically opposed. The deconstructive moves that Honig executes against the virtue theorizing of Kant, Rawls, and Sandel, are thus also turned upon the virtù theorizing of Arendt and Nietzsche and, eventually, upon Honig's own categories and consolidations. Such selfdestabilizing strategies are de rigeur for postmodernists, who—following Derrida following Nietzsche—generally begin tugging at the loose strands of their own tapestries even while undoing the weavings of others. To put the point in somewhat antiquated, Marxian language: the task of the postmodern theorist is not simply to describe the camera obscura of an opponents' ideology. Rather, the theorist must also disclose his or her own reconstructions as necessarily ideological. In turn, the theorist must, following Foucault, shatter the opposition between ideology and truth itself; for the assumption is not that certain political theories simply invert reality. The claim is that any view we might gain of politics or justice can only be got through the prism of power. Our colored, coherent vision—our theoria—necessarily remains a product of exclusions and violent integrations. Hence, those of us initially astounded at Honig's bold claim that liberalism seeks to suppress all dissonance and resistance in the political realm are subsequently mollified by her later admission that this "position" was an artificial, strategic consolidation that ignored as much as it uncovered. Political Theory and the Displacement of Politics thus ends with a rapprochement. The choice, we are told, is not really administration or disruption, identity or difference. The ecumenical declaration both/and is made. Justice and politics exist as poles of a tension: neither can be experienced or understood apart from the

Still, Honig's balancing act leaves her leaning heavily in the direction of a deconstructive *virtù*. While admit-

ting that Arendt and other virtù theorists cannot escape the consolidations and exclusions so evident in virtue theorists like Rawls and while admitting that Rawls and other virtue theorists never totally displace politics, Honig nonetheless maintains that only virtù theorizing contains within it the "resources" to overcome its own shortcomings (p. 204). In other words, theories tipped in favor of politics are potentially self-correcting in a way that theories tipped in favor of justice are not. The reason appears to be that the self-conscious contextuality of agonistic political theorizing allows compromise without conformity, while architechtonic theories of justice do not. Yet it is unclear why compromise, rather than intensified and perhaps bloody factional conflict, would not be the practical outcome of theories of agonistic politics. One is reminded of William Corlett's Community without Unity (1989), which offers the reader a similarly insightful "politics of Derridean extravagance" without, however, offering sufficient assurance that an agonistic theory of politics would or could stem violence and foster justice.

The world of liberal virtue, Honig admits, is safe-at least for those willing to conform to its rules. How safe is the world of agonal politics? Honig has faith that the nobility of the souls of those engaging their agonism will foster appropriately civil virtues. Neither repressing their own agonism nor resentful of such repression, they might be expected to display compassion and magnanimity. Honig thus approvingly quotes Alan White: "Let us be charitable to Nietzsche as to Aristotle: let us grant that those who are most noble, admirable and selfaffirming will not attempt to exploit others" (p. 230, from White's Within Nietzsche's Labyrinth [1990]). One imagines, however, that Aristotle's numerous slaves, not privy to Honig's and White's faith, might object to such charity. As for Nietzsche, he made it clear enough that his multiple soul allowed not only for magnanimity and compassion but for cruelty without bounds.

Honig's faith in the kindness of agonistic actors is problematic. So is her vague treatment of practical matters as a whole. The goal of virtù politics is to stimulate "practices of citizenship" that proliferate the sites of politics-practices that, identified as neither "state centered" nor "state centering" (p. 255n.), might be understood to refer to social movements and citizen coalitions. Yet social movements and citizen coalitions also seek consolidations of their members' identitieseven if only negatively, in protest to state-centered subjectifications. Such consolidations are often much looser and healthier than statist ones and more in keeping with the needs and concerns of an increasingly global polity. But they are consolidations, nonetheless; and they demand justification. Honig can ignore this, I suspect, only owing to the isolation from grassroots politics granted her within the higher realms of theory.

Whatever the shortcomings of Honig's balancing act, however, one thing is clear. Her strategy of agonistic deconstruction offers immense insight into the troubled hearts and minds of fellow theorists. Honig's brilliant analyses justify neither the tilted position she maintains on the fence nor her unwillingness to dismount into the real world of politics. But this is forgivable. For the vista offered is quite spectacular.

University of Florida

LESLIE PAUL THIELE

Cultural-Political Interventions in the Unfinished Project of Enlightenment. Edited by Axel Honneth, Thomas McCarthy, Claus Offe, and Albrecht Wellmer. Cambridge: MIT Press, 1992. 314p. \$16.95 paper.

In 1989, Surhkamp came out with a hefty volume of 30 essays in celebration of Jürgen Habermas' sixtieth birthday. The table of contents read like a who's who of German philosophy and social theory, as well as its American reception. The English translation of these essays has been published in two separate volumes. The first includes the philosophical contributions, while the second, Cultural-Political Interventions in the Unfinished Project of Enlightenment, brings together the 13 essays that loosely fall under the heading of social theory.

Although Habermas himself does not contribute to this volume and less than half the papers address his work directly, the title is a dead giveaway that the book deals with a certain cluster of issues for which Habermas has become the leading spokesman. Habermas is most famous for defending modernity as being not all bad. In opposition to Frankfurt School pessimism, postmodern skepticism, and neo-Aristotelian disenchantment with disenchantment, Habermas champions the emancipatory potential of Enlightenment rationality. Although modernity brings with it new forms of domination, it also brings with it new potentialities for rational criticism and, through criticism, the opportunity to emancipate ourselves from oppression, injustice, and domination. Habermas has been led into a plethora of research areas in his search to rehabilitate reason: linguistics, ethics, jurisprudence, systems theory, rational action theory, developmental psychology, social evolution, hermeneutics, aesthetics, methodology, democratic theory, legitimation theory. The breadth of his research interests is reflected in the broad range of contributions to this book. There is something here for everyone. However, only those readers who have the same eclectic and encyclopedic interests as Habermas himself will find it easy or captivating from cover to cover.

My own research interests bias me toward the four essays by Helmut Dubiel, Seyla Benhabib, Clause Offe, and Jean Cohen and Andrew Arato. All four take their departure from Habermas' theory of discourse ethics and, as such, form a minority group that addresses Habermas' work directly. Dubiel's essay speaks most directly to the title of the book as he compares the pessimistic view of the Enlightenment as domination (associated with the work of Horkheimer and Adorno) and the optimism of Habermas, which sees an emancipatory potential in the rise of rationalization. Dubiel cautiously comes out in favor of Habermas, correctly pointing out that Habermas' championship of the Enlightenment is not a naive faith in progress but, rather, an elucidation of the contradictory forces of the Enlightenment that allow us to criticize the Enlightenment tradition itself.

Against communitarian ideals of an integrated and harmonious community, Benhabib defends a participatory community. This latter compensates for the disenchantments of modern society not by means of collective notions of good but, rather, by giving citizens control over their lives—a sense of efficacy and political agency. Offe investigates the need for a self-limiting public spirit to motivate citizens if the rational potential of the Enlightenment is to be realized. He explores the types of institutionalized interaction that might promote such a

public spirit. (Some might find his style difficult: his prose is dense, and his sentences often run a hundred words or more.) Cohen and Arato defend a revitalized conception of civil society as the arena for radical democratic politics. The emancipatory potential of the Enlightenment is seen in the development of an autonomous, power-free, self-regulating but also self-limiting civil society structured along the lines of undominated discourse.

Taken together, these essays represent an excellent sample of what might be called the democratic wave of critical theory—theorists who use discourse ethics as a starting point in developing theories of democratic legitimation but go beyond Habermas in their elaboration.

The question of the unfinished project of the Enlightenment can be taken in many more directions than democratic theory. Also represented in this volume are contributions in the areas of legal theory (Günter Frankenberg), history of political thought (Axel Honneth), developmental psychology (Gertrude Nunner-Winkler), and theology (Johann Baptist Metz). Frankenberg takes Habermas' distinction between system and life world into the legal arena to defend a critical legal theory that contains a much harsher indictment of systems theory than Habermas himself is willing to make. Honneth, in a well-written and lucid essay, offers an interpretation of Hegel's early writings that he hopes will shed light on the moral logic of social conflict. Honneth argues that contemporary social theory would do well to follow Hegel in seeing social conflict as a struggle for recognition, rather than competition over scarce resources. Nunner-Winkler challenges Lawrence Kohlberg's thesis that the first stage in moral development is amoral, that is, that children respond to sanctions and rewards and are not motivated by any moral sense. With a great deal of empirical evidence at her disposal, Nunner-Winkler argues that the situation is much more complicated: although not thoroughly moral in their motivations, young children nevertheless appeal to moral rules when justifying their actions. Metz argues in a short and somewhat opaque piece that the current crisis in Christianity as well as the human sciences can only be overcome by anamnestic reason. Only reason that is constituted through historical remembrance "enables enlightenment to enlighten itself again concerning the harm it has done" (p. 191).

Hauke Brunkhorst and Hans-Georg Gadamer both discuss the idea of culture, Brunkhorst by tracing its many variations in the history of social theory and Gadamer by pondering its meaning in relation to mass media. Although full of interesting ideas, Brunkhorst's piece pales by comparison with Gadamer's. An engaging exercise in hermeneutics, Gadamer reflects on the changes and continuity in the meaning of the terms culture and mass media. Although he ends on a very Enlightenment note—that culture involves cultivating "the freedom to make decisions using one's own judgment" (p. 187)—he is not optimistic that the leaps and bounds of mass media technology are conducive to this end. Simple yet full of insights gathered over a long and venerable career, Gadamer's essay is one of the best in the collection and one of the few that requires no specialized knowledge but should be of interest to anyone who has ever wondered how computers, television, interactive media, and virtual reality may shape modern culture.

Although those readers who are looking for depth in

their own research area will find only a fraction of the book interesting, the simple variety of contributions is testimony to the vast influence of Habermas' work. As the preface to the German edition points out, Habermas has been an important figure in shaping the ideas of each of the contributors. This fact would be even more impressive to an American audience if the original 30 essays had been kept together in one volume. But this book is not really about Habermas. It and its companion volume are testimony to the fact that the Enlightenment project is, if not completely well, at least very much alive and the center of some of the most interesting political philosophy today. Postmodernism has been somewhat premature in proclaiming the death of reason.

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SIMONE CHAMBERS

Sharing Responsibility. By Larry May. Chicago: University of Chicago Press, 1992. 204p. \$31.95.

In the years following World War II, many Europeans were profoundly troubled by the failure of their fellow citizens to prevent or protest the actions of the nazis. Philosophers such as Sartre, Jaspers, and Arendt turned to existentialist thought to explain this deeply troubling failure and to design a theory of responsibility that would allow the ascription of collective responsibility to large groups of people who do nothing to prevent such evils from befalling society.

Larry May observes that existentialism is no longer in fashion; but in *Sharing Responsibility*, he develops a theory of responsibility that is inspired by existentialist thought, in particular, "social existentialist" thinkers who emphasize that the self is constructed by social experience, as well as by individual choosing. May anticipates that many readers will be reluctant to take his social existentialist proposals seriously, especially British and American readers for whom, he believes, it is not customary to set the requirements of morality very high. But he is convinced that until people begin to feel responsible for the actions of others, great social tragedies are more likely to occur.

May argues persuasively that it is sometimes possible for people to be responsible for attitudes they hold even when these attitudes are not at a given time fully within their control. Moreover, they can be responsible for harms brought about by holding these attitudes. Thus, when racial violence occurs as the result of the racial attitudes that several people hold, one can hold them responsible for this violence even if they are not fully capable of changing their racial attitudes at the time and even if they themselves do not actually participate in the violence. Along similar lines, people should guard against moral insensitivity. Sometimes harm is produced as the result of a group's insensitivity; and May argues that when this happens, the members of the group share responsibility for the harm.

It is well known that groups have a strong influence over the attitudes and behavior of its members; and May shows that this is true of groups with minimal internal structure, as well as with highly structured groups. Part of this influence takes the form of members' transforming the attitudes and values of one another, and out of this comes a sharing of attitudes and values within groups. Because of this, it makes sense to acknowledge

that there is frequently a sharing of responsibility for harms produced by the actions of such groups.

But it is not enough just to acknowledge a sharing of responsibility for what happens as a result of group actions. Frequently, the real problem comes as a result of group inaction. May concentrates upon cases in which a harm can be prevented if enough people band together into a group to prevent it, and he argues that it is frequently possible to ascribe collective responsibility to the people who have the ability to form such groups and do not do so. For example, groups such as OXFAM have done tremendous good for the victims of famine; and often, it is the simple failure of people to come together that accounts for the continued existence of many grave social problems in the world. To the extent that people have the ability to come together in this manner and do not, they are collectively responsible for their collective inaction.

This is perhaps the point in May's book that poses the sharpest challenge to those who are a product of the dominant Western tradition of morality. To many, it may seem overly severe to judge that all of us are collectively responsible for our failure to do something about the overwhelming number of problems that affect the world. May assures his readers that he does not intend to judge that people who do not form such groups are guilty for their inaction. But he does feel that it is appropriate for them to feel a sense of shame for their inaction. It is shameful for them not to have organized themselves into a group to prevent the harms in question.

It is May's intent to offer an account of responsibility that is provocative in two ways. First, people who are a product of the dominant Western tradition of morality tend to think of agency and responsibility in very individualistic terms and, he argues, to set a very modest level for the amount of responsibility that is assigned to moral agents. May is attempting to challenge this way of thinking. Second, it is his intent to inspire people to think in terms of how they can solve the world's problems by taking collective action. People tend to think that it is only when the problems are of their own making that they have any responsibility to deal with them. But from the social existentialists, we learn that taking these problems collectively upon us is the price we pay for the fact that we live in the human community and that whether we are innocent or guilty of these problems is irrelevant.

Although Sharing Responsibility is intended to provoke and unsettle readers who are comfortable with conventional Western approaches to morality, it is also designed to make a plausible case for the alternative view that May wishes to defend. Even those who are the most hostile to his point of view will find themselves compelled to admit that he does an impressive job of defending it. May deserves high marks for reviving existentialist themes that are quite out of touch with the way that most people, especially British and Americans, think about morality. But he likewise deserves high marks for treating these themes skillfully enough so as to present them as a credible alternative to traditional ways of thinking about morality.

This is not to say that all readers will be *persuaded* by his arguments. Many will come away from this book with the feeling that he has done a valiant but ultimately unsuccessful job of defending views that are so extreme that they cannot be defended persuasively. Neverthe-

less, all readers will be challenged to reevaluate their own ideas about moral responsibility in the context of doing something to help solve the tremendous social problems in today's world.

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GREGORY MELLEMA

The Anxiety of Freedom: Imagination and Individuality in Locke's Political Thought. By Uday Singh Mehta. Ithaca: Cornell University Press, 1992. 186p. \$24.50.

Most Lockean scholars, according to Mehta, focus on the circumstances under which individuals agree to and maintain the establishment of peace, order, and authority. Mehta expands this understanding in arguing that "Locke is concerned not merely with settling the boundaries between individuals . . . but also . . . with settling the internal boundaries of individuals . . . Locke is concerned not merely with individuals' interests but also with their subjective identities" (pp. 3-4). More specifically, Locke's liberalism attains its coherence and stability because it fosters a particular self-understanding. This self-understanding depends on individuals being embedded in liberal institutions, and these in turn "furnish the conditions for a transformed self-understanding—a self-understanding that buttresses political institutions as credible expressions of moral norms" (p. 13). Mehta thus points not only to the crucial role of cultural mores in sustaining liberal society, but also to the possibility that the vaunted natural freedom, equality, and rationality of the individual merely serve as "the gloss, the caption, to a well-manicured set of latent culture dispositions and sensibilities" (p. 18). In its emphasis on education for self-control and moderation, then, "Lockean liberalism, while forming the individual, compromises his or her full potential and thus betrays an underlying conservatism" (p. 11).

Mehta looks to Locke's First Treatise, often neglected, as revealing substantive political concerns with the cognitive dimensions of the Lockean individual. Filmer's doctrines are of course politically absolutist in sanctioning the unlimited exercise of political power. But as an exercise of scriptural politics, they are also methodologically absolutist by their "reference to a body of knowledge that is presumed to be absolutely true and hence authoritative" (p. 46). By sequestering political power from its scriptural origins and the implied legitimacy resulting therefrom, Locke grounds a politics which is historically embedded within a specific time and space. In particular, Locke seeks to bind the conscience so that individual judgments accord with socially required political and moral norms, thus circumscribing "the politically significant potentialities of human subjectivity" (p. 79).

In short, those who assume the foundational or essentialist freedom, equality, and rationality of the Lockean individual overlook the educational and cultural institutions

"through which the individual hopefully comes to be free, rational, and equal in the appropriate manner . . . through which the individual—not the natural individual with reason and interests, but the individual with strange passions, with a frenzied imagination, with undisciplined and chaotic urges—is molded and transformed to have particular passions, an ordered imagination, controlled and well-occasioned urg-

es; in brief, to be rational and self-interested and as a result perhaps also strangely confined" (p. 85).

Mehta perceives in Locke a cautiousness towards unbounded curiosity which one would not expect in an age of reason, especially from a philosopher reputed to celebrate human rationality. In fact, Locke blurs the distinction between the normal and the pathological, so that madness connotes no deficiency, but simply involves the mind "in an overexcited frenzy of activity. Its regal pretensions indicate nothing sinful or sedentary but rather an extravagant imaginative profusion" (p. 107). The mad possess the faculties requisite for reason and freedom. Thus the freedom of "normal" liberal politics is not natural, "but rather a carefully created artifice framed with reference to a particular vision of society and the individuals who inhabit it" (p. 177).

Since for Locke "it is the capacity to reason that is natural, not its actual exercise" (p. 121), Mehta concludes his book with an analysis of how education molds and transforms the individual. First, education inculcates self-restraint and submission to authority in a way that resembles Calvinist self-discipline. Second, the individual internalizes public standards of virtue through the devices of shame and reputation, beginning at a tender age. Finally, the principles inculcated do not encourage an open encounter with the world, but instead encourage individuals to "find their places in it by leaning against the solidity of private dispositions that are no more than the hardened surface of habits instilled in them before they could think, remember, and hence reconsider" (p. 153). Mehta briefly applies these propositions to the Second Treatise, concluding that freedom of the will is "curbed without being openly dominated" (p. 158), so that "the political society made necessary by the subjective potential of the Lockean subject in turn excludes precisely this potential" (p. 167). Alternatively, "individuality is what is valorized in the process of constructing and sequestering normality from what one might loosely call social and individual pathology" (p. 170).

Mehta is right to suggest Lockean scholars have not traditionally concerned themselves with the subjective identities and self-understandings of Lockean individuals, and his book constitutes a definite contribution in this regard. My concerns about the book, however, are two. First, I should like to have seen more detailed consideration of how his conclusions apply to the Second Treatise. Since on his interpretation the individual is not naturally free, equal, or rational, but is instead molded to be such, does this vitiate the meaningfulness of consent as a foundation for political legitimacy? The socially constructed individual will consent only to that which he or she has been taught to approve. As Mehta states, for Locke "the correspondence between negative and positive liberty, and between political freedom and self-mastery, is ultimately untenable because self-mastery or self-control is required as a condition for negative liberty itself" (p. 30). Thus consent loses its meaning as an expression of free will, and one wonders if we can make distinctions regarding political legitimacy as it is affected by choice.

Second, all individuals are to some extent embedded in and molded by the political, social, and moral norms of their society. The crucial question is that of *how much* these norms shape or ought to shape the individual, and that is part of the issue between liberals and communi-

tarians to which Mehta alludes. In discussing Sandel's emphasis on the acknowledgement of constitutive attachments, Mehta states that the Lockean individual, "though being informed and supported by these prenatal horizons, could stand apart from them, critically evaluate them, and, despite the inevitable pain and struggle involved in estranging oneself from one's inheritance, also therefore ultimately reject them" (p. 30). The remainder of Mehta's book, however, renders one highly doubtful that Lockean individuals as Mehta describes them will be able to separate themselves enough from the social fabric in which they are embedded to "think, remember, and hence reconsider" (p. 153). Thus Mehta needs to ask himself under what circumstances these individuals might indeed cast off their socially cultivated self-understandings and espouse new ones. Otherwise, the iron fist of Hobbes remains, though clothed with a velvet glove.

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Political Discourse in Early Modern Britain. Edited by Nicholas Phillipson and Quentin Skinner. New York: Cambridge University Press, 1993. 444p. \$59.95.

This is a *festschrift* for the indefatigable J. G. A. Pocock (indefatigable indeed: the volume closes with a daunting nine-page bibliography of Pocock's work to date, a veritable flood of erudition that shows no signs of ebbing). The essays are better than what usually end up stuck in such volumes: better as a simple matter of scholarly quality, but better too as exemplary models of what is distinctive in Pocock's approach. I suppose that at this price, no one will consider asking impoverished graduate students to purchase the volume. But there are always reserve desks, not to mention xerox machines and copyright violation.

The finicky among us might want to insist on several distinctions between Pocock's conception of the history of political thought and that of what is sometimes labelled the Cambridge school, associated especially with Quentin Skinner and John Dunn. Any such distinctions are generously elided here. The history of political thought, we are instructed, is centrally a history of discourse, a labor-intensive study of how particular languages of politics, or conceptual frameworks, are shaped and reshaped by successive writers. Pocock and his followers have wanted to abandon the struggle of titans, the dubious spectacle sometimes exhibited to hapless undergraduates in which Plato and Hobbes, Locke and Marx take their crack at a set of putatively timeless questions. They want instead a more genuinely historical history of political thought, in which chronology matters, questions change, and dozens of desperately obscure pamphleteers appear right alongside—and sometimes instead of-the honored dead.

So this is not scholarship to be produced by theorists who want to cozy up with their favorite text for a year or two and write an internal textual commentary on it. Here, for instance, J. H. Burns writes about "George Buchanan and the anti-monarchomachs"; Michael Mendle comments in passing that "Some elements of Henry Parker's thought... are reminiscent of Johannes Althusius's populist reversal of Bodinian absolutism" (p. 110); and Richard Tuck refers to Henry Hammond, Matthew Wren, Thomas White, Pierre Charron, Denis

Petau, Henry Holden, John Sargent, Kenelm Digby, and more. It is sobering to realize how very few theorists, even those describing themselves as working on early modern Britain, will have a clue what such titles and comments and references refer to. Here too we find some remarkable—some will say alarming—recontextualizations. Quentin Skinner situates Hobbes's *Leviathan* in the context of a Latin dispute about rhetoric, unearthing an elaborate conceptual inheritance underlying Hobbes's own sizzling rhetoric and his notorious diffidence about his own eloquence.

Skeptics will think that this is not even scholarship to be *consumed* by those without reasonably extensive historical learning of their own, that the density of references to arcane sources is high enough to make any underlying arguments opaque. Maybe, though I suspect that is just special pleading for the lazy. Whatever else one might say about it, Pocock's concluding essay, a critical review of the preceding essays, offers a perfectly straightforward vision of the period, one in which the concept of sovereignty and all its vicissitudes are front and center.

Indeed, for all its contributions, the real problem with this sort of scholarship seems to me to lie in precisely the opposite direction. It needs to become more extensively historical, not less: that is, to pull in more sustained accounts of social and political change, instead of brief stage-setting references that get the contested discourses up and running. Pocock describes the enterprise as "the exploration of Anglo-British history as presented in its political literature and the history of its political discourse" (p. 377). But surely, then, we need to be interested in the possibility that that discourse is opaque, confused, meretricious, shot through with pretexts and sinister interests, ideologically loaded in offering too complacent a vision of political possibilities and problems, happily oblivious to what seem on reflection the most pressing political developments of the day, and

Or again: the very thought of writing a history of discourse depends, for all of Pocock's antimarxism, on the cogency of some deep distinction between the ideal and the material. In this light, it is not surprising that Pocock would refer to the "intellectual and material forces" (p. 428) transforming England after the French Revolution. But if we take seriously the claim that society is partly constituted by concepts and categories, any such distinction is confounding or worse. For then, as Pocock has sometimes recognized, social change is always already conceptual change—and vice versa. (Well, not always: think about demographic shifts or currency inflation.) And then we can sharpen the senses in which all those pamphleteers are creative agents engaged in political struggle—and victims along for the ride in large-scale social changes intended by nobody.

University of Michigan

DON HERZOG

Moral Responsibility and the Boundaries of Community: Power and Accountability from a Pragmatic Point of View. By Marion Smiley. Chicago: University of Chicago Press, 1992. 286p. \$44.50 cloth, \$17.95 paper.

Marion Smiley has written a very interesting book on moral responsibility that is really three books in one. First, she surveys the history of philosophy, examining the differences between classical, Christian and modern conceptions of free will and responsibility. Second, she surveys some recent work in applied ethics and social theory, laboriously reconstructing and criticizing the positions of John Harris, John Casey, Dennis Thompson, Peter Singer, and H. L. A. Hart, as well as many others. Third, Smiley sets out her own pragmatic view of moral responsibility. Each of these tasks is important, and rarely does anyone attempt so much in one volume. Perhaps Smiley should not have done so, either, because her original contribution to the literature on moral responsibility (in chapters 8 and 9) is dwarfed by rather uninspired and lengthy discussions of the work of dozens of philosophers and social theorists, both major and minor, in the majority of the chapters. I shall focus on her original offering.

Smiley presents her main thesis as follows:

I argue throughout what follows that if we want to argue openly with one another about our responsibility for the suffering of others and explain the differences that arise among us, we will have to stop thinking about responsibility as a purely factual discovery and begin thinking about it on the basis of our own social and political points of view. One of these judgments is that harm was the consequence of a particular individual's action. The other was that the individual was herself worthy of blame. (p. 4)

Smiley takes up the first of these points in chapter 8 and the second in chapter 9.

Smiley argues that our assessments of causal responsibility are not "purely objective or independent of our normative expectations of those being blamed" (p. 179). Three specific practical or political questions are shown to be intertwined in the question, Who is causally responsible for a certain harm?: (1) Who has the power to hold another accountable? (2) Who is expected to act in a certain situation? (3) Who is considered to be a member of the relevant community in which this harm occurred? Throughout this chapter, Smiley examines examples in which different answers to these three questions will produce different assessments of who is causally responsible. But she then goes beyond this to claim that we can never transcend such questions to arrive at anything like an objective or scientific understanding of who is causally responsible for a particular

In my view, Smiley has performed the valuable service of pointing out very explicitly how assessments of causal responsibility are often far from being mere factual determinations. She has done a much less credible job, however, in showing that judgments about causal responsibility for harm must be seen as a subspecies of political or ideological judgment. Smiley too often places great emphasis on what some people think. She claims, for instance, that "even if we agree that starvation should be prevented and that we could prevent it, we do not place such expectations on ourselves or hold ourselves causally responsible for the starvation" (p. 193). But surely, there are some other people that do hold themselves causally responsible for starvation in distant parts of the world. Even if no one did, it is a mistake to think that this supports the view that judgments of causal responsibility must be strongly influenced by social and political expectations.

In chapter 9, Smiley argues that our judgments of who is morally blameworthy for a particular harm are similarly a function of our social and political expectations.

Smiley claims that "blameworthiness turns out to be conventional rather than metaphysical" (p. 235). Judgments of whether someone should be excused from blame because compelled to act in a certain way or ignorant of acting in a certain way are understood within particular cultural frameworks that vary quite considerably. Excuses from blame thus turn out not to be objective at all, in Smiley's view. Whether someone is blamed or not is a matter of power: "When we blame individuals we make known to them not only that they have acted badly, but that they are 'bad persons', persons whom we consider to be less than worthy members of our community. Blame is thus, among other things, exclusionary: by blaming individuals for acting badly, we threaten to exclude them from our community of respect" (p. 248).

of respect" (p. 248). Surely, if nothing else, Smiley has overstated her case. Not all—or even most—judgments of blameworthiness are equivalent to judgments that someone is a "bad person"; and while power does sometimes enter into the picture in assessments of blameworthiness, metaphysical and universal moral judgments also enter into the picture. Late in the book, Smiley admits that someone might make a judgment of moral responsibility that is not based in considerations of power, but she considers such cases to be so "rare" as to be barely worth discussion (p. 262). This is a serious flaw in Smiley's project: she fails to take seriously the possibility that while many people do make judgments of blameworthiness so as to exert power over, or exclude, others, nevertheless, some people do not, and many others should not. Hence, she fails to confront opponents who claim that judgments of moral responsibility should be about more than pragmatics.

Smiley is on much firmer ground concerning her weaker thesis, namely, that theorists have ignored the importance of power and convention as ingredients in the judgments of moral responsibility people actually make. Indeed, Smiley provides us with a very nuanced view of the role of power in moral judgment:

For as I have tried to show, the relationship between moral responsibility and power is dialectical in nature. On the one hand, our judgments of moral responsibility for external harm are grounded in, among other things, the distribution of power in our society. On the other hand, these same judgments help to shape our configurations of social roles and our conception of communal boundaries, as well as the distribution of power on which both are based. (p. 257)

This is an original and fascinating view. Marion Smiley's book should be read just so that this view gets wide attention, even if her arguments in support of this view will be unsatisfactory to many of us.

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LARRY MAY

Justice and Interpretation. By Georgia Warnke. Cambridge: MIT Press, 1993. 178p. \$27.50.

In her book Justice and Interpretation, Georgia Warnke embarks on a theoretical endeavor attempting to find a way out of the impasse represented by liberal and communitarian arguments on the issues of justice, tradition, and interpretation. Drawing on the hermeneutic tradition in general and on Hans-Georg Gadamer's ideas in particular, the author proposes a "hermeneutic conversation" to address the conflicting views that charac-

terize democratic societies. Her goal is not to provide solutions in the form of another "original position" or another paradigm of justice, but to suggest a way of addressing our disagreements in order to learn both from each other and from the interpretive diversity of the liberal public sphere.

Her project seems to suggest two important shifts or displacements in the way theoretical and political issues are addressed. The first shift is a move from justice to conversation; the second one is a turning away from the tendency to see our positions as representing the right and true alternative toward a recognition that, in the public realm, we may have an aggregate of views that are equally valid interpretations of the political culture of liberal societies. This claim is the cornerstone of her hermeneutic conversation. These two displacements entail a conception of political theory according to which its task should not be one of adjudicating principles of justice, but of promoting discussion (p. 11).

The author develops her views by offering an intelligent discussion of the arguments put forward by Michael Walzer, Ronald Dworkin, John Rawls, Jurgen Habermas, Charles Taylor, and Alasdair MacIntyre.

Warnke's book is illuminating in the expectations it creates and disappointing in the solutions it offers. Once she announces, at the very outset, that she will present another vision of political theory, one is led to expect some criteria to guide our conversation or some standards to discriminate between the plethora of positions inundating the public sphere. Instead, she ends up with a "hermeneutic political theory" which is not different from John Rawls' version of political liberalism and with a "hermeneutic conversation" that is the same program that Charles Larmore has already defended and that Richard Rorty offers in his liberal utopia. In effect, Warnke's "hermeneutic political theory" "takes its task to be that of interpreting shared social meanings, public values and common political traditions with the aim of formulating a conception of justice that is congruent with these" (p. 130). This is, precisely, the same project John Rawls advocates. Warnke follows the Rawlsian path without considering the moral and political impasse to which the Rawlsian formulation may lead. For if a conception of justice has to be "congruent" with the existing values and traditions, that requirement may represent a straitjacket to any project attempting a transformation of entrenched social meanings and practices. She claims that we should be open to learn from others and that we have to interpret our traditions without adopting a dogmatic perspective. Rather, she argues, we should assume that different valid interpretations may issue from a common history. These claims are hardly new; by now they constitute commonplaces of the liberal discourse.

It is thus unfortunate that a book with an auspicious theoretical agenda ends up in the status quo of present political theorization on the issues of justice and interpretation. Warnke's idea of hermeneutic conversation, all her claims to the contrary notwithstanding, is, essentially, a Rortyan one. She defends the importance of a hermeneutic conversation, and so does Rorty. She advocates "the idea that an interpretive pluralism can be educational for all the parties involved" (p. 157) and so does Rorty. She insists on democracy, equality, and fairness, and though Rorty does not address the issue of equality, his liberal utopia is supposed to be democratic and fair. Furthermore, his position is not necessarily

opposed to the notion of equality to which Warnke subscribes. One is surprised that though Warnke apparently thinks that her views are different from Richard Rorty's ideas, there is no reference to his Contingency, Irony, and Solidarity (1989) in which he proposes arguments that are similar to Warnke's views.

For Warnke, the principles of democracy, equality and fairness provide the framework in which interpretations are going to compete. Hermeneutic conversation seeks to "guarantee a level playing field for all the competing interpretations" (p. 155, emphasis added). It is important, she claims, to assure "the fairness of the conversation" and "to give all possible voices equal access" (p. 157, emphasis added). Presumably, the implication of this view is that our task as interpreters is to try to understand sexists, racists, fascists, and all other doctrines that may be offensive to our sense of human dignity.

Yet this is not Warnke's contention. She is well aware that there are certain views which, far from educating us, will degrade us. There are other views (the Ku Klux Klan's interpretation of American history, for example) that do not resist the historical record. So she seems to advocate the exclusion of certain views from the public domain, but her argument, in this regard, is ambiguous. She first defends a marketplace where all interpretations are allowed to compete with one another (pp. 155, 157). Then, in the conclusion, she says that some interpretations may be excluded (p. 162).

We are at a loss. How could we deal with those views that may degrade us? How could we determine that some views are "ideological" or "pathological" and, accordingly, should be excluded (p. 159)? Warnke's answer is to invoke the Habermasian notion of systematic disturbances in communication whose roots can be found in the social and economic conditions of society (p. 140-157). Habermas' premise is that the deep structures of society may lead people to a false consensus or to being deluded by ideas and practices that are not rational. This is, for him, the insurmountable barrier a hermeneutic approach faces. For hermeneutics concentrates on the people's expressions, their language, while their problem lies in the deep structure shaping their lives. That a psychoanalysis-laden thinker such as Habermas has come up with that notion is understandable. That a person who is trying to offer a hermeneutic interpretation would adhere to the same argument is rather amazing. This may explain why Warnke shifts gears when subscribing to the Habermasian notion of systematically distorted communication and forgets the same standard she has used in her discussion of most of her interlocutor's positions. Again and again, she insists that different people may have different understandings of the same traditions and practices. It is a claim presented against Walzer, Rawls, Dworkin, Habermas, and MacIntyre. But when she invokes the Habermasian view, she does not even consider the possibility that

there might be different and conflicting interpretations of what constitutes a systematically distorted communication. For it is clear that the same structures that, for Habermasians, contribute to a false consensus, for other people may constitute the necessary framework for a meaningful agreement. Warnke does not tell us who is going to determine that some people suffer from a systematically distorted communication and, accordingly, are in need of a Habermasian therapeutic guidance. Nor does she tell us the criteria that the social scientist (presumably the person who knows that others are deluded) will use to make his or her diagnosis. It is perhaps more telling that a scholar on Gadamer has nothing to say about Gadamer's own critique of the psychoanalytic model Habermas presents.

Warnke's text is thus symptomatic of a liberal discourse that seeks to address all moral issues by proposing that we all should adhere to the standards of a liberal conversation. One crucial question that is not addressed by Warnke's hermeneutic conversation is the status of social practices. That is, what are we going to do with the disputed practices which continue to hold sway, while we talk? The book is also representative of a tendency in contemporary liberal discourses to colonize all subjects in dispute by arguing that all parties involved should accept liberal values or rules to continue their conversation. Thus Warnke recommends that the debate on abortion should shift from one over two opposing rights or correct principles to one over two interpretations that are consonant with the traditions of the American society (p. 162). This proposal eschews the issue, which is that some people adhere to values that depend on comprehensive doctrines, and in the light of those doctrines, their position is the "right" one.

One important upshot of Warnke's hermeneutic project is a redefinition of political theory. Its role, she claims, should not be one of deciding which interpretations are better than others (p. 155). Rather, its role should be procedural: to identify the conditions that are necessary for a fair conversation. Warnke seems to be unaware that her proceduralist vision of political theory does not seem to be consonant with the major traditions of liberal societies, and it is rather a recent expression emerging from contemporary liberal discourses.

Unfortunately, Warnke's book does not advance our knowledge of the problems besetting the liberal-communitarian divide. And the solution she offers is by now part of the liberal canon. The claim that we have an interpretive pluralism is a commonplace in the liberal critique of communitarianism, and the view that we should carry out a conversation has been already defended by both Richard Rorty and Charles Larmore. So let's talk. The stalemate continues.

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Roberto Alejandro

AMERICAN POLITICS

Runoff Elections in the United States. By Charles S. Bullock III and Loch K. Johnson. Chapel Hill: University of North Carolina Press, 1992. 206p. \$32.50.

Runoff primary elections are used primarily in the South and in local elections. A runoff primary election follows a primary election in which no candidate receives a majority. Most party primary elections in the United States involve the plurality rule, which means that the candidate winning the highest percentage of votes becomes the party nominee. In runoff elections, if no candidate receives a majority, the two candidates with the highest vote totals then compete in a runoff election. The analysis by Charles Bullock and Loch Johnson focuses on the origins of this system, the frequency of its usage, and several allegations about the impact of the runoff system.

The runoff election system has disputed origins. Some argue that it was instituted in the one-party systems of the South to reduce the fragmentation that emerges in such systems. The requirement of a runoff forces candidates to broaden their appeal to achieve a majority. Others argue that it was established to limit black election success in the South. The percentage of the population that is black is relatively high in the South, and a black candidate might be able to mobilize a bloc of black votes to win in a plurality system. The requirement of achieving a majority may have been to make it difficult for blacks to win primaries.

Regardless of its origins, critics have argued that the dual primary system has very specific effects. The primary concern of Bullock and Johnson is the alleged effects of the runoff system. They investigate four effects: (1) the initial leader is presumed to lose in the runoff, perhaps because of sympathy for the underdog; (2) incumbents who are forced into a runoff are likely to be weak, and incumbents in runoffs tend to lose; (3) women are presumed to be losers in the runoff system, because they cannot create majorities; and (4) blacks tend to be losers in runoffs, because they cannot overcome white reluctance to vote for blacks and cannot create a majority. The two final allegations have drawn the most attention. The book focuses on these alleged effects. The authors characterize these four alleged effects as myths, because it is not known whether any of them are really true. The strength of the book is in sorting them out.

To assess the myths, the authors assemble runoff results for statewide and congressional elections from 1970 through 1986. They also draw on available municipal-level results, though these data receive less attention. For the data covering 1970–86, runoffs were required in about 10% of the races. The analysis focuses on those races where a runoff occurred. They analyze results by office, by state, and across time. They also incorporate information on incumbency and the initial vote proportion of candidates. The analysis reveals that several of the expected effects are, indeed, myths. The central question asked in the analysis is how candidates who lead in a first primary do when they have to enter a runoff. For women and blacks, the question is whether

their success rates in runoffs are less than those of males and whites.

Candidates who lead the first primary do not tend to lose in runoffs. Seventy percent of first round leaders win the runoff. The runoff does not appear to create erratic results. The success rate does decline for governors and when there are more candidates in the first round, but most initial leaders win runoffs. Incumbents who lead in the first round also win about 70% of their runoffs. Those who trailed in the first round are likely to lose in the runoff. While not all incumbents win runoffs, it does not appear to be the case that incumbents forced into a runoff are likely to lose.

With regard to the success of women, they also find that women are not disadvantaged by runoffs. Women who lead the first primary do as well in runoffs as men who lead the first primary. They do find, however, that female success in translating initial leads into success is less for higher offices such as governor or seats in the House. The success rates of women are highest for state legislative office.

The main criticism of the runoff has revolved around its alleged negative effects on the success of blacks. The argument is that blacks can capitalize on black constituencies to do well in an initial primary but that they have a difficult time in runoffs because they cannot overcome racial voting. The argument is that blacks tend to lose runoffs, while they might be able to win plurality primaries.

The allegation that blacks face problems in runoffs does not prove to be a myth. The average success rate for all candidates who lead in the first primary is 70%. Blacks who led the first primary win only 50% of runoffs (p. 114). This holds for elections at different levels. The authors are careful to try and point out that this does not necessarily mean that blacks would do better in a plurality system. Blacks might not do better in a plurality system, because whites, seeking to prevent elections of blacks, might change their strategy in a plurality system. They might seek to mobilize white support around a single white candidate to limit white vote fragmentation and elect white candidates. Their evidence, however, does suggest some qualified support for the allegation that blacks do not do well in runoffs.

The analysis presented by the authors is careful, and the presentation is clear and well presented. The one exception is in the presentation of the arguments about the effects of runoff elections. These assertive arguments are scattered throughout the book, and might better have been combined into one thorough presentation near the beginning. What is most disappointing about the analysis, however, is what is not done. This book neglects some very important questions regarding runoffs that deserve attention if we are to fully understand this selection mechanism.

The essential problem with the analysis is that it focuses only on election results. The arguments about the runoff involve much more than just election outcomes, and the analysis concludes with no information on the dynamics of the dual primary system. Five concerns are neglected. First, how does this system affect the candidate field? Does the dual primary system

attract more candidates than plurality systems? Does it attract more candidates who appeal to specialized blocs? The authors mention these issues, but they conduct no relevant analyses. Second, there is the issue of how candidates position themselves in such a system. Do candidates who are trying just to get to the runoff (not necessarily win the first primary) behave differently than in a plurality system? Is there a process of trying to fragment or polarize the electorate to carve out a faction? Or does the ultimate necessity of building a majority constrain such behavior? Third, is there greater electoral polarization in such a system? This is particularly important in the South, where the black population is a larger percentage of the population than in most states. Do black and white candidates lead to strict racial voting blocs? Fourth, what dynamics occur from first primaries to runoffs, particularly when black candidates are involved? Which voters move from one candidate to another? It would also have been helpful to know what percentage of an election jurisdiction was black when assessing black success in runoffs. Finally, for all of these questions, do candidate, press, and electoral behavior within the dual primary system differ from that in plurality systems?

The authors present thorough analyses of election results. Our understanding of this system is improved by this book. What would have made it far more informative would have been a few careful case studies incorporating information on candidate positioning and polling results across the primary process. This would have provided some answers to the questions indicated. We are given considerable information about results but not much about the process that leads to those results.

Syracuse University

JEFFREY M. STONECASH

Money Talks: Corporate PACs and Political Influence. By Dan Clawson, Alan Neustadtl, and Denise Scott. New York: Basic Books, 1992. 272p. \$25.00.

Money Talks consists of two books. The first is a fascinating and illuminating set of interviews with 20 directors of corporate political action committees (PACs). The second is an analysis of the sources and scope of corporate political power in the United States. I shall review each in turn.

Political scientists have long been puzzled by the lack of any measurable relationship between the patterns of campaign contributions and the behavior of elected officials. This book suggests that they have looked in the wrong places. The impact of campaign contributions lies not on the voting behavior of congressmen and senators but, rather, on what Lowi has termed "distributive politics."

Campaign contributions give firms access to members of Congress, who then influence the ways laws are written in committee or interpreted or enforced by regulatory agencies. Thus, the power of corporate PACs lies in their ability to affect the details of policy, not its broad outlines. And this, in turn, helps explain why corporations, with the exception of a brief period during the early 1980s, give a disproportionate amount of their funds to incumbents: the latter, regardless of their party affiliation or ideology, are in a better position to give companies what they really need from Washington, namely, special help with their unique problems.

The book's extensive quotations from corporate PAC managers offer a revealing portrait of the complex, mutually dependent relationships between PAC managers and legislators. They demonstrate that politicians pressure PAC managers for contributions as much as corporate lobbyists pressure politicians for favors. These interviews are supplemented by analyses of the patterns of donations from 1975 through 1988 by the 309 largest corporate PACs. The result is a useful contemporary portrait of the world of corporate PACs and the executives who manage them.

The second book is an essay on corporate power in America. In contrast to the first, it is driven by ideology, rather than evidence. Its central assertion is that the political contest between business and its opponents (who, however, are never clearly defined) is akin to the battle between a professional and high school football team—a conclusion to which the authors had evidently come before they began their book.

This conclusion is not, in principle, incompatible with the authors' portrait of the strategy and influence of PAC managers. But neither does it logically follow from it. To connect the two, the authors must make a series of leaps that leave their data far behind.

The authors recognize that their argument about business power requires that the interests of corporations complement, rather than contradict, one another. After all, if corporations often took opposing positions, the pluralist model of power in America might be a plausible one. They therefore assert that the interests of business are much more likely to converge, rather than diverge. But they offer as evidence only two facts: corporate PACs rarely contribute to opposing candidates, and the PAC managers they interviewed could only come up with two examples of intrabusiness political differences.

Their argument about the lack of political competition among corporations flies in the face of an enormous literature by political scientists, economists, and students of corporate strategy, whose central finding is that for many corporations, politics has become an important source of competitive advantage. Firms and trade associations regularly use the political process to make themselves better off at the expense of their competitors. Somehow, the authors have managed to miss the fierce interindustry rivalries that dominate so much of the agenda of business—government relations, especially in the area of economic (de)regulation and trade policy.

The analysis of business power in *Money Talks* rests on a second unsupported assumption, namely, that the losses that many businesses experience on the floor of Congress are largely symbolic. According to this study, companies more then make up for their highly visible legislative "defeats" in the influence they are able to exert over the details of legislation or in the way in which laws are implemented. This is an important argument, but the authors offer only anecdotal evidence to support it. They make no effort to look carefully at any particular piece of legislation to show the extent to which its implementation has actually been undermined by politicians acting on behalf of corporations who contributed to their campaigns.

Had the authors done so, they would have discovered that industries do sometimes experience real political losses and that these losses can have important economic consequences. After all, businesses in America spend upwards of a hundred billion a year complying with various environmental laws. This may be less then

they are supposed to spend, but it is nonetheless substantial. Yet *Money Talks* only tells us about the regulations that business does not comply with.

To be persuasive, any analysis of business power must systematically compare the political resources of business with those of other political constituencies. However this book makes no effort to do so. Thus, the authors simply assert, without offering any evidence, that lobbyists and lawyers for environmental groups do not have the expertise or resources to counter the efforts of business to shape the "fine print" of regulatory legislation. And their notion that public acceptance of business exempts corporations from public criticism for the failures of government flies in the face of considerable survey data.

The fundamental problem with *Money Talks* is that it makes a series of arguments about policy outcomes, while its data is all about one (albeit important) dimension of the political process. When the authors stick to their research, their analysis is compelling. But when they go beyond it, their argument looses credibility. *Money Talks* is a well-written empirical study that has unfortunately been overshadowed by the political biases of its authors. They should have written two separate books

University of California, Berkeley

DAVID VOGEL

Renewing Cities. By Ross J. Gittell. Princeton: Princeton University Press, 1992. 232p. \$39.50.

The study of urban politics is in trouble. Strongly influenced by a neo-Marxian perspective, much of the work in the field diverges from the theoretical interests and terms of analysis found in other major subfields of the discipline. Ross Gittell's book shows that he has the skills and the inclinations to integrate theories drawn from institutional economics and other branches of political science to address problems central to the analysis of urban politics.

Gittell's book is a study of economic growth and decline in four medium-sized cities in the "rust belt" of the northeast (Lowell and New Bedford, Massachusetts; Jamestown, New York; and McKeesport, Pennsylvania). Gittell's book is divided into three logically defined parts. Part 1 provides an overview (chapter 1), summarizes and discusses existing theories of urban politics and economic development (chapter 2), and uses techniques of urban economics to develop the base line data to show how far these cities have deviated (either better or worse) from regional trends. Part 2, the longest, consists of four chapters, each one a detailed case study of the trends in, and politics of, economic development in each of the four cities. These case studies are informed by the theoretical discussion of part 1 and offer detailed analysis of the actors, institutions, and political interplay of the forces involved in the economic development in each city. Finally, part 3, the shortest, consisting of two chapters, presents the most interesting theoretical arguments and insights of the entire book and makes me look forward to future theoretical contributions by Gittell. Unfortunately, since Gittell uses an "inductive" approach to theory building (p. 3), the reader is led through a long development process to reach the final stage. Moreover, the book is marred by traces of its origin as a doctoral dissertation.

Chapter 2, "Theories of Local Development," is an example of both the strengths and the weaknesses of Gittell's approach. Gittell summarizes a large number of theories of development, drawn from economics and political science. He divides these approaches into static models, "useful at identifying the influence on local economies of national, regional, and industry changes and competition among localities," and dynamic models, which "provide insights into the internal dynamics of local economies, help explain turning points in local economies, and suggest why local economies diverge from national, regional, and industry trends" (p. 19). Gittell emphasizes the importance of "dynamic cycle theories" developed in economics to highlight tendencies in local development. The most important contribution of this chapter is Gittell's demonstration that politics is central to the process of economic development. His argument ensures the centrality of models of local politics in the study of economic growth, by showing that local politics can significantly influence a city's position in the product and process life cycles that define the broad constraints within which growth occurs.

In developing this argument, Gittell summarizes a wide variety of political theories, including those developed by Kantor, Peterson, Stone, Mollenkopf, and others. Unfortunately, this section reads like the "mandatory" literature reviews found in so many dissertations and betrays the origin of the book; that is, rather than using the work of these analysts to develop new insights or to present a thematic development of the issues of economic development, Gittell presents seriatim a oneor two-paragraph discussion of each author. The writing is often belabored; and it is not until the end of the chapter, where Gittell focuses on collective action problems and the role of individual leaders, that we begin to find new insights, in part because Gittell devotes substantial time and care into integrating the work of Mancur Olson and Robert Axelrod to say something new about local politics.

After presenting the case studies, chapter 8 returns to the theoretical issues underlying the analysis. Gittell summarizes 10 factors that he found to be central to successful local economic development: local leadership, strategic development agencies, institutional arrangements (especially public-private partnerships) that foster new relations more fitting to changing economic reality, local development capacity that lowers the cost of growth, regional analysis, sensitivity to labor force characteristics and dynamics, a broad conception of economic development, a positive city attitude toward development efforts, sensitivity to city history and political and social culture, and recognition that success is not forever and that cities will therefore require continual attention to revitalization (pp. 157–58).

Gittell then turns to the identification of the political factors that are crucial to each of these processes. He touches on the importance of entrepreneurial politicians, analyzes the distributional consequences of various growth strategies, and discusses Peterson's argument that the politics of development is consensual and in the city's interest. In dealing with each of these issues, Gittell emphasizes the importance of entrepreneurial politicians, repeated transactions, and reputations in solving collective-action problems. The analysis in chapter 8 melds insights from public finance, institutional economics, and organizational theory with more established approaches to the study of city development processes.

While Gittell himself recognizes that he offers no new theory (p. 171), the discussion and analysis of chapter 8 begins to show how concepts and theories from new approaches to institutional analysis can be integrated into the study of urban politics. This I believe is the direction in which Gittell's contribution to the field will lie. Unfortunately, this book would have been much better if chapter 8 had taken the place of chapter 2 and if the analysis presented in chapters 3-7 was informed by the more rigorous theoretical statements that Gittell finally develops at the end of book. In other words, while Gittell went through a long process of "induction" to arrive at the theoretical insights with which he concludes his book, the book would have been much improved if Gittell had given us more quickly and more concisely the fruits of his long and careful analysis.

State University of New York, Stony Brook Mark Schneider

The Right To Die: Policy Innovation and Its Consequences. By Henry R. Glick. New York: Columbia University Press, 1992. 238p. \$32.50.

From time to time, the pitiful plight of a hopelessly dying or vegetative patient dramatically calls attention to our vulnerability to a uselessly prolonged life. There was Karen Quinlan in the mid-1970s and Nancy Cruzan a decade later, both in seemingly endless comas and litigation; and there was fully paralyzed Elizabeth Bouvia, who had to go to court to keep from being force-fed forever. As modern medicine develops technology able to keep our bodies alive for months or even years, many wonder whether they can avoid such a fate. Is there a "right to die"? When the Quinlan, Cruzan, and other dramas fade, however, most people give little thought to the right to die: neither the prolongation of a terminal illness nor life as a vegetable make pleasant thinking.

Even those who want to think about the right to die as a public policy question cannot do so easily. The law of death is among the last areas of unfettered state policymaking. Meaningful federal intrusion, while perhaps inevitable, is not likely in the near future. A right to die exists to some degree in 48 states (if you plan to die in Michigan or Nebraska, hope that you go quickly); but no two states have quite the same policy. In some states, the right is very restricted; and even the most facilitative policies leave many situations uncovered.

In The Right To Die, Henry Glick gives us a comprehensive analysis of this emotional and increasingly visible issue. He does an excellent job. Glick records the issue's slow rise from political obscurity, noting that legislators' personal experiences served as the impetus for the first bills and enactments. California adopted a living-will law in 1976; but more often, such bills were defeated. Nonetheless, poll takers put the question on their surveys and began recording a rising and soon overwhelming sentiment in favor of a right to withdraw treatment. Legislation followed popular opinion in the next decade, with 23 states adopting right-to-die laws in 1984-86 alone. By 1991, legislation existed in 42 states (along with favorable judicial rulings in six others). But the adoption process did not always run smoothly: sponsors often had to compromise with state Catholic conferences, the primary opponent of right-to-die laws.

Glick looks at the policy's diffusion across the states,

finding that the standard socioeconomic and urbanism variables do not explain innovation. The key variables are percent Catholic (negative) and the number of relevant state court decisions. The cycles of mass media attention help explain the timing of adoptions. He is unsure that Quinlan's and others' plights directly led to adoptions, although they certainly had an indirect impact. Original right-to-die laws centered upon a living will, but more recent ones are usually centered on health-care proxy laws that allow the designation of another to act on behalf of an incompetent patient. Even now, however, because most states require advance medical directives, Glick estimates that 85% to 90% of us do not currently have a right to die.

Foremost, The Right To Die is a study in public policy. It is set in the agenda-building models of Roger Cobb and John Kingdon and in the diffusion-of-innovations hypotheses that a number of political scientists have developed over the last quarter-century. Glick's research is not driven by a particular theory or school of public policy. In fact, at the book's end, he is somewhat deficient in explaining how his findings contribute to a broader theoretical understanding of policymaking. However, Glick tells us about as much as we need to know concerning the right to die as a political issue. (Given his focus on policymaking, he offers only a minimal discussion of the ethical questions.) Because the right to die is a complex matter, this is not an easy accomplishment. There are several variants of the living will and health-care proxy approaches and even more definitions of terminal illness, brain-dead, and other key terms. The question whether the right to withhold or withdraw life support systems applies only to extraordinary medical techniques or includes hydration and food is quite controversial and often goes unaddressed in statutes. Various combinations of options, definitions, and interpretations make for a myriad of possible policies. Glick sorts them out without getting the reader bogged down in undue detail. I finished The Right To Die feeling that I understood the main practical and legal problems rather well.

While short on a theoretical wrap-up, The Right To Die is analytically rich. Unlike many policy studies, it pays attention to interactions between the branches, especially between the judiciary and legislature. (No governor has advanced a right-to-die proposal, although threatened or actual vetoes occasionally bring governors into the policymaking picture.) Lawmakers unmoved by right-to-die advocates may well respond when court decisions threaten to cut the legislature out of a policymaking role. State high courts will sometimes alter their policies in response to legislative enactments. Glick also realizes that a focus on diffusion patterns alone can miss more than it catches and takes pains to note how later adopters often "reinvented" more facilitative policies. (Thus, California now has one of the most restrictive laws while comparatively recent adopters like Maine and Montana have the most facilitative.) He examines the strategies of major lobbying groups, particularly right-to-die organizations, state Catholic conferences, and state medical associations. But in a nice touch, Glick also looks at those interests that did not lobby, particularly the AARP and other groups of the elderly. These groups are more focused on federal policies, especially benefits, and tend to shy away from the depressing aspects of aging and from issues that may divide their members.

Although Glick does not emphasize this, *The Right To Die* is a comprehensive, cross-state policy study. This in itself is a valuable contribution, because there are so few similar comparative studies in book-length form. There are, of course, a number of comparisons of political processes across the states; and though they are not as frequent as a generation ago, "data in, correlations out" journal articles still relate states' political, social, or economic characteristics to their policy choices. Also, there are case-study comparisons that focus even on a handful of states' policymaking or implementation processes. (Glick has mini-case studies of three states.) But the present book is really the first to combine all these approaches and, more, to compare how all the states have handled an important and rising policy issue.

The Right to Die is a "must" book for those interested in the question or in the broader issues of government regulation of health treatment and the rights of individuals to live autonomous lives. It is also a must for those interested in comparing agenda building, policy choices, and diffusion across the states. For these reasons, it will be on the shelves quite a while, to be replaced, perhaps, by a second edition in the next decade, when right-to-die policy is more fully developed.

University of Kentucky

BRADLEY C. CANON

Hollow Mandates: American Public Opinion and the Conservative Shift. By Howard J. Gold. Boulder: Westview, 1992. 214p. \$35.00.

It would be hard to imagine a more dramatic transformation in American politics than that which occurred between 1964 and 1984. There are probably few more telling illustrations of this change than the fortunes of Ronald Reagan, the era's central figure. In 1964, an electorally inexperienced Reagan went on national television and spoke forcefully in favor of the ill-fated campaign of conservative Barry Goldwater. In 1984, Reagan was busily putting many ideas espoused during the 1964 campaign into public policy and getting reelected president in the process. Few would argue that politics of the 1980s had not changed from the 1960s; yet as striking as this turn right may seem, it is not as simple or enduring as some think. The nature and origins of this shift are the subject of Howard J. Gold's Hollow Mandates, a brief book well suited for advanced undergraduate or introductory graduate courses in public opinion or electoral politics.

As a backdrop for his analysis, Gold presents a useful summary of major changes in ideas, campaign rhetoric, and public policy that occurred in the 1960-80 era. Beginning with a sound, if brief, review of traditional and new conservative ideas that permeated this era, he considers if, when, and how these ideas came to infiltrate electoral politics by examining party nomination addresses and platforms. On the basis of a careful examination of nomination acceptance speeches and party platforms, he detects a fundamental shift to the right in the 1970s and early 1980s. Jimmy Carter's campaign of 1976 saw a major infusion of conservative ideas into Democratic campaign rhetoric. A shift rightward was also evident in the policy proposals (if not the candidate) of the Republican party that year. Gold makes a convincing case that Ronald Reagan's campaign of 1980 followed a conservative trend that had begun at least four years earlier. And while Reagan did not initiate the trend, his election and subsequent reelection translated conservative words into action. *Hollow Mandates* presents a detailed litany of the changes wrought in federal policies during the Reagan years.

Hollow Mandates overriding concern is with a basic and important question: Has the transformation of politics and policy resulted from, or caused, a rightward shift in public opinion? He presents three "theories" to account for the change: (1) a "cultural backlash" against the Great Society programs and upheavals of the 1960s; (2) reactions to the "institutional malaise," scandals, and economic upheavals of the 1970s; and (3) a "sweeping mandate" interpretation emphasizing the role of Ronald Reagan's 1980 election. Each perspective predicts different periods of change in the electorate and suggests differing content for any accompanying attitudinal change.

Examining CPS and NORC survey data, Gold finds changes that are far less remarkable than one might expect. Remarkably, the only major ideological change was a growth in hostility toward federal power. Beside this and growing support for capital punishment in the 1972–88 period, white Americans in the 1980s were not remarkably more conservative than their counterparts in the 1960s. The most notable shift in public opinion was in perceptions of the managerial capabilities of the political parties. Increasingly, the Republican party became viewed as more effective managers, as Democrats suffered on this dimension. The conservative shift in America, Gold argues, was to a de facto conservatism resulting from a deepening public affection for the Republican party but not for its ideology.

According to Gold, America's turn right was a hollow mandate (hence the title). The shift was limited to a managerial conservatism lacking explicit ideology. As long as the Republicans could manage the government (and particularly the economy) effectively, the public would support them and tolerate their conservative social pronouncements. Unfortunately for the Republicans, the publication of *Hollow Mandates* came too late to stop Bush from displaying the truth of many penetrating

insights offered in the book.

The main faults I find with Hollow Mandates are with its rather simplistic research design and methodology and a greatly circumscribed theoretical focus. With only minor exceptions, public opinion is not broken down into subgroups. The sometimes sparse analysis would have been greatly enhanced with attention to geographical, gender, ethnic, racial, and other differences (if any) in public opinion about issues and parties over time. Added to this, with a minor exception in chapter 8, multivariate analysis is sadly lacking. At many important junctures, more sophisticated modeling could have allowed concise evaluation of the explanatory usefulness of the alternative "theories" of change and the examination of generational replacement presented in chapter 7. The "theories," furthermore, are nothing more than descriptions of historical events and, as such, provide little that is generalizable about the forces that may promote change in American politics. In this vein, Gold also does not attempt to push his findings to a general statement of the nature of public opinion in America like that offered in more general studies such as McClosky and Zaller's American Ethos (1984). This is unfortunate, because his findings point to important contradictions in the electorate that color so much of the American political landscape.

These methodological and theoretical limitations notwithstanding, *Hollow Mandates* makes a provocative point and should attract a broad audience. It is an easily accessible book; and students of American politics and public opinion, as well as those interested in the contemporary political milieu, will all find much to commend this interesting study.

Florida State University

PAUL BRACE

Feminist Jurisprudence: The Difference Debate. Edited by Leslie Friedman Goldstein. Savage, MD: Rowman & Littlefield, 1992. 286p. \$65.00 cloth, \$24.95 paper.

"A shared concern with the legal significance of difference, that is, the difference between men and women" unites the contributors to this volume, according to its editor, Leslie Friedman Goldstein (p. 1). The authors are linked in another important respect, as well: they are all feminists with secular values. This is a debate about difference among friends of the women's movement. Their orientation is practical and political.

The book begins with a series of reflections on the first question that anyone concerned about using law for feminist ends must ask: To what extent should law acknowledge the fact that women are differently situated from men, biologically, culturally, and socioeconomically? The issue is how to accommodate aspirations for equality with the contemporary reality of difference. At one time, the feminist debate was between those who would launch a frontal attack on distinctions between men and women in the law and those who sought to retain the few protections the law afforded that were responsive to women's social and economic disadvantages. The contemporary battleground has shifted as law has been cleansed of most of its sexually explicit stereotypes. The issue now is whether the best strategy is sex-specific or sex-neutral.

Joan Williams argues eloquently that law should make no assumptions about men and women but must, instead, attack the roots of sexist oppression, particularly those associated with the workplace. The rules of work in contemporary society continue to be built around a model of the ideal worker who is either unattached or emancipated from family responsibilities by a partner who can take up the slack. Law must be changed to reflect the responsibilities of workers for their dependants. This would involve not just child-care options but paid leaves as needed, flexibility concerning hours and location of work, elimination of all forms of discrimination against part-time work, and other changes. We should insist that these changes be made in a sex-neutral way in order to break down old stereotypes about gender roles. Traditionalist judges can use women's culture against women, as the Sears case illustrates. The dangers of women's embracing domesticity are particularly great at this moment in history as men increasingly abandon their own commitments to supporting the household. Shanley and Battistoni endorse this position in a thoughtful final essay.

Mary Becker takes an opposing position. She is skeptical that this nation will find the determination to change its androcentric ways. We seem ready only for reforms that allow women to live like single men. Even the rare exceptions to neglect—sex-specific protections against the worst disadvantages women suffer—pose

painful choices. Citing the famous California case on unpaid leave for women disabled by pregnancy, Becker argues that we must choose between protection for women and no protection at all. In an environment so hostile to fundamental structural change, sex neutrality is not an option. Law must be explicit in helping women achieve protection from socially imposed discrimination; and it must include affirmative action for women only, to begin to redress the gender-specific disadvantages women have suffered.

The book moves on from this general debate about equality in law to particular issues and arenas of conflict. These subsequent contributions help sensitize the reader to other dimensions of the problem of law as a vehicle for social change. Judith Baer begins this series of essays with an argument directed toward those feminist theorists who dismiss the enterprise of legal reform as hopelessly male in the values and practice it supports. She argues that there is nothing inherently male in the combination of abstract reason, practical compromising, and emotional commitment that together characterize constitutional jurisprudence. She, like Williams, is hostile to relational feminism, with its suggestion that there may be a distinctively female way of knowing that may be inherent or social but that in either event, should be acknowledged and celebrated. Baer does accept that there are certain typically male means of arguing about and interpreting law, but she sees these as artifacts of male power or male confusion about emotional data. She notes, for example, Justice Blackmun's effort to separate his personal distaste for the death penalty from the issue of its constitutionality. While no judge should let emotion dictate decisions, she argues, feelings do serve a purpose in guiding action. She concludes, with a touch of irony, "If integration of reason and emotion is essentially female, then women have much to bring to constitutional interpretation" (p. 155).

Other contributions focus on particular policy problems. Graber uses the contemporary debate on the constitutionality of abortion regulation to make a more general point about legal debate. To discuss constitutional norms or rules in the abstract, without considering the realities of implementation, not only is a sterile way to proceed and irrelevant to ordinary people but risks mistaking the fundamental constitutional norms at stake. If abortion is restricted by law, it will have a differential effect on rich and poor. The constitutional debate, in other words, should not simply revolve around the question of whether privacy is or is not protected by the constitution. Our norm of equality under law is also implicated whenever the state seeks to restrict the availability of abortion.

The book also contains essays on two other areas of hot debate: divorce reform and fetal protection laws. Lisa Bower shows how conceptions of conflict, of marriage, and of woman's interests influenced the recent wave of divorce reform that attempts to eliminate moral condemnation and gender stereotyping from divorce. The results, studies of divorced partners suggest, have not been beneficial for women (although there is little consensus on the dimensions of the problem). Joseph Losco provides an equivalent lesson in humility for those who would abolish laws designed to force expectant mothers to take responsibility for the fetuses they carry.

The book is stimulating because it combines serious attention to the roles of law in society without losing

sight of the problem of developing progressive agenda for women. The authors show convincingly, particularly when they are read together, that there are no easy answers to the problem of law's contribution to women's emancipation. Even Shanely and Battistoni's final call for open debate is subject to a caveat. Graber suggests that the more we realize the social dimensions of some legal propositions, such as abortion regulation, the more entrenched our positions are likely to become. There are basic values at stake here, and nonfeminists oppose the whole enterprise of decoupling sex from gender.

We do not get a sense from this book of just how the nonfeminist majority is likely to respond to the legal initiatives under consideration here. A book that includes only essays written from a feminist persuasion can hardly give the reader a feel for opposing views. It might have been helpful to include a representative of the antifeminist position in a volume on the role of law in dealing with sexual difference.

Syracuse University

DORIS MARIE PROVINE

Latina Politics, Latino Politics: Gender, Culture, and Political Participation. By Carol Hardy-Fanta. Philadelphia: Temple University Press, 1993. 249p. \$44.95 cloth, \$18.95 paper.

The steady increase in the Latino populations assures that cities that have not traditionally had politically organized or visible Latino populations now do. A case in point is Boston. Over the past decade, Boston's Latinos have elected a Latino to the state legislature and several others to school board positions. Further, they contributed to the governing coalitions of Mayors White and Flynn. The recentness of Latino political influence offers scholars a unique opportunity to examine the emergence and early evolution of local Latino ethnic politics. Hardy-Fanta seizes this opportunity in order to examine gender differences in the way Latino women and men (hereafter, Latinas and Latinos) understand and act on politics.

Hardy-Fanta offers an extensive review of the literature on gender differences in the conceptualization of politics and an equally thorough review of the more narrow literature on Latina politics. Her own research builds on these earlier findings of gender-based differences in the ways women and men envision the meanings of politics and political participation to identify several gender-based differences between Latinas and Latinos. Latinas, for example, envision a politics that is more participatory than do Latinos. Latinas seek collective political activity among community members who are connected to each other through common political objectives and friendship. Latinos, on the other hand, perceive politics in terms of personal advancement and hierarchical organization. Thus, where Latinas generate a sense of community and citizenship through interpersonal ties, Latinos rely on formal structures to organize politics.

Hardy-Fanta then identifies differences in the impetus to political participation. Politicized Latinas seek to raise the consciousness of the community; politicized Latinos respond to opportunity. Hence, she reports a pattern of Latinos forming community-based organizations and then leaving these organizations to work for the city. Latinas, on the other hand, are more likely to be involved directly in the community. While she finds that these patterns of behavior and understanding of politics dominate each gender's approach to politics, she is careful to note that there are exceptions. Some of the men interviewed sought a more participatory, inclusive politics; some of the women sought a more traditional, formal hierarchical politics.

Clearly, Hardy-Fanta's findings are politically charged and transcend the narrow focus on Latino politics. Regrettably, her discussion is unclear on the degree to which these are gender-ethnic traits or gender characteristics irrespective of ethnicity. The study recognizes this dilemma, but only confuses the analysis by taking each position at different points in the book. She only briefly touches on the degree to which one's class reinforces gender differences; a topic that merits further examination.

Hardy-Fanta bases her study of Boston Latino/a politics on ethnographic interviews and participant observation. The small number of interviews (n = 53) warrants caution in interpreting these findings. While she is sensitive to this issue, noting in the introduction that the findings are "suggestive, not conclusive" (xiv), she is not attentive to the weakness of the data in much of the analysis. At its extreme, this over-reliance on the results of a small sample leads to in claims like "Latino men were five times more likely to define politics in terms of positions and status than were the women" (48). This inappropriate quantification detracts from the importance of the questions being asked about gender and ethnicity and their relationship to politics.

The study could, however, have enriched these data and their interpretation by providing more extensive sociodemographic detail on the interviewees. While it is evident that the respondents trace their ancestry to several Latin American and Caribbean countries as well as Puerto Rico and that slightly more than half are organizational or political leaders, the reader would benefit from knowing such details as age, immigrant generation, length of time since immigration (or migration, in the case of the Puerto Ricans), period of residence in Boston, and citizenship. Each of these factors may have more influence than or interact with gender to produce the reported results. If the men in the sample, for example, are older than the women, they may have had greater opportunities to participate in non-Latinobased community political activities. The native born, regardless of gender, are likely to have experienced a different form of political socialization than the foreign born. With such a small sample, having a different distribution in terms of age or nativity between Latinos and Latinas could skew the findings. Thus, the absence of a clearer demographic portrait of the composition of the sample presents a serious gap in the study.

One area where such information is discussed—national origin, offers some interesting insights into the development of Latino politics, not just in Boston, but nationally. In most areas where Latinos are concentrated, one national origin group predominates—Mexican Americans in the Southwest, Puerto Ricans in New York and the Northeast, and Cuban Americans in South Florida. Few areas are genuinely "Latino" in the sense that they have two or more Latino national origin groups with each group sharing local political power. Traditionally, Chicago has been the best example of this phenomenon.

Hardy-Fanta clearly demonstrates the pan-ethnic nature of Boston's Latino identity. She finds that the individual concerns of one national origin group have not shaped the Latino political agenda. Instead, the groups unite around a series of public policy issues broadly identified as "the needs of the community." These include affordable housing, AIDS, and high dropout rates for Latino youth (138). Only one issue specific to any group appears in Hardy-Fanta's list of community public policy issues-Puerto Rico's status. This finding reinforces other empirical research into the foundations for a pan-ethnic Latino politics. It appears that if panethnicity is to emerge, it will be built on a series of issues within the United States confronting a community in poverty rather than home-country issues. Interestingly, the identification of these issues crucial to the community did not differ between men and women.

Despite the limits imposed on the analysis by the small sample and limited sample demographics, Hardy-Fanta raises some very interesting questions about the role of gender in the development of U.S. ethnic politics. Further, she offers some insights into the emergence of Latino politics in an area where it has only recently begun to manifest itself. This early examination offers an important baseline for future studies of Boston Latino politics as well as a useful point of comparison for examining the emergence of ethnic politics in other cities.

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The Promise and Paradox of Civil Service Reform. Edited by Patricia W. Ingraham and David H. Rosenbloom. Pittsburgh: University of Pittsburgh Press, 1992. 329p. \$39.95 cloth, \$18.95 paper.

The Civil Service Reform Act of 1978 (CSRA) is one piece of legislation for which the Carter administration will long be remembered. Patricia Ingraham and David Rosenbloom's book grew out of a conference marking the tenth year of the reform held at the Maxwell School of Syracuse University. The volume contains revised papers given at the conference as well as chapters from experts who did not attend the conference.

The book is a treasure-trove of information on the reform and its consequences. It is divided into sections on designing the reform, the institutions created by the reform act, processes and procedures under the reform, and the lessons of reform. Much of it is of interest mainly to specialists and inside players, but there are some big issues raised by the reform and addressed by many of the contributors to the book.

CŚRA abolished the Civil Service Commission and established the Office of Personnel Management, the Merit Systems Protection Board, and the Federal Labor Relations Authority. It also established the Senior Executive Service (SES), a monumental change that created a rank-in-the-person system for top federal executives. SES also features performance bonuses, a revised system for evaluating executives, expanded training and development opportunities, and a provision allowing up to 10% of the total SES positions to be filled by political appointees.

The reform has had an interesting history, to put it mildly. Jimmy Carter made it a priority in his campaign. In fact, notes Ingraham, Carter was the "first president

in history to campaign with civil service reform at the top of his policy agenda" (p. 11). Initial problems with the reform emerged when Congress cut back on the bonuses promised to federal executives, but the real test occurred during the Reagan administration when provisions of the reform act were aggressively used to increase presidential control over the civil service.

Chester Newland argues that adoption of the act changed the balance of power in the executive branch between the president and Congress. The president was now acknowledged to be the dominant figure. CSRA, he says, was "a great departure from the earlier view of presidential responsibility, which held that the executive branch, in joint oversight with Congress, was to ensure that the laws are faithfully executed" (p. 64).

There is much evidence that the act did have a significant impact on presidential influence over the bureaucracy. Take two examples. The rank-in-the-person provisions of the act now allow an administration to move career SES executives from position to position, a power that can be used with telling political effect by a savvy administration. Ten percent of SES positions can be filled by political appointees, another provision of immense political significance. The question is why

Congress enacted such provisions.

It is possible that Congress simply intended to change the balance of power between itself and the executive branch, but this does not fit well with the increase in congressional oversight during the 1970s, the enactment of statutes such as the War Powers Act and the Congressional Budget Act, and other attempts to increase congressional influence. Other explanations given by Newland (see also the chapter by Mark Huddleston) include Congress's tendency to focus on the areas of greatest concern to powerful interest groups (e.g. Veterans preference) rather than on issues related to the relatively small and unorganized corps of senior executives, a tendency to concentrate on electorally appealing issues such as whistleblower protection, and even the lulling effects on congressional thinking of the longstanding bias of the community of public administration scholars toward greater presidential influence over the executive branch.

Since Congress killed earlier attempts to enact versions of a senior executive service such as Eisenhower's Career Executive Service (Huddleston, pp. 171-2), we know that it is capable of focusing on such issues, and its other actions in the 1970s do not indicate an institution bent on diminishing its own prerogatives. In short, it is still not entirely clear why Congress gave such power over federal executives to the president. Much of our literature on the politics of administration emphasizes the special ties between Congress and top agency officials and the benefits that both obtain from these ties. When we have a comprehensive explanation for the adoption of the SES provisions of CSRA that increased the potential for an administration to control top executives, we will substantially increase our understanding of contemporary American politics.

Many of the contributors to the book do not evaluate either the reform or its results very positively. Beryl Radin discusses the multiple and often conflicting elements in its design. Robert Vaughan concludes that neither the Merit Systems Protection Board nor the special counsel have "provided the oversight and protection that many of the advocates of the CSRA predicted" (pp. 134–5). Rosenbloom's analysis of the Federal

Labor Relations Authority indicates that "FLRA has not yet molded Title VII into a coherent body of law for the creation and regulation of a vigorous bilateral collective bargaining system" (p. 154). James Perry concludes that merit pay "may have little consequence for individual and organizational performance" (p. 213). And J. Edward Kellough and Rosenbloom conclude that the equal employment opportunity provisions of CSRA did not lead to dramatic progress.

As Ingraham notes, some of the failures of CSRA can be attributed to the fact that the reform actually increased the complexity of an already complex system, some to the fact that it contained potentially contradic-tory elements which made the "effectiveness of the reform unpredictable" (p. 19), and some to the fact that many important parts of the reform rested on untested assumptions about the transferability of private sector management techniques to the public sector. The essays also indicate that one should not underestimate the import of the politics of budget austerity that affected implementation of the reform (the failure to live up to promises made with respect to bonuses for executives is the most prominent example), and the fact that many of the problems were due to an event unanticipated by the designers of the reform, namely that the Reagan administration would soon come to office and play a big role in shaping OPM and implementing CSRA.

Overall, the editors and authors of this book should be complimented for producing a valuable set of essays critiquing the CSRA and its implementation. Those who study the politics of federal personnel policy will find it an indispensable source of information and analysis. Those with an interest in presidential power, executive-legislative relations, and the complex role of bureaucracy in American government will also find much here to think about.

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Even the Children of Strangers: Equality Under the U.S. Constitution. By Donald W. Jackson. Lawrence: University Press of Kansas, 1992. 282p. \$35.00 cloth, \$14.95 paper.

The Color-Blind Constitution. By Andrew Kull. Cambridge: Harvard University Press, 1992. 301p. \$35.00

Andrew Kull traces the history of an idea-that our Constitution is, or should be, color blind. Despite the persistence of this idea, he points out, there is no ban per se on racial classification anywhere in the Constitution, nor has the Supreme Court ever assumed such a ban. On the contrary, far from heeding Justice Harlan's impassioned dissent in Plessy v. Ferguson (1896)—that "[o]ur Constitution is color-blind, and neither knows nor tolerates classes among citizens"—the Court has almost invariably agreed with Henry Billing Brown, who declared for the majority in that same opinion that racial classifications are as permissible as any others as long as they are "reasonable." In his finely-researched and elegant book, The Color-Blind Constitution, Kull discusses the reasons for, and the consequences of, the Court's decision to side with Brown and not Harlan.

Kull begins by tracing the origin of the fourteenth amendment's equal protection clause, and observes that in framing it the thirty-ninth Congress flatly rejected language favored by the abolitionists that would have expressly prohibited the government from engaging in racial discrimination. Such language was unacceptable, Kull explains, because of its "uncompromising effectiveness" (p. 87). The drafters preferred the "equal protection" language suggested by John Bingham because, apart from outlawing the detested Black Codes, it did not necessarily mean anything in particular. As William Nelson said, in an observation quoted by Kull, "Americans of 1866, like Americans of today, could all agree upon the rightfulness of equality only because they did not agree on its meaning" (The Fourteenth Amendment [1988]).

In the short run the fourteenth amendment's protean language meant its enforcement, "in an unsympathetic climate, would narrow to the vanishing point," yet in the long run, Kull points out, this same language would yield a virtual revolution in American law (p. 87).

The "revolution" began in earnest after Brown v. Board of Education (1954). By the mid-1960s most de jure segregation had been abolished on Brown's authority and Congress had passed major civil rights legislation. Ironically, Kull argues, it was at precisely this time, when the historic struggle for equal rights had fulfilled its goal, and the Constitution had come as close to being "color blind" as it ever would, that the "classical" civil rights movement came to an end.

The movement ended once African-Americans realized that guaranteeing legal equality to the residents of Watts and Harlem did nothing to ease the stark inequalities that still separated the races. Civil rights strategist Bayard Rustin was correct, Kull says, in his observation that the successful attack on the "imposing but hollow structure" of Jim Crow had involved institutions which were "relatively peripheral . . . to the fundamental conditions of life of the Negro people." Hereafter, Rustin predicted, the civil rights movement would concentrate on achieving "the fact of equality" ("From Protest to Politics: The Future of the Civil Rights Movement," Commentary 39:25–27 [Feb. 1965]).

In 1965 Daniel Patrick Moynihan noted, in his muchmaligned and sadly prophetic Report, that "equality in fact" could never be achieved unless blacks as a group were helped in their efforts to become stronger competitors. Many of his specific recommendations were echoed in the subsequent Kerner Commission Report, which urged the government to implement a broad range of programs requiring massive public expenditure for education, housing, job creation, and social reform. These programs were necessary, the Commission explained, because "[w]hat the American economy of the late nineteenth and early twentieth century was able to do to help the European immigrants escape from poverty is now largely impossible. New methods of escape must be found for the majority of today's poor" (Report of the National Advisory Commission on Civil Disorders, 1968, p. 282).

These "new methods of escape" never materialized.

These "new methods of escape" never materialized. The public was unwilling to pay for them, and many African-Americans were no longer willing to wait for them. After riots broke out in hundreds of cities during the summers of 1966 and 1967, public officials realized that something had to be done right away—something with maximum visibility and minimal cost.

Race-conscious "quick fixes" were thus adopted and, as Kull observes, "the color-blind consensus, so long in forming, was abandoned with surprising rapidity" (p. 183). Where the Kerner Commission had urged "a

comprehensive approach designed to reconstruct the ghetto child's social and intellectual environment," judges could only mandate racially-proportionate school districts. Where the Commission had recommended large-scale job creation, the civil rights agencies and the courts "could only impose racial preferences in the zero-sum game of allocating existing jobs" (p. 189).

These race-conscious remedies were often accomplished, Kull says, by turning relevant laws on their head. The Civil Rights Act, enacted only after its proponents assured their southern colleagues that it would never result in preferential treatment, was soon interpreted to permit and even mandate such treatment. The Voting Rights Act, designed to prohibit racial discrimination in the electoral process, was by 1970 being used by the federal government to compel states and municipalities to alter systems of representation in order to facilitate the elevation of black and hispanic candidates. ("It followed," Kull observes, "that the statute's true purpose would be served by refusing its enforcement" [p. 207].)

[p. 207].)
When Kull turns his attention to affirmative action programs, he demonstrates the same circumspection that characterizes his work as a whole. He is clearly troubled by such programs, which are possible only in a society that has largely abandoned its color-blind ideal. Yet he appreciates the appeal of these "largely symbolic gestures" if the alternative is nothing at all, and he recognizes that people who are the descendants of slaves have a unique claim to governmental solicitude. His conclusion is unequivocal, however, that "the moral awkwardness of asking black Americans to be content with nondiscrimination should not stop us from giving that answer to everyone else" (p. 223).

If a policy of nondiscrimination becomes the exception, rather than the rule, Kull fears that "the convenient and destructive practice of allocating social resources by racial and ethnic groups" will become the American political model (p. 221). This, he says, would preempt "the belief in a transcendent individualism [that] once seemed central," and profoundly alter "the aspirations of American democracy" (pp. 221, 223).

Whereas Kull concludes by lamenting the prolifera-

Whereas Kull concludes by lamenting the proliferation of what he considers deleterious group-based remedies, Donald Jackson concludes *Even the Children of Strangers* by suggesting the opposite: that sometimes such remedies are both necessary and just. Despite this fundamental philosophic difference, however, the two authors have much in common: both discuss the evolution of equal protection jurisprudence, and do so within a distinctive moral framework; at the same time both avoid speaking in anything approaching simplistic or doctrinaire language.

The two books are different, however, in both tone and focus. Whereas Kull's is closely-focused and scholarly, Jackson writes primarily for the undergraduate and gathers his material from secondary sources. Yet Jackson, however modest his ambitions, nevertheless wrote a book that in its own way is as satisfying as Kull's—and, at least in those chapters where he challenges conventional notions of equality, also as provocative.

Jackson observes that Americans are thoroughly committed to "individual-regarding equality," as this term was defined by Douglas Rae and his co-authors in Equalities (1981). Accordingly, they tend to believe all human beings experience roughly the same "life chances," and thus should be treated alike. This attitude

stems in turn from their devotion to the ideal of the self-reliant individual on the one hand, and to personal merit (as they conceive it) on the other. (While such views may be more mythical than ever, Jackson says, he cites recent studies showing that a large percentage of Americans still believe they compete in a world where success stems solely from intrinsic merit.)

Proponents of what Rae calls "group-regarding equality," on the contrary, favor policies that focus on the needs of a particular class, and Jackson concedes that such policies are troublesome for all the reasons Kull cites: They encourage a welter of competing group claims, and in time become seen as "entitlements," rather than as the strictly temporal and remedial expedients they were initially intended to be (witness the case of World War Two veterans, who successfully lobbied to retain their employment preferences long after the need for them ended). Finally, progress toward group-regarding equality in one context might deflect attention from "malignant inequalities in another" (p. 123).

Yet, Jackson notes, group-based remedies should not be dismissed out of hand, if only because they have gained broad, if hardly unqualified, acceptance in many other countries: the constitutions of both India and Canada explicitly acknowledge the legitimacy of compensatory policies, for instance, and so do many international conventions (while simultaneously repudiating permanent proportionality).

More important, Jackson argues, group-regarding policies, however problematic, have their own claim to moral legitimacy: they offer some compensation to those victimized by institutional racism, and, short of undertaking the expensive structural reforms advocated by the Kerner Commission, they alone can reduce the deep socioeconomic gulf that separates the races. For example, without race-conscious programs the overall disparity in employment between blacks and whites, far from decreasing, will in fact continue to grow.

Finally, because proponents of individual-regarding equality minimize the enormous influence of socioeconomic forces, they tend to perpetuate the status quo by insisting upon what Jackson considers an artifically narrow and class-biased definition of "merit." Just as Britain once required its high-ranking civil service officers to be fluent in Latin and Greek, we also award positions of power and status to an elite whose qualifications (i.e., success in school or on standardized tests) may be equally unrelated to job performance. Anyone whose intelligence and ability fall within some broad "normal" range, including most beneficiaries of socalled preferential treatment programs, is capable of filling most positions in society, Jackson argues, and probably of filling them at least as well as someone with more "quantifiable" skills.

Jackson proposes a way we might retain our commitment to individualism and merit, as we define these terms, and still support programs that appreciably benefit members of disadvantaged classes: in assessing an applicant's qualifications we could consider the "distance they have traveled," i.e., the extent to which they have overcome, say, poverty, homelessness, or a physical handicap. We could thereby allocate valuable resources on the basis of criteria that are individual and neutral rather than collective or race-based, and, at the same time, rationalize our definition of merit. (While Kull and Jackson are guided by very different premises,

they reach similar conclusions: Kull also recommends remedial policies that focus on disadvantage, not race, but which still promise "disproportionate benefits to blacks" [p. 222]).

Both authors also agree that individual-regarding remedies should remain the ideal, although Kull alone considers race-conscious ones inherently deficient. Kull also regards the individual as the only unit properly entitled to equal protection, and fears that any expansion of this unit could imperil the unifying principle of American democracy. He does not, however, explain (at least to this reviewer's satisfaction) why this is necessarily so. Jackson, by contrast, understands that our reverence for individual-regarding equality contributes not only to the nobility of our vision, but also to its limitation.

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ELIZABETH HULL

Markets and Majorities: The Political Economy of Public Policy. By Steven M. Sheffrin. New York: Free Press, 1993. 285p. \$24.95.

According to Steven M. Sheffrin, in the United States "when markets fail, governments intervene and impose regulations on the economy" (p. 2). But not all market failures are political magnets for regulations. Market failures, he argues, become politicized only when they cause distributional outcomes that public majorities find unacceptable. On a chapter-by-chapter basis, he discusses seven cases of politicized market failure: health care; social security; environmental policy; product liability; international trade; monetary policy; and fiscal policy. In each case, he finds, the regulatory process begets perverse and unintended consequences, both economic and political. The economic costs of regulation, he maintains, often outweigh the benefits. And the regulatory process, he claims, totally excludes average citizens.

As Sheffrin sees it, the regulatory process is dominated by "the Washington money-go-round," by which he means inside-the-beltway bureaucrats, lawyers, lobbyists, politicians, pundits, staffers, and think tankers who "keep track of the nuances" of regulations in order to protect "their own interests—sometimes financial, sometimes ideological, and sometimes just the quest for self-importance" (p. 254). To remedy the situation and enhance understanding, he promises readers "principles for reform" derived "from the science of economics and the art of government" (pp. 4, 253–54).

Markets and Majorities has some bright spots, but it does not deliver on this promise. General readers are likely to find parts of Sheffrin's discussion of market failure too esoteric. Specialist readers are sure to find his discussion of the political economy of regulation in the United States oversimplified, incomplete, or just plain wrong. Four shortcomings of the book are worth noting.

First, it adds little to the scholarly and popular literatures on the seven issues it covers. For example, chapter 2 is entitled "The Peculiar World of Medical Economics." It provides a brief introduction to the micro-economics of health-care market failure. "The market for medical services," declares Sheffrin, "differs dramatically from the idealized picture of perfect markets in almost every dimension" (p. 22). Indeed it does. But rather than cut

through the "dizzying complexity of health care reform proposals," he glosses over their complexity, briefly discusses competing regulatory strategies, and concludes with a one-page summary of "the essentials of the story," including such banalities as "Federal and state governments have become dominant payers in the market for health" (pp. 57, 58).

Second, Sheffrin's thesis that regulations proliferate only when public majorities object to distributional outcomes remains unsupported, as does his thesis about the built-in failures of regulation. The conditions under which regulation occurs, the conditions that produce different types of regulatory regimes, and the relationships between regulatory structures and regulatory outcomes, are far more complex and varied than he allows. At a minimum, a better analysis of the political economy of regulation would draw on the extensive and increasingly sophisticated literature on the role of public opinion in national policy making, and systematically explore the reasons for differences in regulatory outcomes.

Third, while it may please some readers, Sheffrin's Washington-bashing tone, and tiresome references to "the Washington money-go-round," distract one from his more serious points about the limits of regulation, the degree to which economic policy has become the province of experts, and the democratic accountability problems that this may or may not pose. For example, he is certainly correct that monetary policy and international finance "raise difficult issues of delegation of authority and responsibility to the experts" (p. 160). His discussion of monetary policy and international finance is commendable.

But when it comes to regulation, Sheffrin overgeneralizes about how average citizens "are excluded from the process and viewed as naive by the Washington crowd" (p. 253). Much of the relevant political science literature indicates that regulatory accountability problems are often more apparent than real, as citizens are more capable of monitoring legislators' behavior, and legislators are more cognizant of citizens' preferences and more capable of monitoring bureaucrats' behavior, than is generally supposed. Unfortunately, he ignores this literature.

Finally, the book is not organized or edited as well as it might have been, and the concluding chapter reneges on the opening chapter's promise of general reform principles. In the concluding chapter, Sheffrin finds it necessary to remind readers that "an understanding of the tensions between market failure and political obsessions" is "the organizing theme of this book" (p. 254). On the excuse that "the application of these ideas are subtle and every area of economic policy has its own contours," (p. 254) he ends not by articulating and defending general principles for reform, but by rehashing chapter two's discussion of health policy and chapter eight's discussion of fiscal policy. The book's appendix is a fair annotated guide to Washington think tanks and sources of policy information (pp. 263–75).

Sheffrin is to be applauded for his ambitious effort to advance the political economy of public policy and bring it to a general readership. For the most part, however, the book is unsatisfying.

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COMPARATIVE POLITICS

Political and Economic Interactions in Economic Policy Reform. Edited by Robert H. Bates and Anne O. Krueger. Cambridge: Blackwell, 1993. 481p. \$59.95.

This is a true work of comparative political economy. Each of the case studies is the collaborative product of a political scientist and an economist. The authors seriously analyze how politics shape economic policies and economic policies shape politics during the pre-reform, reform, and post-reform periods in eight developing countries: Brazil (Deepak Lal and Sylvia Maxfield); Chile (Philip Brock and Barbara Stallings); Ecuador (Francisco E. Thoumi and Merilee S. Grindle); Egypt (Robert Holt and Terry Roe); Ghana (J. Clark Leith and Michael F. Lofchie); Korea (Richard N. Cooper, Stephan Haggard, and Chung-in Moon); Turkey (Anne Ö. Krueger and Ilter Turan); and Zambia (Paul Collier and Robert H. Bates). Bates and Krueger provided the authors with an agenda of questions but also encouraged them to write detailed country investigations. The aim was to systematically evaluate existing hypotheses on economic policy reform as well as to generate new insights. The experiences and successes (and failures) of the countries are sufficiently different to raise skepticism about the validity of creating a single model of the reform process, but what is most remarkable about this book is the extent to which the editors are able to uncover some generalizable propositions about the reform process.

The amassing of such high-quality evidence has the effect of convincingly debunking some standard arguments while suggesting alternatives. For example, there is not a unique kind of crisis that precipitates the reform process, but economic deterioration does precede reform efforts. Nor is political authoritarianism a necessary condition for successful reform although a strong executive and an empowered technocracy may be. I am in agreement with Bates and Krueger that "one of the most surprising findings . . . is the degree to which the intervention of interest groups fails to account for the initiation, or lack of initiation, of policy reform" (p. 454). Rather, they find that political institutions often inhibit mobilization and that ideology transforms interests. Consequently, there is no apparent correlation between citizen interests and the initial policy content. On the other hand, as the study by Leith and Lofchie best demonstrates, the interest groups created by a policy

may account for its stability.

The most important claims of the book have to do with ideology, political institutions, and implementation. Given the uncertainty of the effects of comprehensive reform, groups and individuals may not know where their interests lie. They are thus subject to the persuasive power of politicians. And what determines the ideological choice of politicians has a lot to do with the institu-tional rules of politics. The fact that at least half the authors are economists and others are rational choice scholars makes these findings particularly compelling. Certainly they help to account for some of the policy

Equally significant is the claim that neither good policies nor a commitment to reform are enough. A competent and strong technocracy is also necessary. The

editors, drawing on the cases, on Barbara Geddes' forthcoming The Politicians' Dilemma, and on recent work in American politics by Keith Kreihbel and by Roderick Kiewiet and Mathew McCubbins, offer a series of interesting and testable explanations of why and how politicians might delegate power to the technocrats. The argument rests on the necessity of government providing credible commitments to its reform aims so that economic agents will then make the appropriate choices. As Bates and Collier so tellingly document in their discussion of Zambia, politicians must demonstrate to constituents that their self-interest and their policy commitments are compatible. Political institutions help to solve this problem.

The breadth of the book highlights the weak points in the text. First, the incorporation of other countries, particularly those of Eastern Europe, might affect the findings. Second, there needs to be more of a methodological discussion. A stockpiling of comparative evidence on the same issues is an important step in evaluating hypotheses, and it may even be enough. Is the underlying apologia for the failure to provide statistical tests appropriate or not? Third, there is too little attention to the design of the technocracy and other bureaucratic structures, by their own account so essential to the development enterprise. This is particularly surprising given that Krueger is one of the progenitors of the argument that regulatory design can promote rent-seeking and that Bates has used the rules that create rent-seeking and other institutional arrangements to explain how rational politicians

can make nonrational policies.

The strengths of Political and Economic Interactions outweigh any weaknesses a reviewer is compelled to delineate. The work represents a significant contribution to the new institutionalism in political economy. It is not just a question of "getting the prices right." It is also necessary to get the institutions right. However, this book goes far beyond that insight. It demonstrates exactly where, and for what, institutions and ideology matter. It lays out the research agenda for scholars of development and offers an approach that integrates the skills of the area study specialist, the political scientist, and the economist. It indicates how one can have a concern with country specificity and, at the same time, with model building, model testing, and generalizability. And it does all this while improving our understanding of development and our capacity to initiate and implement economic reform.

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MARGARET LEVI

Merchant Capital and the Roots of State Power in Senegal, 1930–1985. By Catherine Boone. New York: Cambridge University Press, 1992. 299p. \$59.95.

The political roots of Africa's current economic crisis have become a favorite research topic for Africanist political scientists. The general conclusion of most of this work is that economic growth has been sacrificed at the altar of state consolidation and political stability. The economic policy choices of African leaders since independence have been motivated less by economic rationality and a coherent development strategy than by the political exigencies of state elites that lacked legitimacy and a stable social base. For example, much government intervention in Africa has been motivated by rent seeking and patronage and has proven impervious to reform despite its obvious economic costs, because it yields great political dividends. The inevitable consequence has been pervasive corruption, economic stagnation, and international debt.

This explanation for Africa's economic plight is, by now, a familiar one that most scholars share, albeit with wide variations in theoretical emphases and not always with adequate evidence. This excellent monograph by Catherine Boone tells the Senegalese version of this story with considerable insight and convincing empirical proof. Boone begins her story in the colonial era and shows how the interests of French commercial firms conditioned industrial policy and resulted in a weak industrial sector at independence. Focusing special attention on the textile sector, she then demonstrates quite convincingly how, in the first decades of independence, the state elite "senegalized" the sector so that its rentseeking potential would benefit key elements of the national elite and at the same time maintained policies that undermined the expansion, vertical integration, and competitiveness of the industrial sector. Pervasive and often destructive state intervention in the economy facilitated "the fusion of elites" on which the postcolonial order was built. Such scholars as Bayart or Lonsdale have emphasized this process with regard to ethnically divided societies, but Boone convincingly demonstrates its applicability to the relatively homogeneous Senegal. In sum, "consolidating power and promoting economic growth proved to be contradictory imperatives" (p. 4) for the postcolonial Senegalese state. In turn, the increasing costs of maintaining political stability led to the economic crisis that erupted in full view in the early 1980s, where Boone concludes her analysis.

Boone places her analysis in the context of recent academic debates about state autonomy in the developing world, the social bases of state power, and the relationship between capital and state structures. Particularly interesting is her discussion of the state's highly ambivalent attitude toward indigenous capital. Again and again, Senegalese state elites have worked to undermine the emergence of an autonomous bourgeoisie, in contradistinction, Boone points out, to the Bonapartiste state of Marxist fame.

The book provides a trenchant contribution to these academic debates, which alone would make it highly recommended reading for anyone interested in statesociety relations in the developing world. However, Boone's outstanding achievement rests in the empirical richness and care with which she documents these processes. Previous African studies in this area have either been inattentive to theoretical disputes, cavalier about evidence, or illiterate in economics. Boone makes judicious use of rich primary materials, notably from the colonial era, to promote a sophisticated theoretical argument. Her attention to economic logic makes the study political economy in the true sense of the term. I found particularly incisive the discussion of the textile sector since its origins in the colonial era. Descending to the level of individual firms, Boone expertly deconstructs the sector to reveal the social networks and interests being served by state policies. Fortunately, she never loses sight of the theoretical or comparative implications of her data, so that the argument never gets lost in the details.

Several minor flaws should be noted. The analysis ends in the early 1980s with the argument that the contradictions underlying the Senegalese regime prevented it from emerging as a developmental state and led it to an unstable impasse. During the 1980s, however, the regime parlayed tentative attempts at simultaneous political and economic reform into increasingly generous support from Western donors. Boone might have discussed whether and how access to international finance has increased the state's autonomy vis-à-vis various domestic interests. That the regime has persevered to this day suggests that Boone has underestimated its resourcefulness and adaptability. In addition, it forces us to reassess the earlier period, in particular, the extent to which the state in Senegal has been a prisoner of its social base. Boone underestimates the extent to which the absence of a developmental state reflects the ideological proclivities and personal interests of the state's elite, rather than only societal pressures. It is true that ISI policies served political interests, but they also conformed to the received wisdom of the entire policy community. Today, these policies remain largely sustainable thanks to donor finance, and individuals within the highest echelons of the remarkably stable political elite continue to derive material benefits from these arrangements. Why should they try to promote rapid economic development? Boone's answer to this question is not entirely persuasive.

The question is particularly pertinent given Senegal's limited resource base, which leads to a second criticism. In her discussion of policy outcomes, Boone pays insufficient attention to the economic constraints that have limited available choices. Agriculture is a good example: particularly insightful about the role of traditional rural elites and patronage on the failure of modernization, the book barely mentions the devastating impacts of persistent drought and increasing desertification or of sharp declines in the world price for groundnuts, Senegal's traditional export. Consideration of these nonpolitical constraints on development policy should lead to a more nuanced appreciation of the premium that Senegalese elites have put on political stability.

These criticisms notwithstanding, Boone has produced a splendid book. In its theoretical intelligence and empirical richness, it is one of the most important recent works on African political economy. It deserves to be widely read.

Michigan State University

NICOLAS VAN DE WALLE

The End of the Soviet Empire: The Triumph of the Nations. By Helene Carrere d'Encausse. New York: Basic Books, 1993. 292p. \$24.00.

From Union to Commonwealth: Nationalism and Separatism in the Soviet Republics. Edited by Gail W. Lapidus and Victor Zaslavsky. New York: Cambridge University Press, 1992. 127p. \$39.95 cloth, \$12.95 paper.

The Post-Soviet Nations: Perspectives on the Demise of the USSR. Edited by Alexander Motyl. New York: Columbia University Press, 1992. 322p. \$34.50.

Reading these three books completed in the fall of 1991 only reminds us how much has changed in less than two years and how quickly has faded the euphoria that the end of the Soviet Union would usher in a new era of world democracy and peace. As of Fall 1993, Soviet communism has been replaced not by liberal democracy but by an ideology of rampant ethnic tribalism. Autocracies as ruthlessly dictatorial as Soviet communism have already emerged in Azerbaijan, Turkmenistan, Uzbekistan, and Tajikistan, legitimating their rule on ethnic nationalism. In these and other Soviet successor states, tens of thousands have died from interethnic violence. Millions have become political refugees fleeing persecution by secessionist movements and even full-scale civil wars. Weak national governments and illegitimate policymaking structures commanding little political authority have sparked open warfare from Russia to Georgia and from Moldova to Tajikistan.

Ethnic nationalism throughout Central Eurasia has made the world a much more dangerous place than it even was with the armed threat of a nuclear Soviet Union. An arc of crisis extending along almost the entire perimeter of the 15 Soviet successor states threatens to flare into numerous regional wars before the end of this century. These are wars likely to dwarf even Bosnia-Herzegovina in the scale of casualties and atrocities and to escalate inevitably into wider conflicts with the intervention of border countries like Iran, Turkey, and Afghanistan to advance their own geostrategic interests, defend various ethnic-religious minorities, and extend their political dominance. The triumph of the nations in the subtitle of Carrere d'Encausse's book has become, by 1993, a revenge of the nations. The USSR was replaced more by a state of chaos and anarchy than by 15 newly independent nation-states.

These somewhat dated retrospectives of recent history based on little original research can still be recommended to nonspecialists for their panoramic overviews of the cumulative factors leading to the rise of ethnonationalism in the former Soviet Union. They provide invaluable historical insights into the origins, in the collapse of the Soviet Union in 1985-91, of the numerous ethnic conflicts alive in 1993. The authors in all three books contend that ethnic nationalism, more than any force, accounted for the demise of the Soviet Union and Soviet communism. Ethnic nationalism represented a series of spontaneous grassroots movements arising out of universal failures of the Soviet Union and communist rule but conditioned by unique historical circumstances and grievances among the various ethnic groups in different locales of the country.

All three books avoid polemics and the more simplistic one-dimensional view of Soviet ethnic policy and of Soviet leaders over generations as "nation-killers." With religious and racial hatreds inflaming the violence in Central Eurasia, the authors provide sober and balanced analyses of the complex causes for the many interethnic conflicts ringing the late Soviet Union. They eschew a tendency to blame one side or to attribute the outbreaks of interethnic conflicts in the former Soviet republics solely to conspiracies hatched by hard-line pro-Unionist communists in Moscow. Dated as they are, all three books are also prescient in anticipating that ethnonationalism would remain the major irrational force to harness in building stable nations, creating political authority, and founding democratic governments after 1991 in the countries that arose out of the late Soviet Union.

The authors emphasize Soviet communism's foibles, failures, repressions, insensitivity, and contradictions in dealing with ethnic nationalities over seven decades.

This was a system that attempted to assimilate over a hundred ethnic nationalities through modernization while encouraging them to retain their ethnic identities through a federation based nominally on ethnic self-determination. Ethnic self-determination was an improvisation and pragmatic accommodation forced on Lenin after 1917. Marxism–Leninism provided only the vaguest outlines of a policy to deal with a multiethnic country except for its admonition that ethnic consciousness was destined to wither away with communism. Soviet ethnic-nationality policy varied with the leader in power and actually constituted a hodgepodge of several often contradictory policies.

The improvisations, insensitivity, foibles, and failures in a context educating and modernizing these ethnic groups were fated to produce the very centrifugal force tearing the system and country apart. The catalyst was Gorbachev, who loosened authoritarian controls over the population after 1985 and encouraged citizen participation and civil society. The citizens rallied not in defense of Gorbachev's reforms of communism but in support of their own ethnic grievances. Civil society formed not around a democratic consensus and identification with a renewed Soviet Union but around the various and competing ethnic nationalities.

The two books edited by Gail Lapidus and Victor Zaslavsky and by Alexander Motyl overlap somewhat as a series of interpretive essays by noted Western and Russian social scientists and historians. They analyze ethnic nationality policy during the Soviet era; the impact and consequences of Gorbachev's reforms on ethnic nationalities after 1985; and the causes of challenges to Soviet rule in the Baltic region, Georgia, Armenia, Azerbaijan, and others by 1990–91. The ethnic outbreaks and defiance of the Union spelled the end of any remaining central political authority and the de facto dismemberment of the country even before the abortive putsch in August 1991.

Three lengthy essays by Lapidus, Ronald Suny, and Victor Zaslavsky make up most of the Lapidus-Zaslavsky book. Ideal for graduate seminars, the three wide-ranging essays place the events of 1985-91 and the rise of ethnonationalism conceptually and historically in the broad evolution of the Soviet Union and the often contradictory phases of policies affecting ethnic nationalities. Lapidus singles out the impact of Gorbachev's "cognitive" and "political" liberation after 1985 on the explosion of ethnic discontent. Zaslavsky attributes particular importance to the rise of Russian ethnic national consciousness after 1985 in legitimating the demands for independence by all of the other nationalities. Suny, the eminent historian of twentieth-century Armenia, is perhaps the most successful of the three. Suny argues, contrary to conventional wisdom, that the Soviet era and policies actually, in many ways, heightened ethnic national identities and that ethnic discontent against the Soviet system in the conflicts between Abkhazians and Georgians and others antedated the Gorbachev era, appearing as anomic outbursts in the 1960s and 1970s. The book also includes briefer but insightful perspectives of ethnonationalism and the demise of the Soviet Union from two academic advisers to Boris Yeltsin: Leokadia Drobizheva and Galina Starovoiteva.

Motyl's contributors (notable Western Sovietologists like Walker Connor, John Hazard, and Theodore Friedgut) cover similar ground in well-focused chapters on specific dimensions of ethnic policies and the conse-

quences of the changes brought about by Gorbachev after 1985. Amy Knight draws from her expertise to assess the role of the Soviet "political police" in regimenting ethnic nationalities over decades. Friedgut considers the change, after 1985, from mobilized participation to autonomous political behavior and diversity that quickly evolved into demands for ethnic independence. Ronald Hill points out the improvisational and contradictory nature of the communist ideology as a source of guidance for decades in dealing with Soviet ethnic nationalities.

Fortunately, except Motyl, Gregory Gleason, and Mark Beissinger, the contributors bypassed Motyl's secondary goal for the book, namely, to substantiate the failures of Western Sovietology in having underestimated or ignored the importance of ethnic nationalities as a political force in the former Soviet Union. No one would question Motyl's assertion that a post-Soviet scholarship must focus comparatively on the importance of ethnic nationalities and cultural pluralism in many countries of the world to understand the politics of these 15 countries. Yet his raising of the issue as an indictment of Western Sovietology and his insistence on defending the concept of totalitarianism to characterize the Soviet system even under Gorbachev seem completely out of place in what is otherwise an excellent series of individual essays.

His blanket condemnation echoes similar charges against the "Sovietology establishment" that others, notably Martin Malia and Robert Conquest, have recently sounded and that seem more akin to a settling of academic scores than serious methodological critiques. Motyl himself is best refuted by the insights on various Soviet ethnic nationalities provided over the past two decades by scholars like Ronald Suny (Armenians and Georgians), Nancy Lubin (Central Asians), Roman Solchanyk (Ukrainians), and even Motyl contributor Zvi Gitelman (Jews). As to the concept of totalitarianism, Ronald Suny points out in his essay in the Lapidus-Zaslavsky book that even Zbigniew Brzezinski, in his classic defense of totalitarianism in debate with Carl Friedrich, had failed to include nationalities among the "islands of separateness" potentially threatening to Soviet totalitarian rule (pp. 23-24).

French political scientist Helene Carrere d'Encausse is a member of the Sovietology establishment—lauded by Henry Kissinger and Richard Pipes—who can hardly be accused of having ignored Soviet ethnic nationalities. Ten years before the collapse of the Soviet Union, she had warned that Soviet ethnic nationalities would be a source of major turmoil and conflict in the Soviet system. In The End of the Soviet Empire, Carrere d'Encausse ambitiously attempts nothing less than to analyze and describe in great detail all of the major ethnic conflicts and sources of ethnic challenge to Soviet rule that arose from 1985 through the abortive August putsch of 1991. Her principle sources are the numerous reports in the Soviet media over this period of time, especially those appearing in the more liberal Soviet newspapers and journals.

The book is a highly readable overview of the events in 1985–91; but readers of similar books by Hedrick Smith, Stephen White, Roman Nachaylo and Victor Swoboda, Geoffrey Hosking, David Remnick, and others will find much very familiar. Subscribers to the weekly Radio Free Europe/Radio Liberty Institute Report in Munich would probably find the book even more

disappointing in not providing anything original in its interpretation of the events or the factors surrounding the explosion of ethnic conflicts. The book is an excellent descriptive and historical account but lacks any overarching conceptual framework or integrated explanation for the rise of ethnonationalism.

It is a little annoying that the translation includes numerous errors. The Nineteenth Party Conference in 1988 is repeatedly called the Twenty-Ninth; Anastas Mikoyan, rather than Andrei Gromyko, is cited as the chairman of the USSR Supreme Soviet in 1985–88; Geidar Aliyev is termed the first "vice-president," instead of the first deputy prime minister; the chairman of the KGB is described as KGB "president"; Kazakhstan, with a population of only 17 million in a territory the size of India, is termed "populous"; and so forth.

A best-seller in France and an alternate selection of the History Book-of-the-Month Club in its English translation, the book was clearly intended to reach a wide lay audience and would be an ideal choice for undergraduate courses in Russian and Central Eurasian politics and government.

Iowa State University

JOEL C. MOSES

Immigrants, Markets, and States: The Political Economy of Postwar Europe. By James F. Hollifield. Cambridge: Harvard University Press, 1992. 305p. \$35.00.

What a good idea to present a political theory of immigration not based on strictly economic (Marxist or liberal) theory or on international relations theory! These theories are clearly and very well presented at the beginning of the book. Hollifield's demonstration is built and tested on the French and German immigration policies. Expanding the European examples of Germany and France to the United States, he tries to demonstrate that a mixture of political and economic liberalism explains immigration phenomena: the strength of the market explains why states are unable to control entries, and political liberalism explains the rise of civil rights granted to illegals and the difficulty to implement their deportation even when it has been legally decided.

The problem is that Hollifield bases his demonstration of the weakness of the states on the activity of institutions whose role in immigration policy (in France, CES or ONI) is very small or uses very debatable statistics. The state cannot be considered a whole. It is split, particularly in the immigration domain, between a huge number of departments or agencies, all of them pursuing particular and sometimes contradictory objectives.

Sometimes Hollifield seems to forget (but he is not the first or the only one) that only one category of immigration flows have really been stopped in the European countries he studies—those of non-European Community unskilled workers. Others categories are still authorized: European Community citizens, spouses of citizens, relatives of legal immigrants, political refugees and skilled and seasonal workers.

To prove the failure of the French state to stop immigrations, Hollifield produces inadequate statistics of categories whose rights to immigrate were never abolished. A comparison of immigration flows of permanent workers before and after 1974 would have showed a huge decrease, affecting also such authorized categories as families. But these effects (sometimes but

not always those wanted by policymakers) depend on some national specificities, such as history, geographical situation, and means or organization of implementation. European global figures must be used cautiously. Otherwise, how is one to explain the fact that France registered 27,000 applications in 1992, as against 438,000 in Germany?

Hollifield's book swarms with ideas, and he has some good intuitions. He has a good perception of the national and specific cultural backgrounds of France and Germany; and he explains why Germany applied to the Turks a liberal and pluralist policy, which differs from France's assimilationist tendency. Nevertheless, its too rapid global theorization and conclusions prevent the author from accounting for the recent rise of the extreme Right and xenophobic attacks against foreigners.

The best part of the book, which makes a valuable contribution to the European academic debate, is that in which Hollifield demonstrates the power of undocumented aliens facing the law. Too often, academics have pointed to the contrary—their absence of rights vis-à-vis the state. In this field, Europe has not followed but anticipated the U.S. Supreme Court's Plyler v. Doe decision. In France, for example, a 1884 law decrees that children of undocumented aliens be admitted to public schools.

Finally, Hollifield asks interesting questions and provides some pertinent answers. But some historical mistakes and the unfinished construction of the whole show that his theoretical demonstration should be further refined. The book can then be considered an essai, in the double sense of a general point of view, with the positive effect of creating a debate and a first attempt that needs to be pursued further—in rugby, one would say "transformed."

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PATRICK WEIL

Latin America's Christian Democratic Parties: A Political Economy. By Edward A. Lynch. Westport: Praeger, 1993. 244p. \$45.00.

Edward A. Lynch has uncovered an important and underexplored theme in his treatment of Christian Democracy in Latin America. His ambitious effort constitutes one of the few attempts to analyze one of contemporary Latin America's largest political movements from a comparative perspective. If for this reason alone, Lynch's is a singular and welcome contribution.

At the outset of his book, Lynch presents the reader with an interesting puzzle. Given its great promise in the 1960s, why has Christian Democracy consistently failed to gain greater popularity in Latin America? His answer: Christian Democracy has failed because it has neglected Catholic social teaching, its ideological heritage. Driven by short-term, pragmatic considerations of office, Latin American Christian Democratic parties have repeatedly ignored the teachings of the Church, preferring, instead, to imitate the statist economic policy prescriptions put forward by their rivals. Veering unpredictably to the right and the left, Christian Democratic parties have consequently taken a beating elector-

Lynch begins his book with a discussion of Catholic social teaching, "the acknowledged forerunner of Christian Democracy." According to the author, the social teachings of the Catholic Church from Leo XIII's Rerum Novarum (1891) until John Paul II's Centesimus Annus (1991) provide "a coherent, consistent, and comprehensive basis for creating and implementing economic and social policy" (p. xiv). The fundamental tenet of this teaching for the purposes of this analysis is that "families, private schools, churches, unions, professional organizations, and even sub-state political entities are all anterior to the state in authority, even if they are created after a particular state is established" (p. x). Therefore, "the political, economic, and social thought of the Catholic Church is anti-statist in its essentials" (p. xii). Any alternative interpretation of Catholic social teaching, such as Alfred Stepan's rather opposite view in State and

Society, (1977), is "simply incorrect" (p. x).

Once finished with his portrayal of the unambiguously antistatist character of Catholic social philosophy, Lynch moves to a brief discussion of Christian Democracy in Western Europe, particularly in France, Italy, and Belgium. He traces Christian Democracy's fall from grace to the postwar period in Western Europe when Christian Democratic parties suddenly found themselves in power and consequently came to view state power, under their control, as a benign force. The same basic story repeated itself in Latin America: once Christian Democrats came to power, they abandoned Catholic social teaching and allowed the state under their tutelage to become the principal agent of social and economic change. Like their European counterparts, Latin American Christian Democrats decided that state power in friendly hands was no longer a threat. Using the power of the state, Christian Democratic governments proved to be as grossly interventionist as their rivals, violating the independence of key social groups (e.g., the family, private schools, and labor unions). Lynch goes on to argue, in separate chapters, that Latin American Christian Democrats thoughtlessly mimicked the intrusive state-centered policy prescriptions proposed by their political adversaries in three arenas: land reform, natural resource policy, and the role of the state in the overall management of the economy. Lynch concludes his book with an analysis of how well Latin American Christian Democrats have performed in the current environment of economic liberalization. Not surprisingly, he finds that the statist Christian Democrats have been left "completely unable to take advantage of the most significant political and social change in Latin America in the last 50 years" (p. 141). He looks at Christian Democratic parties first in Central America and then in South America and argues that recent electoral outcomes provide ample support for his hypothesis; that is, where Christian Democratic parties have unambiguously embraced Catholic social teaching (as in post-Pinochet Chile), the party has reaped rich electoral rewards. Where it has been ambivalent (as in Costa Rica recently), the party won but barely. Finally, where Christian Democrats continued to call for statist policy responses (as in El Salvador or Guatemala), they went down in defeat.

Lynch's rather sweeping analysis provides a useful beginning for further study into the phenomena of Latin American Christian Democracy. In addition, however, the book raises a number of questions. Much of the argument depends upon a relatively unidimensional interpretation of what might more usefully be seen as part of a much more complex and multidimensional Church social teaching. For instance, it is perhaps overly facile to dismiss alternative interpretations of the centuries-long tradition of Catholic social philosophy as simply "incorrect." Interpretations such as Stepan's "organic statism" differ from Lynch's not necessarily because they are incorrect but because the Catholic tradition lends itself (contra Lynch) to multiple and sometimes competing interpretations. Stepan, Wiarda, and others derive a "profoundly statist" view of Catholic social thought not principally from Aristotle (as Lynch claims) but from the foundational thought of St. Thomas Aquinas. Thomist philosophy became embedded, from the start, within the social, political, and economic institutions that developed in Latin America; and its impact has been even more pervasive than the tradition of Catholic social teaching since Rerum Novarum. To ignore the historical impact of St. Thomas on the one hand and, on the other, to overlook social teachings emanating from contemporary Latin American Episcopal Conferences (e.g., Medellín, Puebla, Santo Domingo) or, for that matter, national episcopal conferences, suggests that Lynch has relied on an overly narrow understanding of Church social teaching. As a result, his emphasis on private property and initiative and the inviolability of certain independent social groups could usefully be balanced by more attention to issues of equity and social justice, more the focus of recent Latin American Church teaching. I do not wish to imply that Lynch's interpretation of Catholic social teaching is in error but, rather, that his is not the only valid interpretation.

Nor should Catholic social teaching necessarily be presented as the only legitimate source of inspiration for Christian Democratic parties. While Lynch is surely correct that Catholic social teaching provided the initial inspiration for the Social Christian movement, it does not follow that the Church will forevermore be the only possible source of public policy for Christian Democrats. For example, Christian Democratic thinking on economic policy was profoundly and appropriately affected by the work of Raul Prebish and economists at CEPAL in the 1940s and 1950s (as was the Latin American Catholic Church itself, later, at Medellín and Puebla). The statecentered development matrix that emerged from this context need not be viewed as simply an errant detour or a hopeless "waste of time" (p. 182) but, rather, should be viewed as an important and legitimate adaptation of economic policy to the developmental challenges of the times. Indeed, many have argued quite persuasively that the decades of institution building that accompanied state-centered economic policies in most of Latin America from the 1930s onward was a necessary prelude in order to endow state institutions with the capacity to pursue coherent policies of economic liberalization in the 1980s.

At every opportunity, the author points out the calamitous effects of state intervention in the economy, namely, rotund policy failures combined with party identity loss and electoral hemorrhage. Yet according to Lynch, Christian Democrats around the world cannot wait to make the same mistake all over again. Indeed, he presents his case so consistently that the reader is left to wonder, Why are Christian Democratic politicians so stupid? Lynch pins the blame on Christian Democratic "pragmatism" but then points out repeatedly that their pragmatism was utterly unpragmatic, consistently producing both policy failure and electoral failure. Unless

we postulate that Christian Democratic politicians are quite dumb, we need to look beyond pragmatism as a motivating factor for policy choice. Very crucially, politicians and policymakers, most of the time, are not confronted with infinite policy options to chose from when they attempt to formulate policy. In order to understand their actions, we need to understand the structure of choice that Christian Democratic policymakers faced in most Latin American countries throughout the period of state-led development. What is viewed as mere "pragmatism" can, in fact, be seen as the practice of politics, the art of the possible.

Perhaps the most troubling aspect of Lynch's work is its almost entirely descriptive character. There is no sustained attempt to explain why some Christian Democratic parties sometimes eschew pragmatism and maintain their fidelity to the Catholic social doctrine (as they apparently did in Belgium and in post-Pinochet Chile), while most parties do not. Why did Venezuela's COPEI craft a more faithful and more successful agrarian reform than the PDC in Chile or El Salvador? What shaped their respective choices? Missing from the discussion, for example, is some appreciation of how the structure of choice available to policymakers was radically altered in the late 1980s by the collapse of international Leninism and the resulting change in the role of the state. In place of explanation, the author often engages in disquieting counterfactual reasoning, always with the same underlying message: if only Latin American Christian Democracy had retained the movement's commitment, it could have become a genuine alternative to the right and left statism that have plagued the continent. Such an alternative would have had a broad and loyal constituency among Latin Americans left out by statist, insider politics (p. 48). The problem is, of course, that such statements are nearly impossible to falsify.

The evidence for Lynch's central claim that Christian Democratic electoral success (and failure) is highly correlated with its fidelity (or lack thereof) to Catholic social teaching is somewhat thin. His argument often presumes that the predominant cleavage underlying electoral politics in the region hinges on differences with regard to economic policy. Thus, he claims that the electoral success of Patricio Aylwin's Concertation government in 1989 in Chile was due to Aylwin's fidelity to Catholic social doctrine in economic policymaking, ignoring the key role of other, more salient issues, like the ongoing struggle against Pinochet and the authoritarian regime. Similarly, he attributes the loss of the Christian Democrats in El Salvador in 1988 to their betrayal of Catholic ideological roots, discounting popular dissatisfaction with the civil war. Lynch's claim that Alberto Fujimori's "Cambio '90" victory in the 1990 elections resulted from his adherence to Catholic social teaching even though the Catholic Church publicly supported his opponent ratifies the impression that the author's interpretation of the social teachings cannot be the only

Looking to the future, there is much to suggest that Edward A. Lynch is correct in arguing that Latin American Christian Democracy faces a watershed in how it responds in different national contexts to the contemporary challenge of economic liberalization sweeping the continent. Lynch's contribution, therefore, may prove to be timely reading for Latin American scholars and economic policymakers. Whether Christian Democrat or

other, readers familiar with Latin America will most likely find this work lively reading.

University of Notre Dame

TIMOTHY R. SCULLY

Issues in Democratic Consolidation: The New South American Democracies in Comparative Perspective. Edited by Scott Mainwaring, Guillermo O'Donnell, and J. Samuel Valenzuela. Notre Dame: University of Notre Dame Press, 1992. 357p. \$42.95 cloth, \$19.95 paper.

This volume presents some of the recent research emanating from the increasingly prominent Hellen Kellogg Institute for International Studies at the University of Notre Dame. While much has been written of late on the issue of democratization, the approach here has two advantages over many of its competitors. First, it is tightly focused geographically, confining itself almost entirely to South American cases. Some chapters focus on one country, such as Frances Hagopian's study of Brazil, others focus on a cluster of countries, such as Catherine Conaghan's study of Ecuador, Bolivia and Peru, while still others range more broadly throughout South America. Second, and more importantly, the authors all concern themselves with the same theme, namely what the editors term "the second transition to democracy." In the first transition, authoritarian rule breaks down and governments are elected by universal suffrage and under conditions of respect for civil liberties. The second transition is the process by which these elected governments are consolidated. The book then, as the title states, is a study of the process of democratic consolidation. The term takes on a clearer, more operational definition in the excellent chapter by Adam Przeworski, in which he uses the term "self-sustaining democracy," a developmental stage in which all sides have agreed to accept the inherent uncertainty of electoral contests and abide by their outcomes. Self-sustaining democracies are ones in which opposition parties have a realistic chance of gaining office, policy changes can be affected by the victors and the military is controlled by the civilians.

An even broader definition of consolidation is provided toward the end of Guillermo O'Donnell's insightful introductory chapter. By this definition, a consolidated democracy will also be one in which democratic practices extend beyond political life and into social relations more generally. This perspective alerts us to the possible difficulties that consolidating democracies may face if democratic practices do not spill over into social life beyond politics. From this perspective, Japanese democracy would appear to be safe from decay since, as we understand it, consensual democracy within the work place is so central a component of industrial success in that country. More worrisome might be the U.S. model, in which corporations operate along vertical lines of authority and command, and in which work place democracy is feared as "creeping socialism." One could make the same critique of labor union organization and even university organization in this country. To understand more clearly the implications of O'Donnell's thesis, much more work needs to be done on the connections between democratic practices in the home and on the job, on the one hand, and those practices central to the managing of the political system, on the other.

A repeated theme in the volume is the importance of the development of a system of beliefs and behaviors that support democracy. Yet the book reverses the order of causation normally utilized by those who adhere to the "civic culture" thesis. O'Donnell explicitly stands on its head Inglehart's notion of political culture, by persuasively arguing that democratic beliefs emerge from democratic practices in societies in which authoritarian elites were in control of key political positions. O'Donnell states that if democratic beliefs were a prerequisite to the emergence of democratic systems, "... then no democracy would ever have emerged anywhere in the world; in no known case does there appear to have been a majority of democrats before the advent of political democracy" (pp. 91-92). J. Samuel Valenzuela, in his thoughtful chapter ranging beyond South America to include cases in Europe and Asia, also defines the achievement of consolidated democracy as in part being that point at which there is widespread belief that non-electoral politics is illegitimate.

The papers prepared for this volume were written over a span of several years, the earliest having been written in 1987. Each presents a hopeful perspective, noting with satisfaction the number of years that democratic government has ruled in a particular country, or the number of times democratic elections have occurred. Yet every chapter places great emphasis on the things that could go wrong and the distance that South American nations must travel in order for democracy there to become truly consolidated. Conaghan's chapter shows in great detail that in Bolivia, Ecuador and Peru, electorally democratic systems engage in systematic exclusionary policy making that has left domestic capitalists uncommitted to the process. We should take a lesson from Felipe Agüero's perceptive chapter on the role of the military in the democratization process, which begins by taking note that there had not been a single case of reversal of democratization in South America. Since his chapter was written, Peruvian democracy has been extinguished by an auto-golpe, there have been repeated attempts to overthrow the government in Venezuela, and civilian rule was briefly halted in Guatemala by an unsuccessful presidential coup.

It is always risky to criticize scholars for errors of omission, especially in a volume as comprehensive as this one, but the absence of a detailed treatment of Venezuela, the oldest extant democracy in South America, is problematic. Many readers also would be happier with the volume if more of the chapters contained the detailed election data that greatly enrich the Hagopian chapter on Brazil.

The book concludes with an able summary by Scott Mainwaring. He takes careful note of the disagreements in the volume, ranging from the definitional, through the role of elites versus masses, the problem of legitimacy, and the varying modes of democratization. Many will conclude that as a profession, political science was not successful in being able to predict the rapid expansion of democracy in the world and has yet to fully understand why the first transition occurred. This volume gives us many critical signposts to follow in attempting to predict the course of the second transition, the one to stable democracy. Unfortunately, we are still

far from a comprehensive theory of transition and consolidation.

University of Pittsburgh

MITCHELL A. SELIGSON

Thinking Theoretically about Soviet Nationalities: History and Comparison in the Study of the USSR. Edited by Alexander J. Motyl. New York: Columbia University Press, 1992. 284p. \$45.00.

The unraveling of the Soviet system as a consequence of *perestroika* and the transformation of the 15 constituent republics of the Soviet Union into independent states was largely unanticipated by scholars and policymakers alike. In Moscow, as in the West, the failure to anticipate how political and economic liberalization would ignite the "national question" left analysts, as well as political leaders, unprepared for the explosion of ethnopolitical mobilization that would ultimately undermine Gorbachev's attempt at reforms, as well as the Soviet state itself.

Curiously little of this unfolding drama—a drama of world-shaking significance—is captured in this volume of essays completed in late 1990. Prompted by dissatisfaction with the alleged failure of Sovietology to approach the study of the Soviet nationality question with sufficient theoretical imagination, the editor turned to a group of distinguished social scientists "unconstrained by Sovietological preconceptions and prejudices" to show the way. He holds out the expectation that by applying their impressive intellectual powers to an issue with which they were largely unfamiliar they would illuminate the subject in novel and fruitful ways, demonstrating how "conceptual rigor, comparative perspective and explanatory vision might be incorporated into the study of the Soviet Union and its nations." While a stellar cast of authors, including Donald Horowitz, John Armstrong, Crawford Young, S. N. Eisenstadt, and Ernest Gellner have contributed a valuable set of essays, the collection does not constitute a pathbreaking new departure. Few startling insights or promising new theoretical frameworks emerge from this collection, nor even does a deeper understanding of the underlying dynamics of the different but simultaneous patterns of ethnopolitical mobilization in the Soviet system. Even more surprising is the fact that virtually none of the authors, with the notable exception of Crawford Young, considers the breakup of the Soviet Union a serious possibility. Indeed, some of the contributions make it clear that if intimate familiarity with a particular region is no guarantee of wisdom, its absence can result in inapt comparisons if not grievous errors.

Part of the problem may stem from the very focus on ethnicity and ethnic mobilization, which understandably draws on the vocabulary and the metaphors used to examine cultural pluralism in multiethnic societies. Theories of collective protest or imperial decline might have better illuminated some of the broader trends of the late Gorbachev era. Perhaps most suggestive as a point of departure for analyzing the Soviet case is Crawford Young's effort to situate the problem of ethnic mobilization in a broader economic and political context with his observation that the critical precipitants of ethnic conflict are struggles over domination and resource distribution. Indeed, Gorbachev's political and economic reforms unleashed contestation over precisely these issues, cre-

ating the context in which ethnonational mobilization would become a powerful political force.

This volume would have been further enriched by a more extensive treatment of another fundamental question, namely, how to explain differences in the scope and pattern of ethnonational mobilization among different groups. Although Donald Horowitz emphasizes the importance of recognizing and examining different types of ethnic conflict, only two essays come directly to grips with the issue. First, the joint effort by Charles Furtado and Michael Hechter to apply a theory of group solidarity to explain the divergence of the Estonian and Ukrainian cases required modification, in an addendum, to account for their subsequent convergence; the addition of a Central Asian case study might have elicited a more revealing comparative analysis. Second, a provocative essay by David Laitin, Roger Petersen, and John Slocum using a comparative analysis of the dynamics of language policy to refute the idea that the Soviet Union was vulnerable to an "ethnic time bomb" is weakened by a series of questionable assumptions about the nature of the Soviet state. Whether in puzzling over the differences between the treatment of the Russian minority in Estonia by Estonions and of Spaniards in Catalonia by Catalonians or in considering who is organizing the Russians, the authors display insufficient awareness of the very different circumstances of the two migrations and of the role of the military-industrial complex in creating the Russian settlements in the Baltic.

Finally, one of the most distinctive features of Soviet strategy for managing the "nationality question"—the creation of ethnoterritorial units endowed with the formal attributes of statehood—and its impact on ethnic identity and mobilization escapes the attention of most contributors. Indeed, Ernest Gellner's otherwise perceptive essay surely errs in asserting that modern ethnic feeling does not require for its manifestation any preexisting institutions and springs from shared culture in a "structureless mass anonymous society." The fact that ethnic mobilization in the USSR occurred most readily among those groups possessing the array of institutions conferred by republic status and especially among those which had some prior and recent experience of independent statehood (e.g., the Baltic states and Western Ukraine) would appear to challenge that assertion.

Paradoxically, some of these essays may have greater relevance to the dilemmas of state building and ethnic conflict in post-Soviet Russia and other newly independent states, which more closely resemble the multiethnic societies familiar to comparativists.

University of California, Berkeley

GAIL LAPIDUS

The Political Economy of Argentina: Power and Class since 1930. By Monica Peralta-Ramos. Boulder: Westview, 1992. 191p. \$45.95.

Argentine Workers: Peronism and Contemporary Class Consciousness. By Peter Ranis. Pittsburgh: University of Pittsburgh Press, 1992. 313p. \$49.95.

Peter Ranis's book is a valuable contribution to the literatures on Argentine politics and on workers and their consciousness. The book examines Argentine workers' perceptions of a range of interesting questions, including work and labor relations; political parties,

socialism, and private enterprise; and democracy and civil liberties.

The working class has been much discussed and often (especially in leftist circles) venerated. Although there is a reasonably abundant literature on the Argentine union movement, few quality works had been published on workers per se. This is true not only for Argentina, but for Latin America as a whole. Indeed, since political science has increasingly focused on elites and institutions, it has been true for other regions than Latin America.

In 1985–86, Ranis spent 13 months in Buenos Aires, where he conducted lengthy open interviews with 110 workers, randomly selected, from seven different unions. These interviews provide the empirical basis for Ranis's observations about workers' perceptions and consciousness. Ranis works with two primary categories of workers—laborers and employees—and shows, not surprisingly, significant differences between them.

Ranis devotes a lengthy part of the book (pp. 17-88) to discussing needed contextual background, but the most interesting material focuses on the interview findings. Even those who have long followed Argentina will learn something new and find some surprises. Among many interesting tidbits are the widespread dissatisfaction with-and hostility toward-unions, the high degree of workers' support for the Radical party, the low degree of party activism, the strong prevalence of centrist political orientations, and the positive views of foreign companies. Workers rejected class-based parties as being too divisive, and a solid majority were receptive to the privatization of Argentina's many public firms. Most workers, especially laborers, saw poverty in terms of individual shortcomings, rather than systemic causes (p. 149). Among the most appalling findings was that 30% of the laborers and 18% of employees condoned the illegal kidnappings and executions committed by the military government of 1976-83 (p. 139). Among the most encouraging was the reasonably strong support for liberal democracy, notwithstanding the failures of the Alfonsin government in offering better economic condi-

After presenting this portrait of the consciousness of Argentine workers, Ranis analyzes his findings in light of Marxist and socialist theory on class, capitalism, socialism, and related questions. Not surprisingly, Marx and his followers (except for revisionist Eduard Bernstein) do not hold up well. Ranis convincingly argues that workers share many values and ties with the well-off, that class does not form a unified or consistent basis for political action, that workers are not inexorably or even often opposed to capitalism, that workers want better opportunities within capitalism (rather than a revolutionary change), and that they are very dubious about socialism. The discussions about Argentine workers are more interesting than the reflections on Marxist and socialist theory, which are not particularly new.

In the penultimate chapter, Ranis discusses labor's response to the orthodox, private-market-oriented economic program of Peronist president Carlos Menem (1989 to present). Interestingly (and contrary to what one would expect on the basis of "collective action" problems), Ranis claims that most unions and workers have supported measures that resulted in increases in unemployment and public sector layoffs and in increasingly regressive income distribution.

Ranis evinces sympathy and empathy for workers, yet

he avoids romanticizing them. The reader senses that many findings surprised the author and pushed him to rethink some of his own initial assumptions not only about workers but also about capitalism, socialism, and democracy.

Although the merits of this book outweigh its short-comings, some of Ranis's interpretations are debatable. Ranis carefully situates his subjects in historical and political context; but occasionally, he is too sanguine about how much one can *generally* infer from interviews conducted at a given point in time. For example, to a greater degree than he acknowledges, the large number of workers who sympathized with the Radical party was influenced by the moment when he did his interviews: the radicals were then enjoying greater prestige than has been usual over the past half-century, while the Peronists were at their nadir.

Ranis argues that most workers are truly committed to democracy; and in so doing, he usefully points to democratic proclivities in a working class often characterized as authoritarian in the past. Yet the facts that 20% of his sample stated that Argentines should have less freedom than they do (p. 163), that most workers agreed that some social groups might take advantage of too much freedom (p. 162), and that only 68% preferred democracy as a form of government (p. 152) suggest that adherance to democracy was far from uniform as of 1985-86. At times, Ranis excessively praises Peronism, glossing over its authoritarian character, as well as the deleterious impact of Peronist economic policies. He overstates the democratic character and consequences of the current government of Carlos Menem. The empirical basis for his claims regarding workers' reactions to Menem (new interviews in 1990 with eight of those who were interviewed in 1985-86) is not as compelling as that for his observations on the Alfonsin period. But these are minor quibbles with a fine book.

Like Ranis, Peralta-Ramos is an experienced analyst of Argentine politics; and she, too, uses class as a central category. But in other respects, these books diverge. Peralta-Ramos examines Argentina's political and economic developments since 1930 in light of class relations. The book focuses on Argentina's difficulties in achieving democracy and promoting development, particularly in terms of that country's class relations. The second half of the book, which provides an analysis of the 1983-90 period, is the more useful. In general, her original research contribution is limited; and the empirical evidence for some assertions is thin, especially for the 1930-83 years. Peralta-Ramos sees economic policy as being determined by the need for capital accumulation. Although there can be no doubt of the need for capital accumulation in capitalist societies (or, for that matter, any other societies that want to promote economic growth), the way in which this need drives economic policy is more complex than what is presented

University of Notre Dame

SCOTT MAINWARING

The Limits of Social Democracy: Investment Politics in Sweden. By Jonas Pontusson. Ithaca: Cornell University Press, 1992. 261p. \$28.50.

During the late 1970s and early 1980s, the efforts of a handful of left-wing Swedish intellectuals and their talented allies in Britain and North America revitalized the study of parliamentary socialism. Against the currents of both pluralism (for which class politics was a non sequitur) and neo-Marxism (for which social democracy was a sham), these scholars perceived the contemporary radicalization of the Swedish Social Democratic party (SAP) as signaling its long-awaited maturation. Bold new legislative initiatives to democratize the management of firms and collectivize control of investment promised to move social democracy beyond the limits of party politics and redistributive policies, into the economic arena and a direct attack on the mainsprings of capitalist power.

Ten years ago, Jonas Pontusson, then a graduate student at Berkeley, launched a trenchant attack on these optimists in the pages of the New Left Review. Pontusson pointed out that changes in global economic conditions and attendant economic restructuring had altered the material conditions for the pursuit of the social democratic program in ways that the optimists, with their essentially voluntarist conception of political economy, were unable to see. Even if political creativity or will was a sufficient condition for reforming capitalism, there was not enough of it in Sweden to cross the threshold to radicalism. Moreover, Pontusson's ongoing research into how Sweden's existing mechanisms of public steering of private investment actually operated showed that in practice, they functioned very much in conformity with the criteria dominating private capital accumulation.

Despite his vindication by subsequent events, Pontusson's book, a retrospective on Swedish labor's multipronged "industrial policy offensive" of the 1970s, is much more than a victorious "I told you so." Pontusson is conscious of the fact that the empirical record is too untidy to warrant a one-dimensional postmortem. While the transition to socialism anticipated by optimists like Walter Korpi and John Stephens was thwarted, it is nevertheless true that for a time, the SAP's programmatic objectives were radicalized in ways that pessimists like Adam Przeworski regard as incompatible with the structural power of capital and the political dilemmas confronting social democrats. The record shows that the SAP indeed failed to achieve-or even seriously advocate—either a planned economy or the gradual collectivization of capital envisaged in the Meidner Plan. Nevertheless, during the long period of postwar Social Democratic government, the Swedish state did achieve a good deal of control over labor markets, within a regulatory framework in which organized labor was decidedly ascendant. Pontusson argues that these polar experiences should be seen as the ends of a continuum, with two other noteworthy forays into "production politics" (industrial policy proper and workers' participation in management) located somewhere in between. The task that Pontusson sets himself in The Limits of Social Democracy is to chronicle what happened to each one of these initiatives and to make analytical sense of the variations in the extent to which they actually succeeded in altering the power and practices of Swedish capitalism.

By focusing upon empirical variation, Pontusson moves us beyond simplistic debates over the "success" or "failure" of reformism. His analytical framework is equally nuanced. The structural power that comes from private ownership and control of investment capital is recognized, but so is the potential political potency for business of collective action and influence over the

levers of public opinion. The organizational and political power of the labor complex is afforded due weight; but so are the imagination and determination of labor's leaders, internal cleavages between party and union elites, and the scope for conflicts of interest between workers in different occupations and sectors. The dynamics of state intervention into markets and production are similarly unpacked, with a distinction made between policymaking and policy implementation and due attention paid to how the institutionalized policy commitments of the past shape and limit options for the present. While none of these nuances is especially innovative by itself, Pontusson's contribution is to meld them together into a coherent perspective that preserves a prudent balance between interpretive parsimony and an empiricist demonstration that everything matters.

Six substantive chapters are sandwiched between the introduction and conclusion. Chapters 2-4 chart, in chronological order, the development of social democratic strategies and policies of economic intervention from the SAP's rise to power through the 1950s. The reader is familiarized with the key instruments of economic steering with which Swedish governments confronted the economic upheavals of the 1970s and beyond and with the challenges these upheavals posed for the existing mold of labor reformism. Chapters 5-7 chart both the content and the fate of the three policy "offensives" that represented labor's responses to these challenges: (1) initiatives during the 1968-73 period aimed at facilitating "selective state intervention in the restructuring of capital at the level of firms and industrial sectors" (p. 127), an agenda that failed; (2) the campaign for industrial democracy mounted by the LO (peak organization of blue-collar unions) in the 1970s, a genuine challenge to "managerial prerogatives" that resulted in some heavily circumscribed moves in the direction of codetermination; and (3) the 1971 plan gradually to collectivize share ownership of large and profitable private corporations, the brainchild of a committee led by LO economist Rudolf Meidner, which was enacted in very watered-down form in 1983.

These six chapters are full of meaty information and perceptive observation. In spanning both long-term historical developments and the multiple targets of the 1970s reform spurt, the book has enormous value simply for having furnished the factual record, hitherto largely inaccessible except to Swedish readers and, even for them, fragmented among multiple sources. But beyond this, the rationale for such a wide and deep account is, of course, analytical and political, as a test of the limits of social democracy. Pontusson's praiseworthy attempt to sift the evidence by pitting multiple explanatory factors against multiple empirical cases falls short of yielding a clear explanatory hierarchy. What does emerge rather clearly is that Swedish social democracy went about as far as it could in the 1930s, bequeathing a legacy that was expanded and consolidated in postwar reforms but has never really advanced into the realm of "allocative investment decisions." The most interventionist measure of the postwar period—active labor-market policyworked for a long time because, in conjunction with 'wage solidarity" and other measures, it promoted the mobility of both capital and labor on terms that were advantageous to the leading sectors of private industry. The strengths of this type of compromise are also its limitations: it was founded on (contingent) mutual interest, not fundamental alterations to the workings of the capitalist economy; and it was advantageous to both workers and business in certain sectors, rather than to a labor class, in opposition to a capitalist class.

Commenting, in a similar vein, on the fate of later attempts at reforming industrial policy and industrial relations, Pontusson explicitly observes that "in both cases labor's reformist ambitions were implemented through and thwarted or deflected by the politics of class compromise" (p. 184). This observation matches well with others, such as the observation that the progress of so-called pension-fund socialism was stymied by the social democrats' concern to maintain business confidence and their long-standing commitment to allocating capital through efficient markets (p. 93). This is not to say that changes in LO and SAP policies and their origins in altering economic and political circumstances are not without interest. Moreover, the comparative framework of the study does yield some important insights. For instance, it turns out that codetermination was blocked primarily at the point of implementation, while collective shareholding ran aground already at the legislative stage. Whereas the common interests of blueand white-collar labor and relatively mild opposition from organized business permitted codetermination to become law, reformism was stymied by its own pretensions in the case of the Meidner Plan, which "posed a more direct threat to the systemic interests of business" (p. 229). Different aspects of the same systemic power also hampered labor's adoption of a coherent "alternative model of development" that could have guided its struggle for a farther-reaching industrial policy.

Yet despite the influence of business on labor's strategic thinking and its capacities for mass mobilization and despite the necessity for reformists to offer positive incentives to at least some sectors of business, Pontusson refuses to fall back on a one-dimensional explanation (or political defeatism): "The systemic power of business can be curtailed through legislation, and the politics of legislation contains an important element of indeterminacy" (p. 236)—a judicious conclusion to an important study.

Hebrew University

MICHAEL SHALEV

Labour and the Political Economy in Israel. By Michael Shalev. New York: Oxford University Press, 1992. 400p. \$69.00.

Many Israelis mourn the collapse of their country's idealism. Once, their national enterprise was characterized by the determination to create not only a Jewish state but also a just society. Now political leaders proclaim their allegiance to economic enrichment and to winning the battle for global competitive advantage. Labor Zionism once provided the vision and social energy to revitalize a culture, resettle a country, and defend its borders. This vision no longer grips the imagination of Israelis, nor is social change central to the political agenda. But Labor acquired significance in Israel's history not simply as the banner of the vanguard but also because of its political dominance. The passing of the old guard and of its idealism has not only occasioned distress, it has also provoked a reexamination of Israel's power structure. How close a correspondence were actions to creed? Did workers control the instruments of political power? Did powerful institutions truly represent workers' interests? Or was the effort to establish a vigorous egalitarian society marred by the external challenges to Israel's existence and survival? In his masterful study, Labour and the Political Economy in Israel, Michael Shalev addresses these questions from the perspective of political economy. Not too long ago, the questions that Shalev poses would have raised a storm of debate among scholars, with many likely to dismiss this kind of analysis as an attack on Israel's right to exist. Like many other serious students of Israeli society, Michael Shalev shows that Labor Zionist ideologies did not shape policies. What differentiates this book from other accounts are the reasons Shalev proffers for why the Labor Zionist elite structured society in one way and interpreted it in quite another.

Of utmost importance is the central premise out of which Shalev develops his analysis, namely, that Israel's Labor party is no longer assured of a commanding role in government because it pursued policies that weakened, rather than strengthened, its matrix of institutional supports—foremost among them the Histadrut, the union under whose rubric most workers in Israel are organized. The leaders of the major Israel Labor Zionist political parties (Mapai and the Israel Labor party) have had long and close relationships with their counterparts in the Histadrut. This relationship, Shalev argues, has elevated the institutional interests of party and union over the needs of workers.

In observing the weakening of the Histadrut and the Israel Labor party in recent decades, scholars have searched for signs of weakness in earlier periods. Shalev finds the source of this weakness in the Histadrut's role in Mandatory Palestine. The Histadrut demanded work for Jewish laborers and thus established a priority for pursuing national, over class, interests. This observation, by itself, is not remarkable. But Shalev insists that the national agenda became an essential element in the conflict between Arabs and Jews in Palestine. Moreover, in Shalev's analysis, the subordination of class interests left its imprint on subsequent political configurations, ultimately constraining Histadrut options and strategies. In Mandatory Palestine, the exclusion of Palestinian Arab workers from Histadrut membership generated ample rewards accepted by the organization in adherence to the principle of "moving from class to nation." That stance joined the Histadrut's viability to the political interests of Mapai. In the transformed circumstances of sovereignty, when it became useful for Mapai to pursue policies consistent with the legal equality granted to Israeli Arabs, the Histadrut organized Arab workers but, as Shalev observes, also kept them in line by controlling access to employment.

This symbiotic institutional arrangement generated temporary advantages for political party and Histadrut in the context of full employment and a rapidly expanding economy but engendered serious damage during economic contractions. In prosperous times, the Histadrut had trouble controlling all its members. The Histadrut's integrating function was increasingly challenged by groups better able to extend their benefits on their own. In times of economic hardship, the coalition of labor and capital was held only by advancing policies that hurt workers. The extreme reliance of both industry and agriculture on government subsidies enabled the leadership to use allocations as an instrument of political control. Recessionary policies bolstered the Histadrut's waning control over labor, but only in the short run.

Eventually, they contributed to the organization's loss of legitimacy.

A sluggish economy took off in the aftermath of Israel's victory in the June War, in 1967. Israel's occupation of the conquered territories and its administrative control over a greatly enlarged Arab population permitted its political leaders to resume their older orientation of providing subsidies to capital and creating jobs for workers. Economic expansion (interrupted for half a decade) resumed but was soon sapped by the costs of continued war and dramatic alterations in world market forces. Israel's labor movement institutions could never fully satisfy its several constituencies. And without substantial economic growth, the labor movement could no longer function as an effective electoral instrument. Failing to mobilize enough votes in the general election of 1977, the Labor party lost its dominant position in the government and its hegemony over the Histadrut.

Labour and the Political Economy in Israel is an interpretive essay, rather than a conventional history; and the book's thesis is complex and controversial. It will draw fire from many specialists who will find some events and developments difficult to fit into an argument that ties economics and politics so tightly together. Readers who are searching for an introductory text on Israeli politics may find the thesis difficult to understand or appreciate. But for those well versed in the conventional interpretations, this book is essential reading. Although amendments, even corrections, may be offered to some aspects of the analysis, Shalev's framework for understanding Israeli political and economic development is likely to retain its vitality.

Smith College

DONNA ROBINSON DIVINE

Presidents and Assemblies: Constitutional Design and Electoral Dynamics. By Matthew Soberg Shugart and John M. Carey. New York: Cambridge University Press, 1992. 316p. \$54.95 cloth, \$16.95 paper.

Since the mid-1970s, dozens of countries have become democratic, some for the first time and some after an authoritarian interlude of long or short duration. In each of these countries, many choices about the large and small details of the institutional form democracy would take had to be made. In response to these events, scholars have renewed their interest in the consequences of different institutional choices for representativeness, economic policy performance, and political stability, to name some of the most important. This book, by Matthew Shugart and John Carey, makes an important contribution to the debates over the effects of different political institutions.

It takes issue with an idea that has almost achieved the status of stylized fact: the idea that parliamentary systems are inherently more stable than presidential systems. The two main arguments that support this idea are that parliamentarism prevents the intense conflict that can arise between the executive and the legislature in presidential systems, and that parliamentarism provides a mechanism for removing unsuccessful governments without overturning the regime. Since seemingly unresolveable conflict between the president and congress as well as government failure in the economy have played key roles in many democratic breakdowns, these arguments have strong initial plausibility.

Shugart and Carey do not challenge these arguments head on. Instead, they first show empirical evidence that presidential systems have not broken down more frequently than parliamentary systems. Most of the remainder of book is taken up with demonstrating the enormous institutional variety that occurs in what we commonly call presidential systems, and how different institutional details interact to affect party fragmentation and the likelihood of executive-legislative conflict, factors that in turn affect the quality of government and likelihood of political stability.

In their analysis of the frequency of breakdowns in presidential as opposed to parliamentary systems, Shugart and Carey show that for the twentieth century as a whole, about the same number have occurred in each type of system. Most breakdowns of presidential systems happened after World War II, and they were especially likely in Latin America (the region with the highest incidence of presidential democracies). Most breakdowns of parliamentary systems occurred either in Europe before World War II or in Britain's former colonies (the two groups with a high incidence of parliamentarism). The predominance of presidential systems among more recent breakdowns seems to account for the general perception that presidential regimes are more fragile. Since Shugart and Carey do not provide denominators (that is, they tell us that 22 parliamentary systems have broken down, but they do not tell us how many parliamentary systems over how many years there have been altogether), we cannot tell which kind of system is really more unstable. But one conclusion emerges straightforwardly from their data: newer democracies are more likely to suffer breakdowns than older ones, so whichever form is currently more popular among emerging democracies will appear to be less resilient. During recent decades presidentialism has attracted most adherents and, consequently, more breakdowns have occurred in presidential systems.

Most of the book is devoted to explorations of the effects of institutional variations within presidential systems. Prominent among these is the discussion of the effects of the timing of legislative elections on party fragmentation and the likelihood of conflict between the president and the legislature. Shugart and Carey show convincingly that non-concurrent elections increase party fragmentation. They also show that concurrent and especially honeymoon (that is, within a year of the presidential vote) congressional elections increase the probability that the president will face a supportive legislature; that midterm elections on average lead to a substantial reduction in legislative support for the president; and that a congressional election schedule different from that for the president increases the likelihood of divided government and, consequently, of conflict between the branches of government and immobilism.

Since intense conflict between a president from one party and a legislature dominated by others was a major cause of the political crisis and economic chaos that precipitated military interventions in several countries during the 1960s and 1970s, these conclusions about the effects of the timing of elections have obvious implications for political stability. Timing helps explain the instability of some presidential systems without implicating presidentialism per se. Here and elsewhere, the authors show that features characteristic of some presidential systems contribute to instability, but that presidential systems contribute to instability, but that

dential systems that lack these institutional features are at least as stable as parliamentary regimes.

Not all of the authors' arguments are as persuasive as the one about the timing of elections, however. The claim that the combination of weak presidents and strong parties increases democratic stability seems especially problematic. It rests on an arbitrary definition of democratic "success" as absence of breakdown since the constitutional or legal establishment of a particular system. This definition leads to counting Venezuela, democratic from 1958 to a 1990 cut-off point (though as of this writing seriously threatened by a coup), as more stably democratic than Chile or Uruguay, democratic according to the authors' unrestrictive definition from about 1900 to 1973. It is really not obvious that Venezuela's 32 years should count as greater stability than Chile and Uruguay's approximately 70 years. Nor is it obvious that Venezuela's 32 years should count as less, since Venezuela may remain democratic for the rest of human existence.

If instead of using the authors' no breakdown rule to define successful stability, we define it as, say, 30 or more years of uninterrupted democracy, we find little difference between cases with strong presidents and weak parties and cases with weak presidents and strong parties. Among developing countries, there are three stable democracies in the strong presidents-weak parties category (Chile, Colombia, and Uruguay) and three (Venezuela, Costa Rica, and the Dominican Republic) that have, according to Shugart and Carey, weak presidents and strong parties. In addition, in each category there are some conspicuously unstable countries, some that most would call authoritarian, and a substantial number that have not been democratic long enough to allow a judgment.

Most readers of this book will probably disagree with some of the arguments, and country specialists will find some of the interpretations odd. I suspect, for example, that most Venezuelan specialists will be surprised to find the Venezuelan president called weak, though they may acknowledge that he has few formal legislative powers. Nevertheless, this is a tremendously useful and thought provoking book. No one who reads it will see the debate over parliamentarism versus presidentialism in the same light again. It contains an impressive amount of detail about political institutions in a remarkably large number of countries. Most important, it takes a serious look at the effects of a number of institutional variations that have not been treated carefully by other analysts, and reaches conclusions that go considerably beyond the classic works on the subject.

University of California, Los Angeles Barbara Geddes

The Political Economy of Foreign Investment in Mexico: Nationalism, Liberalism, and Constraints on Choice. By Van R. Whiting, Jr. Baltimore: Johns Hopkins University Press, 1992. 313p. \$55.00.

Van Whiting seeks to illuminate the past two difficult decades in Mexico, the dynamics of foreign investment in industrializing countries, the determinants of policy choices by governments in newly industrializing countries, and the character of adequate explanation of social action. To accomplish this, he draws on case study research by a variety of scholars, interviews he has

conducted with corporate and government officials, and a diverse array of theoretical works.

Whiting is interested in exploring how effectively Mexico (or any industrializing country) can steer or regulate foreign investment. After an initial chapter that announces his main concerns, he commences his argument with a demonstration that Mexico has a strong state: only in such a circumstance could failures of state action credibly be laid to structural constraints rooted in the organization of the world economy. He convincingly shows that Mexico has not only a strong state but a "resilient" regime, one that can absorb shocks but persist nevertheless (p. 43).

There follow four chapters providing a comprehensive account of the varied and changing strategies of foreign investment regulation in Mexico during the 1970s and 1980s: import substitution; promotion of manufactured exports; restrictions on foreign ownership; and regulations concerning technology, patents, and trademarks. Case studies of the successes and failures of state efforts to regulate foreign investment in automobiles, computers, and food processing comprise the next three chapters. A final chapter draws the various stands of the argument back together.

For most of the century, Whiting chronicles, a nationalist "metapreference" guided Mexican government policy, which became steadily more sophisticated and differentiated. The 1970s saw a marked increase in initiatives directed toward foreign investment; and by the end of 1980s, a liberal "metapreference" had come to hold the upper hand. Mexican officials had dramatically reversed course to accept judgments of markets, especially internationally organized ones.

In part, Whiting attributes this shift to the failure of nationalist policies "not only by liberal standards but in terms of the very nationalist goals [the policies were] meant to achieve" (p. 53). Most interesting and most compelling here is Whiting's demonstration that Mexico had been unable to develop an adequate domestic capacity for innovation to break reliance on foreign-developed technology. He also outlines the failure of nationalist policy to mobilize adequate financing for development.

Were these failures inherent in nationalist policy? This would be more difficult to show; and on the whole, he does not show it. A similar issue arises in his case study of food processing. Whiting chronicles the ineffectiveness of government policy to contain foreign investment or to maintain competition in the sector. He concludes that "the failure of regulation to change structure helps explain the [subsequent] shift to liberalization" (p. 190). But his account of the failure shows that the government never took the steps that would have been most effective: How can he conclude that a more forceful and savvy policy would not have succeeded? Later, he acknowledges, "My bias is to assume informed choice rather than either irrational change or what Hirschman calls akrasia, weakness of will" (p. 227). In food processing, at least, weakness of will appears to be a more compelling hypothesis.

Even more important than failures of nationalist policy, Whiting attributes the overall shift from nationalism to liberalism in Mexican policy to recent changes in the structure of the world economy, particularly to the decline of U.S. hegemony and to internationalization and increasing competitiveness in a range of industrial sectors. This contention is announced early in the book

but frontally engaged only at the very end (p. 239). In advancing it, he also means to sketch an explanation for the rise of "southern liberalism," that is, a similar shift in other countries (p. 147).

This is a strikingly novel argument, but he does not convincingly explain why increased international competition in many industries should encourage a shift to liberal policy. It is certainly credible that the internationalization of production has created niches that industrializing countries can occupy. But why does it not also give more bargaining leverage to the countries, like Mexico, with large domestic markets, abundant natural

resources, and compliant labor?

Whiting does identify other factors that might also explain the shift from nationalism to liberalism: the sapping of all leverage in bargaining with foreign investors by the pressure of Mexico's severe debt predicament; the capture of the allegiance of Mexico's best and brightest by changing currents of international ideology (the resurgence of market dogma in the Reagan years; and the failure of Mexico's domestic bourgeoisie to sustain investment, thus constraining the government to rely more on direct foreign investment to provide exports and jobs. From the outset, he frames his presentation in terms of hypothesis testing; but he does not provide evidence why these factors should be seen as less important than changes in world industries.

In raising these issues, I mean principally to honor the book's considerable strengths. Whiting never wavers from pursuing large questions of fundamental importance. Any work addressing these issues would be similarly challenged because arguments about what might have happened are essentially contestable. In this regard, I particularly applaud his insisting that these questions are best addressed as ones involving both choice within constraints and the mobilization of power to transform structural constraints. He draws together, coherently, an immense amount of material. The book serves as a grand retrospective on two decades of research by others in several different traditions, and there are skillfully researched and important additions of his own across the broad fabric of his argument.

Finally, Whiting well marks the most important present issue regarding Mexican public policy, now guided by the liberal "metapreference": "The opportunity resulting from the crisis of the 1980s was for Mexico to become a global player, an intermediate power, in an interdependent world economy. The danger of the liberal vision is the failure to see within, to understand the implications of the liberal program for those marginalized by the crisis" (p. 236).

Oregon State University

Douglas C. Bennett

INTERNATIONAL RELATIONS

Singular Europe: Economy and Polity of the European Community after 1992. Edited by William James Adams. Ann Arbor: University of Michigan Press, 1992. 388p. \$39.95.

Singular Europe is a collection of articles dealing with issues of European Community integration that arose out of a series of seminars hosted by the Chicago Council on Foreign Relations in 1991. The collection includes articles on a variety of topics by 14 different European and American authors drawn primarily from law, economics, business, and international relations. The authors have provided solid analyses of the developments leading up to Project 1992, as well as explorations of the problems and likely trajectories of the policy agenda for the next decade. Despite the innumerable political, social, and economic changes that have occurred in just the short time since the articles were first written, the book remains timely and relevant to current political science scholarship concerned with such wideranging topics as democratic theory and federalism, regulatory policy, social welfare, and foreign policy. Even though the book encompasses a wide range of disciplines, perspectives, and substantive foci, the articles in the book are accessible to the nonexpert in a particular field or policy specialization.

William James Adams, the editor of the volume, has thoughtfully organized the book into three parts, each with a useful introduction. In each introduction, he attempts to tie together the articles in the part by relating them to a basic question or two; and he offers brief commentary on key arguments to follow. Although recognizing that Project 1992 was not intended to eliminate social, economic, and regional variations among member countries, part 1 nevertheless addresses questions about what more needs to be done on the path to integration. It contains articles by Joseph Weiler on political union and problems of democratic deficit and legitimacy, Ernst-J. Mestmäcker on German unification and its effect on the development of the community, Tommasò Padoa-Schioppa on the necessity for monetary (though not fiscal) union, Frédéric Jenny on mergers and the nature and adequacy of competition policy, and Stephan Leibfried on the extent of social policy and the need for a more developed welfare state. Each of these pieces makes excellent contributions to furthering understanding of the extent of integration to date and many of the problems which will need to be solved in the years to come. Especially noteworthy in part 1 is Weiler's cogent account of the nature of democratic deficits in the institutional arrangements of the EC and the importance of distinguishing these deficits from lack of political legitimacy. He offers a note of caution about the ironic possibility of the EC's becoming a superstate, promoting nationalism when its historic roots lie in the attempt to solve the problems of excessive nationalism. In addition, the Leibfried article is noteworthy for its helpfulness in stretching one's thinking about the possible parameters of future EC social welfare policy by offering, for consideration, four possible schemes for the provision of social welfare (two incremental reforms and

two much more major reforms) that could be developed over the next decade.

Next, part 2 addresses itself to how far the EC, through new legislation, regulatory policies, and administrative actions, has succeeded in altering business behavior in the direction of a more competitive and pan-European marketplace. It includes articles by Alex Jacquemin on the increasing tendency, in restructuring and rationalizing efforts, toward mergers, acquisitions, and joint ventures and the role of commission oversight in the area of competition; Bo Carlsson on the increasing tendency toward small business among most industrialized nations (including most of the EC) and its implications for community policy directed at small enterprises; Gunter Dufey on changes in financial markets both predating, and following from, Project 1992; Reinhard Ellger on the effects of EC legislation deregulating the telecommunications industry on firms in various telecommunications sectors; and Severin Borenstein on liberalizing air travel and the constraints posed by political, institutional, and market factors. The Ellger piece is particularly valuable for its extremely detailed examination of specific pieces of EC legislation and how they have been applied to specific firms in different countries. Especially interesting in this context is the discussion of the treatment of state-owned firms. Each of these articles in different ways helps to further understanding of how EC economic policies (especially regulatory policies) have been affecting, or are likely to affect, the behavior of the market. However, with the exception of the Carlsson article, part 2 is heavily weighted toward consideration of the knock-on effects of regulatory policies on business. There is almost no discussion of EC industrial policy initiatives, such as ESPRIT and EUREKA, or European Bank for Construction and Redevelopment involvement in regional policy.

Finally, the articles in part 3 address the relation of the EC to other nations, with a special focus on whether the EC is or is likely to become a "fortress Europe" protecting itself against outsiders. Part 3 contains articles by William Wallace on the changing international context in which the EC is coming to play far more powerful roles in trade negotiations and foreign policy than in the Cold War era; John D. Steinbruner and Susan L. Woodward on the need for both the United States and the EC to involve themselves more directly in Eastern Europe in the interests of promoting political and economic stability in the area, as well as world peace; John Jackson on the EC and its external commercial relations, with a primary focus on rounds of the General Agreement on Tariffs and Trade (GATT) and problems with the United States; and Gary Saxonhouse on the failure of Trilateral commercial policy and EC trade relations with Japan. The articles in part 3 nicely round out the effort to get a picture of the distance that the EC has come and the distance it still has to go with respect to becoming a major world player, by moving away from the discussion of internal matters to examining some of the major external issues confronting the community. The Saxonhouse article differs from most of the rest of

the articles in this volume by virtue of its quantitative rigor. He removes the discussion of barriers to trade on theke parts of the EC, the United States, and Japan from the realm of speculation and rhetoric to a neat empirical examination. He offers a sophisticated quantitative model for determining the extent of trade distortions among the EC, the United States, and Japan and argues that there is a discernible pattern of EC discrimination against Japan.

Not all of the pieces articulate with one another well enough to add up to a well-defined whole. Although authors occasionally speak to one another, for the most part, they do not. Because there is overlap in the topics and concerns of the articles, one finds oneself wishing sometimes for more dialogue, sometimes for just less repetition. The book might have been better pulled together by the inclusion of concluding chapters summarizing the articles in each part, which would have allowed the reader to perceive more readily what, if any, general points or themes had emerged in answer to the basic questions posed in each part. This is not, however, to ask for some overarching theory, either for the book or for its separate parts, that would serve to explain past developments and help to predict the future. The authors can be forgiven for neglecting grand-theory building not only because of the wide-ranging topics and disciplines covered in the book but also because most of the articles have presented excellent descriptive and empirical data and perceptive analysis that will help to provide a basis for subsequent theory building.

Besides providing a wide range of audiences with a number of very good analyses of the EC's move to integration, the book may also help many scholars overcome their recent fear of engaging in intellectual projects related to the EC. In the early run-up to 1992, political scientists and members of other disciplines made a mad dash to tackle the intellectual issues and problems involved in the movement toward greater European union. No sooner was the dash fully under way than events such as the reunification of Germany, the ratification of Maastricht, GATT battles, and the disintegration of Yugoslavia appeared to have engendered a crisis of rising expectations among scholars. So certain had political scientists been of the coming into being of a powerful new economic and governmental colossus that at the first signs of trouble, a disproportionate amount of pessimism set in. Political scientists have thus been increasingly reluctant to want to take on long-term EC work. This book stands as a good exemplar of the fact that if one poses fundamental questions and engages in thorough analysis, the staying power of the scholarship is likely to continue.

In short, this book contains well-written, accessible articles that will make it valuable for advanced students, as well as scholars. The articles contain a wealth of background information on the history and institutional machinery of the EC and on the intricacies of economic policies and foreign relations that will allow novices a chance to acquire a great deal of knowledge about this new governmental and economic order. At the same time, this is anything but a primer; scholars interested in the EC, as well as in many other areas of politics and economics, will find the book thought-provoking and quite helpful in their research endeavors.

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CANDACE HETZNER

States and Provinces in the International Economy. Edited by Douglas M. Brown and Earl H. Fry. Berkeley: Institute of Governmental Studies Press, 1993. 247p. \$24.95 paper.

Although federalism in Canada and the United States might seem an obvious topic for comparative research, it has not been a fashionable one in recent years. Canadians, preoccupied with Quebec nationalism and its challenge to the viability of the Canadian state, tend to regard their own federal system as *sui generis*. American scholars, apart from such notable exceptions as Samuel Beer and Danial Elazar, have implicitly and sometimes explicitly reduced federalism, under the less than stimulating rubric of "intergovernmental relations," to a sub-field of public administration. In both countries, furthermore, political science is so structured and organized as to create a fairly rigid boundary between the study of one's own government and the more exotic field of comparative politics.

Despite these obstacles, a Canadian-American Federalism Project has quietly proceeded for several years under the auspices of the Canadian Studies Program of the University of California at Berkeley. The present volume of essays, one of three that have resulted from the project, should interest students of international relations as well as those specializing in the domestic politics of either Canada or the United States. As the title suggests, it focuses mainly on the efforts of sub-national governments to attract foreign investment and gain access to foreign markets. However, their impact on non-economic aspects of foreign policy is also considered, particularly in the last two chapters.

Perhaps confirming the observations made in the first paragraph of this review, few if any of the chapters in this multi-authored book are truly comparative. Four American-authored chapters are devoted exclusively to the United States while three Canadian-authored chapters are devoted exclusively to Canada. In addition there is an introduction summarizing and comparing the findings of the various chapters, a theoretical chapter by Panayotis Soldatos (chapter 2, although it would have been more appropriately placed at the beginning of the book), and a case study by Martin Lubin of transborder regionalism in the northeastern corner of North America

Efforts by sub-national governments to influence foreign policy, or to conduct their own, are not an entirely new phenomenon. (The Foreign Relations of the Federal State by Harold W. Stoke, a neglected classic of comparative government, was published more than 60 years ago.) Quebec conducted extensive relations with France and other francophone countries in the 1960s, to the dismay of the Canadian federal government. Nonetheless, the extensive involvement of North American subnational governments in the international economy is primarily a phenomenon of the last twenty years.

International interdependence is more of a popular cliche than an adequate explanation for this phenomenon. A better explanation is the relative decline of North American competitiveness vis-a-vis both Europe and East Asia, and the resulting scramble for jobs, investment, and markets. The end of the baby-boom prosperity that lasted for a generation after the Second World War also undermined the credibility of demand-oriented macroeconomic policy (which required strong central governments) and shifted attention towards supply-

oriented microeconomic policy, which states and provinces could conduct more effectively than governments in Ottawa or Washington. In the 1980s, finally, the conservative governments of Reagan, Bush, and Mulroney were less inclined to resist the decentralizing trend than their predecessors.

The consequences of these developments are effectively described by Brown, Fry, and their collaborators. They include a proliferation of state and provincial offices overseas, not to mention provincial offices in the United States. Provincial governments were heavily involved in the Canadian side of the Canada-United States free trade negotiations. States and provinces have competed against one another to attract major investments, notably from Japanese automobile producers. Texas has sent observers to OPEC meetings and intervened, possibly to the detriment of U.S. interests, in the dispute between the United States and Europe over European restrictions on beef imports. State and provincial governments have boycotted imports from countries of which they disapprove, such as South African wines.

Even apart from Quebec nationalism, which is rather neglected in this collection of essays, some differences are apparent between the two countries. Municipal and local governments have much less autonomy within Canadian provinces than their counterparts within American states; in fact Canadian cities are prevented by law from offering direct financial incentives to investors. Canadian provinces orient their external activities towards the United States much more than American states orient theirs towards Canada. No American state has developed a "foreign office" as impressive as those of Quebec or even Alberta. On the other hand the provinces seem less inclined than the states to adopt political positions on such issues as nuclear disarmament, Northern Ireland, or the Middle East.

Generally speaking, the Canadian contributors to this volume regard the external activities of the provinces as beneficial, while the American contributors tend to have reservations about the desirability of state and local involvement in external relations. Most contributors of both nationalities seem to accept the inevitability of decentralized foreign policy, at least in the economic realm, although Kim Richard Nossal rightly warns against premature obituaries for the nation-state, in Canada or elsewhere.

In any event, the irreversibility of present trends should not be taken for granted. Since the manuscript was completed, Ontario has closed all of its external trade offices as an economy measure. The return of a Democratic administration in Washington, and now of a Liberal government in Ottawa, may lessen the tolerance for sub-national diplomacy on the part of both central governments. Whatever happens, however, this volume will remain a useful source of information on a significant aspect of the political economy of North America.

Brock University GARTH STEVENSON

The Logic of Anarchy: Neorealism and Structural Realism. By Barry Buzan, Richard Little, and Charles Jones. New York: Columbia University Press, 1993. 267p. \$40.00 cloth, \$15.50 paper.

This is an impressive but also, in a way, a curious book. It is impressive because it represents one the most

serious sustained theoretical efforts of the last few years in regard to the structural realist paradigm. It is curious because many a reader might raise the question why the authors, who are leading exponents of the British international relations school, bother (in the words Charles Jones himself) "to continue busily fitting a copper sheath to our man of war while the submarines slink by," that is, try to fix a research program at a time when the era of structuralist theory in the social sciences seems to come to an end.

These initial suspicions are further enhanced by the foil that the authors use to revive the interest in a structuralist theory of international relations. Given their interest in a theory of international relations, rather than merely of international politics, and given the mounting criticisms of the rigidities, inconsistencies, and rather limited explanatory power of the predominant Waltzian approach, one wonders why the authors have chosen the Theory of International Politics as a point of departure, instead of starting de novo. Despite some of the brilliant insights that Barry Buzan conveys in his contribution addressing the issue of "Rethinking System and Structure," a critical reader is likely to be less than enamored with the long exegetic passages of the tediously familiar Waltzian text. The fact that such exegesis recedes more to the background in Richard Little's contribution on "Rethinking System Continuity and Transformation," and plays even less of a role in Charles Jones's "Rethinking the Methodology of Realism," gives an important clue as to the strategy of persuasion that the authors have used for making their theoretical point that the structuralist realist research program is well and quite promising as soon as the fetters of neorealism (being a particular version of structural realism) are left behind.

The emphasis on persuasion as the appropriate mode of theorizing about human action in general and its various modes (political, economic, action, etc.) is by no means accidental. As Jones indicates, the neorealist attempt to marry structuralism to a largely positivist methodology has misfired and is to blame for the considerable incoherence in Waltz's approach, ranging from the problematic nature of his analogies (in particular, the market analogy) to the difficulty with "testing" his brand of theory or even specifying criteria for application. The careful examination of the contradictory implications of, for example, the pragmatic and representational elements in Waltz's "theory" provides much food for thought and explains the often question-begging nature of the Waltzian enterprise. Given Waltz's instrumentalist stance of theory building (theories are to be assessed not so much by the truth of their assumptions as by their usefulness), this pragmatic stance (imported from Friedman's writings in economics and erroneously accepted as the dominant "scientific" orientation of this field) conflicts with his empiricist position on testing and fails to articulate criteria of usefulness of this "theory" for the practice of states.

It is here that the authors see much value in the "realism" of Carr and realists of the older school; for the latter theories were less like Popper's set of true statements in the "Third World" but rather more like critical discourses on political and social practices, which showed their usefulness in discursive debates (1) in illuminating, rather than hiding, the partial nature of the initial assumptions; (2) in making possible debates, rather than pseudoscientific demonstrations; and (3) in ex-

panding a heuristically fruitful research agenda, rather than sacrificing the most interesting questions about persistence and change of international systems on the altar of a mistaken ideal of parsimony.

At this point, the persuasive strategy pursued by the authors becomes clear. Although they start with the established neorealist model, in their subsequent substantive respecifications of the theoretical terms and their inclusion of entirely new elements in the concept of "structure," they open up new avenues of investigation that have been prematurely closed by neorealist theory. This strategy of persuasion engages the reader at different levels. It leads one from the accepted but problematic model of neorealism, via a theoretical reformulation, to some historical case studies. The latter demonstrate the actual existence of anarchies with functional differentiation and the generative force of the internal structure of power, as well as the unavailability of a balance of power in certain anarchical formations. Taking the agent-structure debate as a point of departure, Little suggests that the problem of system reproduction cannot be reduced to ecologically determined homogenization, as Waltz suggests. Instead, we have to take the importance of constraints imposed by state structures into account.

How useful such an approach can be is demonstrated by the discussion of the demise of Greek city-state system (quite at odds with Gilpin's hegemonic war thesis) and of the Macedonian and Carthagenean Empires. Little also explodes the myth that Rome's decline was the result of the barbaric invasions. Instead, he traces its demise to the increasingly divergent internal economic and social structures within the Constantinian Empire. Such case studies of conceptually informed historical research not only suggest the fruitfulness of the reformulated structural paradigm but indicate that attempts of building theories of international relations—or even of international politics-on the basis of "assumptions" dispensing with historically informed hypotheses often pay the heavy price of irrelevance (due to their impoverished ontology) and ideological distortion.

These case studies, in turn, justify both the analytical choice of a system concept that is not limited to the "political" aspect alone (precisely because the changing nature of politics and economics represents one of the most interesting debates about the international political economy) and the important historical dimension of the international system, which, the authors claim, has been "subsystem-dominant" until recently. Finally, the reader is confronted with the epistemological implications of this analytical historical exploration.

It is, of course, impossible to lay out here the analytical and historical arguments in any detail. But it might suffice to say that reader will be richly rewarded by carefully pondering the arguments, which blend historical materials and analytical discussion very nicely. While some of the insights, such as the existence of anarchies with functional differentiation, have not been new since Ruggie's critique and its subsequent debate, Buzan and (in part 2) Little not only show the rather persuasive existence of historical counterfactuals but systematically examine how the functional differentiation of the units affects the character of the deep structure of anarchy. Furthermore, by including in "structure" factors not restricted to the capabilities that Waltz aggregates in a highly problematic concept of power, the English scholars identify important aspects of the dynamics of international systems, as well as sources of change that have eluded traditional neorealist analysis.

In this context the disaggregation of capabilities and the introduction of the systemic factors of technology and of shared norms and organization proves useful, since it systematically elaborates Ruggie's notion of "dynamic density." These two conceptual changes have important implications for the new structural realist research program. The first leads to the conceptualization of "sectors" of the international system that are no longer governed by the logic of politics and its organizing principle of sovereignty. For example, the impact of the economy on politics contradicts the logic of inclusion and subsumption of these separate systems or sectors under the "political system." Instead of this subsumption, prevalent among neorealists, the authors opt for the metaphor of different "lenses" in order to convey the distinctiveness, as well as the "totality," of the different sectors. The British scholars point out that only the wider conception of international relations (rather than international politics) is capable of capturing the interaction effects between different sectors as the conflicting logic of "totalities" inform us about important tensions within the "international system."

The second addition—the inclusion of systemic capabilities of organization and technology, captured by the term interaction capacity (of systems)—allows for important distinctions among historical systems and for the nuanced assessment of these factors for the system's reproduction. Since these capabilities are systemwide but are located neither at the unit-level nor at the "deep" level, they escape the attention of neorealists even though the operational logic of the Waltzian system defined as coaction of units depends on the types of interaction capabilities. Consequently, the interaction will be very different in two systems that share the same deep and distributional structure but differ markedly on the level of communication and transportation technology and/or the level of institutionalization. It seems, then, entirely implausible to argue that such an increase in "dynamic density" will make the units "otherwise similar" (as Waltz maintains).

After this brief outline of some of the most important modifications of the traditional version of structural realism, one might, indeed, wonder whether, with all the (e)mending that has taken place, there is still anything left of the old sock of neorealism or whether these improvements, piecemeal as they seem, do not, indeed, present an entirely new research program. Whatever the answer to these questions might be (and the emphasis on a rhetoric of persuasion in international relations theory building provides at least one powerful reason why the emendation strategy, rather than a more radical one of replacement, was chosen), there is no doubt that the authors have presented a strong prima facie case in favor of the new program.

On a more critical note, there is some unevenness, weak arguments mingling with strong. One also wonders a bit about the omissions, the most obvious being the failure to utilize—or even mention—the highly sophisticated theory of "world society" developed by Niklas Luhmann. It could have given the emphasis on international relations, rather than international politics, substantially more theoretical backing. But none of these flaws would make me change my assessment that this is a sophisticated theoretical book that will be read with profit by international relations scholars, social theorists,

and historians alike. It is a must for anyone who wants to be informed about—and transcend—the current debates in the discipline.

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FRIEDRICH KRATOCHWIL

When the Third World Matters: Latin America and the United States Grand Strategy. By Michael C. Desch. Baltimore: Johns Hopkins University Press, 1993. 218p. \$38.50.

This book should more appropriately be titled "When Latin America Matters," for that is its essential concern. Michael Desch examines four episodes in the U.S. relationship with Latin America in order to demonstrate the connection between that region and the pursuit of more vital security interests elsewhere. And, on the basis of this analysis, he posits a broader theory about the relationship between the third world and the "grand strategies" of the great powers.

Whether one can infer such a general theory from a so narrowly-focused set of cases is just one of the troubling aspects of this book. Perhaps a more fundamental reservation stems from the author's prevailing assumption that security policy is rational and coherent and based on self-evident understandings of the balance of power.

Desch argues that in its pursuit of a grand strategy, a great power acts to protect certain "intrinsic values," i.e., those vital to the nation's security. "Peripheral" areas, such as the third world, may, from time to time, take on "extrinsic value" when they "enhance or constrain the great powers' ability to defend themselves and maintain the balance of power" (p. x). He appears to accept without question that these values can be—and have been—determined objectively, unhindered by domestic and bureaucratic politics or by any number of other non-rational variables that impinge on the policy process.

The first case concerns U.S. preoccupation with Mexico just prior to U.S. entry into World War I. Here, Desch makes extensive use of original documents and develops a somewhat unconventional argument. He contends that before Washington could intervene in Europe (an intrinsic value) it was necessary first to take care of the German threat to U.S. security emanating from Mexico (an extrinsic value). According to Desch, the German attempt to divert U.S. attention from Europe to Mexico was successful and converted Mexico from a peripheral interest to one linked to the balance of power in Europe. While this is a convincing argument, its contribution to the overall theory is weakened by the assumption that a country bordering the United States would ever be considered "peripheral." There is also likely to be disagreement over his portrayal of the seriousness of the German intrusion into Mexico.

The U.S. responses to perceived German threats to Latin America during World War II form the basis for a second case study. Although Desch asserts that Washington "integrated" Latin America into its grand strategy for Europe, the evidence falls short of demonstrating how this linkage actually influenced important aspects of U.S. conduct of the war.

In his consideration of the role of Cuba in the missile crisis, Desch provides persuasive support for a) the contention that the Soviets acted primarily to alter the strategic nuclear balance, and b) the suggestion that their backing away from a confrontation was due to

superior U.S. conventional forces in the Caribbean. As interesting as this discussion is, however, the case only indirectly supports his general hypothesis about the role of the third world. Again, as in the case of Mexico, Cuba has traditionally held a special place in U.S. security concerns and it was not merely Soviet missiles that raised its status from peripheral to extrinsic value.

Curiously, throughout this entire study there is no reference to the concept of spheres of influence or the literature on that subject. This is especially puzzling inasmuch as Desch draws considerably upon geopolitical concepts to rationalize the distinction between intrin-

sic, extrinsic, and peripheral values.

The viability of Desch's thesis is stretched further by his contention that what mattered about Latin America during the Cold War was the protection of "sea lines of communication" (SLOCs). Although it is unclear if he is arguing that this was the case or if he is suggesting it ought to have been, he seems to assume that protecting SLOCs for their contribution to a potential war in Europe was the fundamental strategic interest guiding U.S. policy. He does not address how this shaped U.S. intervention in Nicaragua during the 1980s, for example, but he later recommends against third world interventions aimed at changing local governments. In addition to exaggerating the actual threat to SLOCs during the Cold War, Desch neglects the extensive body of literature relating to a century of U.S. preoccupation with a different set of SLOCs affecting access to the isthmus of Panama.

Desch presents a reasonable case for disregarding the "credibility" argument as a justification for U.S. intervention in the third world. Acting in the third world to send a message to enemies and allies about U.S. reliability, he states, is ineffective and costly. The difficulty is determining whether the author thinks this has not actually been done, despite the rhetoric, or whether he is simply recommending against it because it distorts the search for rational strategic interdependence.

To claim broad theoretical implications from these limited case studies is to ask too much, especially when all of the cases come from one region, Latin America, and when part of that area has had a traditional role as a U.S. sphere of influence—independent of its third world status. Desch's proposition would have been strengthened by consideration of other obvious third world cases such as the Vietnam war. Too often, the author overstates his conclusions; he claims more than the evidence supports and too often allows the compelling logic of grand strategy to take the place of a more modest and useful analysis. The documentary evidence, especially for World War I, provides the basis for a study of Latin America's link to broader U.S. interests, but care should be taken not to extrapolate too far.

Ohio University

HAROLD MOLINEU

Doing Good or Doing Well? Japan's Foreign Aid Program. By Margee Ensign. New York: Columbia University Press, 1992. 198p. \$24.50.

Japan's Foreign Aid: Power and Policy in a New Era. Edited by Bruce M. Koppel and Robert M. Orr, Jr. Boulder: Westview, 1993. 378p. \$49.50 paper.

Japanese foreign aid programs merit examination for a host of reasons. Some involve treating them as a window to look into motives and mechanisms of Japan's foreign policy and international role. From this perspective, changes, continuities, and variation in Tokyo's foreign aid can illuminate broader questions of how Tokyo relates to the world and seems likely to relate in the future. How strong are the signs of a strategic pattern at work, and to what extent does it involve more than the pursuit of mercantilist advantage? Foreign aid practices can also throw light on Japan's policy capacity to take timely, proactive, and coherent decisions and adapt to external changes. Is the conventional wisdom that Japan lacks those abilities warranted? Other reasons involve interests in development per se-the impact on recipients, as distinct from the motives and mechanisms of donors. Here, examination of Japan's record can inform us about the consequences of different forms of aid and clarify the development consequences of Japanese behavior.

Finally, analysis of Japan's programs can enrich our understanding of foreign aid as a generic policy instrument—one whose importance is only likely to increase in the post—Cold War world marked by transformations away from autarkic command economies. Foreign aid has been a more important policy instrument for Japan than for the United States, compared with other means to exert influence and manage foreign relations. Accordingly, the Japanese case offers fertile empirical ground for exploring the variety of purposes that the aid instrument can serve.

Margee Ensign's Doing Good or Doing Well? and Koppel and Orr's edited collection, Japan's Foreign Aid have three things in common: (1) emphasis on detailed information about the particulars of what Japan has done; (2) concentration on the Official Development Assistance (ODA) element of Japanese behavior, as opposed to private flows to recipients; and (3) recognition of the important role of the Japanese private sector in ODA behavior. They differ in other major respects.

Koppel and Orr seek to provide an improved framework for understanding Tokyo's ODA in the context of changing Japanese bilateral and regional foreign policies (including security policies), the influence of those changes on ODA policy and management, and the nature and cause of variation across recipients. They do so by providing a rich set of case studies: ASEAN, Indonesia, the Phillipines, Thailand, Vietnam, Burma, China, Bangladesh, Korea, the Pacific Island States, sub-Saharan Africa, Latin America, the Middle East, and the Asian Development Bank. The cases thus cover the bulk of the Japanese ODA of a bilateral character and do so over the full history of the ODA to the receiving parties.

As a set, the case studies lead to a clear and challenging set of conclusions that differ from much of the conventional wisdom about Japanese foreign aid. The Japanese ODA emerges as a flexible instrument adjusted according to Japan's and recipients' situations and U.S. priorities, to serve purposes that go far beyond mercantilist penetration and dependency. This harnessing of bilateral and regional ODA programs to multiple foreign policy purposes and recipient absorptive capacities is not a new development. Nor is the use of the ODA as a mechanism either to manage relationships with other donor nations or to compensate for friction with donors on economic and political issues. The domestic determinants, in Japan, of ODA actions are shown to be

complex (involving different parts of the bureaucracy, politicians, and the private sector) but not disabling.

What emerges is a realistic portrait, in which the ODA serves both foreign policy interests and domestic interests to a substantial extent. The world in which the Japanese ODA exists is a complex one warranting variation in policy practices. Japanese behavior appropriately manifests substantial variation. In part, that is because of a notable freedom from any rigid general ideology about the road to development beyond the view that economic growth is conducive to political stability and democratization. Japan does not lack a development philosophy; but it does recognize situational differences and the role of the ODA in advancing purposes other than the economic growth of the recipient. We are left with a view of the Japanese ODA as less than perfect from the perspective of recipients but open to influence by them in ways that reflect long-run Japanese interests and choices. No systematic assessment is offered of the ODA's economic impacts; but it is clear that many of the major recipients have achieved notable growth performance and increased democratization and have found no available and preferable alternative to Japanese grants and loans.

Ensign focuses on the implementation of the Japanese ODA with respect to the foreign role (especially of U.S. firms) in project selection and receipt of procurement funds from Japan's ODA coffers. Her book is more of an indictment than a systematic analysis. She concludes that in general, the Japanese ODA is selfish in its beneficiaries and adverse in its consequences for recipients. This negative verdict is a consequence of Japanese emphases on capital infrastructure projects, loans rather than grants, and informally tied procurement.

The empirical basis for these conclusions consists of seriously limited information—fragmentary in its coverage both temporally and across recipients. Her information surely does not support arguments that Japan eschews mercantilist benefits. At the same time, it does not suffice to demonstrate that mercantilist objectives dominate the Japanese ODA. That would require attention to a broader range of cases than the six Asian countries and the grant element of aid to Africa and a more extended time period. It would require a more extensive list of variables that might explain Japanese export shifts following ODA commitments. It would have to explore the multiple interests that Japan has in recipients, be they Asian or African.

Ensign clearly has a view of development in which the ODA is best used for purposes other than capital infrastructure projects, a conclusion she rests primarily on a rather old set of evaluative judgments of U.S. aid projects. Aside from questions about the quality of these judgments, it is not obvious that Japan chooses or manages its capital infrastructure projects the way the United States did. Nor is it self-evident from the Asian cases examined that the capital project efforts have been predominantly bad for the recipients. After all, her Asian cases are, for the most part, often cited as examples of development success. The one that is not (the Phillipines) involves the greatest Japanese accomodation to U.S. policy concerns. In any event, the evolution of the Japanese ODA shows substantial and increasing giving for purposes other than capital infrastructure projects.

Finally, Ensign's views on how aid monies should be spent in terms of procurement seem to resemble some

American arguments about U.S. entitlements to some substantial share of the domestic Japanese market for goods. The general argument that untying leads to lower procurement prices in itself does not support the inference that procurement from U.S. firms will lead to lower prices. Nor does she provide substantial evidence that Japan is exceptional in the role played by her nationals as project consultants and contractors.

University of Pittsburgh

DAVIS B. BOBROW

The Paradox of Continental Production: National Investment Policies in North America. By Barbara Jenkins. Ithaca: Cornell University Press, 1993. 234p. \$28.50.

As the political debate over the North American Free Trade Agreement (NAFTA) heats up, some insight into the agreement's political foundations provides an important perspective and helps one to evaluate the NAFTA's future prospects. Barbara Jenkins's *The Paradox of Continental Production* provides such a perspective and evaluation. She documents the evolution of national politics, policies, and interests that made the agreement possible. She also explains several political paradoxes that have emerged in the wake of the growing integration of the continental North American economies. The central paradox emphasizes the continued importance of national politics and government policies just when one would expect the increasing scope of markets to make state actors and political preferences of domestic constituencies obsolete.

Why do the strategic capabilities and political preferences of individual nation-states continue to play a key role when market integration appears to have removed more and more policy levers from the hands of national policymakers? Jenkins argues that states need to attend to international economic policy and its linkages with domestic industrial policy because markets, if left unregulated, will cause severe social dislocation resulting in nationalistic "Polanyian" backlashes, thereby threatening moves toward further market integration. In addition, major market actors, if left to themselves, may underinvest in human capital and research-and-development in specific countries if these capabilities exist elsewhere in their dispersed multinational networks.

Jenkins reviews the literature on firm-state relations and finds that it lacks adequate political analysis of the determinants of states' bargaining power in negotiations with multinational investors. She then turns to a discussion of the politics of inward foreign direct investment (IFDI) review in the United States, Canada, and Mexico. A major goal of the three case studies is to document the preferences of political actors who have played an important role in IFDI review liberalization in Mexico and Canada and the increase in the stringency of IFDI review in the United States. This careful documentation of the views of both policymakers and interest groups in each of the three countries provides a good political background for scholars new to many of the current political debates going on over the costs and benefits of the proposed NAFTA.

Missing from this political analysis, however, is some reference to the strategic interaction between states and firms, which together comprise states' international economic capabilities. In an increasingly integrated regional

and global economy, neither politicians nor scholars can afford to ignore the interrelatedness between state and firm strategies and the implications of this interaction for the interests of domestic political actors. For regional trade agreements to succeed, both state and firm strategies need to contribute to the competitiveness of national economies.

Jenkins begins by demonstrating empirically that regulatory policies that ensure that multinational corporation investments enhance the national economic objectives of their hosts do not discourage the inflow of investment. She concludes from this that there are few costs involved in requiring multinational corporations to meet specific domestic policy objectives. One implication of this argument, in the Canadian case, is that the failure to regulate IFDI results more from a failure in political will than from the economic consequences of such regulation (p. 153). The relationship between the impact of these regulations upon multinational corporations' assessment of the relative attractiveness of the Canadian market and Canada's lack of political will, however, goes unexplored.

For example, Jenkins suggests that because multinational corporations have historically underinvested in research-and-development as a percentage of sales in Canada, Canadian policy, to increase the amount, should "mold incoming FDI to fit national industrial priorities" (p. 151). This could involve "the targeting of both domestic and foreign firms possessing critical product or process technologies and matching them with Canadian business partners" (p. 212). However, studies of cooperative research-and-development projects, including those as visible as Sematech, have found that most firms will cooperate with one another in primary research but typically become less willing to continue ongoing cooperative efforts as a product gets closer to the development process. A Canadian policy to mandate cooperative research-and-development risks alienating potential investors who do not want to risk losing control over proprietary product and process technolo-

How, then, can Canada prevent multinational corporation investors' research-and-development budget allocations from going primarily to the United States when without regulation (as Jenkins points out), few inducements exist for these investors to consider Canada? And if this trend continues, what will prevent a backlash against the U.S.-Canadian Free Trade Agreement (FTA) as Canadian industry loses its technology base and political support for the agreement steadily erodes. Jenkins would suggest that few alternatives exist to state-mandated cooperative agreements that would partner multinational corporations with Canadian firms and, hopefully, increase research-and-development expenditure in Canada. As continental markets continue to integrate, she argues that this kind of regulation of IFDI remains critical to sustaining the political support for the FTA and eventually for the NAFTA.

Some discussion of the kinds of factors that attract multinational investors would be useful here. Only mentioned in passing is a brief reference to Reich's suggestion that government support for the development of human capital may influence the research-and-development location decisions of multinational investors (p. 214). The presence of specific factor endowments still go a long way toward attracting multinational corporation investment, regardless of what extra financial

incentives states choose to offer. State subsidies to home firms may reduce their attractiveness as suppliers to, or partners with, multinational corporations, since few guarantees exist to ensure the continuation of this financial support as state policies and priorities change over time. In any case, to figure out the likely success of a specific state policy, some consideration needs to be given to the likely strategic response of foreign investors.

Embedded in her discussion of the politics of American IFDI review policy, Jenkins includes prescriptions regarding American research-and-development policy that take a slightly different tack. As in the Canadian case, she suggests that the United States needs to increase its institutional capabilities to implement technology policy in order to avoid a nationalist backlash. Her argument begins with the intentional ambiguity of the meaning of national security as written into the Exon-Florio amendment to the Omnibus Trade and Competitiveness Act of 1988. This amendment states that the president has the authority to block a foreign takeover of a U.S. high-technology company by a foreign investor if it can be demonstrated that this would constitute a threat to U.S. "national security." Given the perceptions of unfairness on the part of U.S. business regarding foreign competitors' research-and-development subsidies, Jenkins fears that this ambiguity could be turned against foreign companies seeking to acquire U.S. firms as a part of a nationalistic backlash. In order to stave off this possibility, she argues that the United States needs a coherent commercial research-and-development policy run by the Commerce Department that would involve "a centralized agency with its own policy agenda, technical expertise, and ideological legitimacy (not to mention extensive budget)," in order to put U.S. high-technology firms on an equal footing with their competitors and diffuse potential protectionist political pressures (pp. 98-99)

While this kind of institutional framework for technology policy may have been appropriate when a relatively slow rate of technological innovation characterized industrial change, with the increasingly rapid rate of technological change, questions arise regarding the appropriateness of this kind of centralized technology policy. Specifically, the United States government has demonstrated little of the institutional flexibility needed to anticipate the direction of technological innovation. Even the authors of the strategic trade models have argued about the difficulties in picking technologies to subsidize and firms with the managerial capabilities to earn excess returns. If these subsidies slow the innovation process within firms, a coherent U.S. commercial technology policy could detract from, rather than contribute to, U.S. competitiveness. Again, the reciprocal effects of state policy and managerial decision making need to be taken into account to understand the relationship among politics, policy, and national competitiveness.

In the Mexican case, Jenkins argues that the austerity policies and lack of economic growth resulting from Mexico's failed import substitution policies prompted the "technicos" trained in elite U.S. universities to open the Mexican economy to foreign trade and investment. Like Canada, once Mexico began to rely more on exportled growth, the possibility that U.S. administrative trade law could be used to block Mexico's access to the U.S. market and the increased reliance of Mexican exporters on the U.S. market provided sufficient incentive for the

Mexican political leadership to pursue the NAFTA. This leadership found political support among the moderate ranks of the Partido Revolucionario Institucional (PRI), who believed that they would benefit from guaranteed access to the U.S. market. Jenkins suggests that the government's policy to devalue the peso has played a critical role in the competitive success of Mexican entrepreneurs in U.S. markets. She goes on to contend that an unexpected appreciation of the peso would reduce the competitiveness of small- and medium-size Mexican firms in the United States, which, in turn, could alienate this critical constituency for the NAFTA and threaten the future success of the agreement: "The revaluation of the currency or the appearance of vigorous competition to Mexican business in the domestic market would affect capital's attitude toward liberalization and would destabilize the precarious political coalition that is behind the drive toward continentalization" (p. 157).

Recent experience with the European Monetary System indicates that governments acting alone have less and less ability to influence the value of their currency. Underlying economic conditions strongly influence currency traders' expectations of whether a country's currency will appreciate or depreciate. The peso was devalued from 447 to the U.S. dollar in 1985 to 2,379 to the dollar by the end of 1987 because of inflation, the decline in oil prices, and the need to export to earn foreign exchange to allow Mexico to pay off foreign debt (p. 179). The devaluation of the peso may have encouraged Mexican entrepreneurs to seek foreign, especially North American, markets for their goods—but not only because the "technicos" figured out that this would create political support for the increased integration of the North American economies. Given the pressures on the peso, it is difficult to see what the government could have done to prevent the peso's devaluation beside strict rationing of foreign exchange, which it tried with little success. If the peso does appreciate in the wake of the NAFTA, unless it results from an extremely tight antiinflationary monetary policy that may threaten Mexico's economic growth, this appreciation could reflect the increasing productivity in the Mexican economy and its ability to consume more foreign imports. To respond competitively as the peso appreciates, Mexican entrepreneurs will have to adopt increasingly differentiated strategies and more capital-intensive production processes to ensure the continued appeal of their products in foreign markets. Only if Mexican entrepreneurs stick to low-cost strategies will an appreciation of the peso put them at a competitive disadvantage.

The politics of trade and investment liberalization remain important, and (as Jenkins argues) politicians need to pay attention to adjustment costs to avoid a political backlash to increased regional and global economic integration. Historically, to demonstrate their autonomy, states have been willing to incur significant economic losses, as Canada did with its National Energy Policy when it paid top dollar for foreign-owned energy assets. Yet this kind of expenditure has become increasingly more difficult to justify politically as government budgets strain to cover health care obligations and their other social welfare responsibilities. Increasingly, an understanding of the politics of trade and investment policy will need to take into account the opportunity costs of specific policies. These opportunity costs result from the strategic interaction between governments and firms. In addition, consideration needs to be given to the

dynamics of the economic environment in which trading and investing takes place.

Jenkins provides an insightful analysis into the various national politics of trade and investment policy. The paradox that emerges from the analysis, however, is that as the domestic political foundation of continental economic integration increases in importance, political analysis alone may decrease in its utility. Future research may benefit from incorporating theory from strategic management and international economics in understanding the domestic politics of international economic policy.

University of Minnesota

STEFANIE ANN LENWAY

Security without Nuclear Weapons? Different Perspectives on Non-nuclear Security. Edited by Regina Cowen Karp. New York: Oxford University Press, 1992. 296p. \$55.00.

This book, filled with creativity, imagination, and wisdom, moves well beyond most existing scholarship on national security policy. The authors encourage us to think about what has become almost unthinkable among security policy experts—security without nuclear weapons. The contributors directly address many questions that are seldom discussed but profoundly important: How far can nuclear arsenals be reduced before security risks and instabilities will significantly increase? Can people be secure in a world without nuclear weapons? How do the security risks of existing policies, including the accompanying prospects for the proliferation of nuclear weapons, compare to the risks of alternative policies leading toward the abolition of nuclear arms?

Fourteen contributors systematically explore the nature of nonnuclear security and examine the opportunities for, and constraints upon, those seeking to abolish nuclear weapons. Because most of the book was written before the depth of change in the former Soviet Union had been universally acknowledged, a few chapters seem dated, such as "The End of Superpower Arms Control?" by Lynn Eden. Nevertheless, most remain profoundly relevant, because the authors focus on nonnuclear security, not nuclear disarmament. They examine the problems and benefits of deftly moving nuclear weapons into the background of security maintenance.

Regina Cowen Karp leads off with a compelling discussion of why it is intellectually responsible—even essential—to study how the denuclearization of security policies might enhance security. She persuasively sets the tone for addressing the problems of nuclear proliferation in a crisis-prone environment by calling for a strategic debate focused on international security problems, rather than primarily on nuclear weapons.

Ken Booth and Nicholas Wheeler point out in a highly original essay that such a debate may not be easy to conduct, because the security community's thinking has become far more rigid than most recognize. Most security experts' dismissive attitude toward nuclear abolition reflects their persistent hegemony within academe and policymaking circles, rather than the rigor of their scholarship or the open-mindedness of their reflections. The authors ask for no utopias, only for willingness not to rule imaginative ideas off the agenda before reflection. They then demonstrate that rethinking the nature and purpose of international political processes, as opposed to

focusing primarily on military deployments and doctrines, could enable societies to implement policies that would genuinely enhance security while allowing reliance on nuclear weapons to decline.

Erwin Häckel, who explores the economic arguments for moving toward nuclear abolition, acknowledges that the nuclear powers and most of the countries near the threshold of nuclear weapons do not consider economic costs to be a decisive argument against nuclear arms. Nonetheless, the utility of nuclear weapons for military planning and combat has declined; and nuclear powers feel the need to maintain sizable conventional forces in addition to their nuclear arms, in order to avoid nuclear combat if war should occur. As a result, Häckel sees practical economic benefits arising from measures that would decrease the security relevance of nuclear weapons.

Julie Dahlitz demonstrates that in addition to economic incentives, international law can also play an influential role in buttressing governments' political will for decreasing reliance on nuclear arms, because governments can give law increased authority by the manner in which they make it and abide by it. In contrast to the particularly hostile environment of the Cold War, Dahlitz argues that a deliberate effort could now be made to create laws that institutionalize cooperative security policies. A wisely engineered evolution of world law, constituting one part of a more general transformation of the world system, could enable nonnuclear security to become a prudential choice.

Paul White, Robert Pendley, and Patrick Garrity explain in their article that depending on governments' political decisions, technological innovations can either encourage or impede steps toward abolishing nuclear arms. In addition, they caution that if governments downplay nuclear arms, strategies for conventional weapons must also be turned away from offensive deployments for security to be genuinely enhanced. The elimination of all wars, not just nuclear war, must be addressed. I wish they had discussed the employment of new nonlethal technologies by international enforcement agencies as one additional means for easing both nuclear and conventional weapons into a less prominent role.

Patricia Lewis explains that enormous potential for nonnuclear security arises also from the recent progress that Moscow and Washington have made in monitoring weapons tests, deployments, and even manufacturing. It is far easier, she demonstrates, to verify the complete absence of a weapons system than to inspect it reliably when maintained at a particular level above zero. Moreover, if states ever intend to move toward nuclear abolition at some point in the future, then the sooner comprehensive international monitoring can begin, the better. Even during the current period of large arsenals, the more detailed the available knowledge of every country's evolving nuclear capacities for military and nonmilitary purposes, the fewer the chances that nuclear activities could be disguised or hidden later on.

Frank von Hippel confirms that uncertainty about the amount of fissile material that has already been produced constitutes a serious problem for verifiable nuclear abolition. Although it may never be possible to prove the existence of a totally nuclear-weapon-free world, absolute proof may be unnecessary as cooperative security arrangements replace nuclear weapons and make any hidden fissile material less consequential than

the nonnuclear security mechanisms already put in place. In examining nuclear proliferation, George Quester correctly notes that it has not been as large a problem as many feared it would be.

Harald Müller creatively explores how the elimination of nuclear weapons would affect the traditional function of alliances and hegemonies. He concludes that nonnuclear security can be maintained only in a low-threat environment. In order to achieve such an environment, to move toward a nonnuclear world, and to avoid the major security problem of reversibility once such a world is being achieved, he recommends a series of interlocking security regimes to address adversaries' grievances, reduce misperceptions, and avoid threatening security postures. Moreover, because nonnuclear enclaves cannot survive long in a world of harsh conflicts, nonnuclearism "must encompass the globe, or it will have no future at all" (p. 249).

Nicholas Wheeler explains that the widespread, common objections to a minimum deterrence policy ought not deter movement toward abolition, because (1) increased military transparency and confidence-building measures can provide reassurances that no militarily significant cheating can occur, (2) technological innovation for unilateral military advantage can be constrained by replacing the goal of strategic superiority with policies for common security, and (3) the alleged problem of targeting inflexibility associated with small nuclear forces will not cause deterrence to lose its effectiveness, because of ambiguity about whether cities or other assets have been targeted.

My only reservation about the book springs from feeling that in a few instances, the authors have failed to follow their own bold analytic paths to their logical conclusions. Is it sufficient to explore the benefits of nonnuclear security, or should the exploration also probe more deeply to explain the impediments to reaping those benefits? The analyses could be strengthened by commenting more fully on resistance to building a new code of international conduct even when rational thought demonstrates its prudence. Moreover, the book may give insufficient attention to the need for, and benefits of, structural change in the world system.

Nonetheless, this book is a profoundly valuable addition to the national security debate. Its most original work demonstrates that the risks and costs of a well-developed plan to move the world toward abolition of nuclear arms are significantly lower than the risks and costs of today's world, in which the major powers maintain their existing nuclear arsenals and a double standard on who may possess nuclear arms. Future scholarship, one hopes, will explain why this conclusion is not widely recognized and what must occur to enable more careful reasoning to shape security policies.

University of Notre Dame

ROBERT C. JOHANSEN

Foreign Military Intervention: The Dynamics of Protracted Conflict. Edited by Ariel E. Levite, Bruce W. Jentleson, and Larry Berman. New York: Columbia University Press, 1992. 334p. \$32.50.

This book represents the collective effort of more than a dozen scholars to understand and describe aspects of modern military interventions. Drawing principally on six case studies developed around a standard analytic framework, the goal is to extract general characteristics of long-term intervention experiences that explain how countries become entangled and disentangled from complex political-military commitments.

The research design features a structured comparison of protracted interventions that took place in diverse settings involving different players, including the United States in Viet Nam; the Soviet Union in Afghanistan; Syria and Israel in Lebanon; India in Sri Lanka; and the superpowers, Cuba, and South Africa in Angola. Each instance is examined in a separate chapter that chronicles the intervenor's policy choices through initial involvement, progression of intensified activity, and withdrawal phases. The case studies introduce key events within the target country and in regional affairs and note various success and failure points. Together, these chapters (roughly two-thirds of the text) offer little new information. The material is important, however; for it constitutes raw historical data for interpretative abstract summaries and conclusions that follow.

The final section of the book provides rich generalities derived from common patterns detected across the cases. The editors organize the analysis by segmented time periods of intervention experience: there is a chapter each on "getting in," "staying in," and "getting out." What fresh insights are offered about intervention? The reader is given a combination of standard themes and ideas and lessons taken from the research strategy, yielding several interesting conclusions.

Why do states intervene? Civil conflicts around the world create ample opportunities for outside state involvement. In foreign military interventions, the objective is to shape the political authority structure through coercion and a local ally who is to be assisted in gaining or maintaining power. Such goals are hard to achieve using conventional military assessment criteria, because they are not easily translated into practical and concrete terms and may incorporate combinations of cross-cutting motives, misread conditions, and ill-understood consequences.

What consequences follow when military troops are directly involved? A military presence poses riskier potential costs, sharpens the meaning of commitment to goal achievement, and affects a greater investment of human and material resources. Yet in the cases examined, military involvement brought to the situation not clarity but ambiguity and murkiness, instead.

Why do military interventions become protracted? Although the editors admit the question cannot be conclusively answered based on this study, some partial responses are presented. Decisions to intervene are arrived at gradually and reluctantly, consisting of small steps and complex interaction of forces. The analysis does not show a slippery-slope idea capturing the progression of involvement. Elites were conscious of the potential magnitude of operations they authorized and were aware that the scope of their commitment could widen; yet they believed that the conflict would be short, with military forces bringing political victory. But policymakers made errors of judgment: they tended to believe that a balance of resolve would favor the intervening state; that local allies would be reliable, strong, and trusting partners; and that intervention would create only intended effects on the political scene. Human failings and miscalculations in intelligence and cost estimates are not overplayed, however. As Downs cautions in his chapter on disengagement, the ease of overestimating human error in ex post facto analysis can be dangerous.

Why do states withdraw their troops? The answer is based on decision makers' perceptions of changes in global politics, enemy composition, resource base, and home public support. In the end, new administrations had to come to power to replace the prevailing intervention strategy and get out. The message is that old leaders continue policy patterns, and only new ones may change the course.

This study rejects the notion that policymakers are universally unwise, motivated by grandeur or ideology, or that they have distinct human limitations. In fact, leaders are judged neither harshly nor favorably. Protractedness is not the result of foolishness of choice but murkiness of the situation. Cohen, in summarizing the progressive stage of interventions, argues that the middle game is hard to evaluate for success or effectiveness. Assessments are muddy, and a general fog prevails, limiting clear understanding.

In the end, conclude the editors, foreign military interventions are unpredictable, complex, destructive, and difficult to evaluate on a cost-benefit scale. Why? Because the cases chosen emphasize these factors, and the framework cannot screen out noise. On a broader note, intervention may continue to be characterized in this way because, as part of international relations conflict, political goals will continue to be advanced by military means, opportunities and incentives for intervention will exist, and international controls marking parameters of acceptable intervention will not be delineated.

An appraisal of this book must rest on the wisdom of case selections and on the theoretical framework used to extract information and build general patterns about protracted military intervention. The emergence of a new era in international relations characterized by intensified, violent nationalist movements, poses problems relating to the balance between self-determination and sovereignty and assessments of security and stability—in short, serious challenges for a potential intervenor. Are there guidelines for policymakers in this book? Do scholars have a better understanding of the process of intervention?

Many questions are still unanswered, yet the book is a catalyst for anyone who wishes to probe the matter further. In these cases, for example, if protracted interventions are costly and less than successful, we might want to know more about the effects of policy planning. Were the intervention strategies essentially random or clearly planned? Were they outlined in both short- and long-term frameworks? Did the introduction of military troops to solve the conflict reconfirm or invalidate policymaker beliefs about force effectiveness? One acquires no real perspective on these points, in spite of the historical detail and conscious comparative analysis presented. The study lacks criteria to measure intervention policy success and failure. What does each entail? What degrees of difference exist? What happens once intervening states begin to realize that they may not achieve their objectives? This choice is essentially one of flight or fight; but since military intervention in this project is conceived as a process composed of small steps, the overarching strategy is hard to unveil. Perhaps including some short, successful interventions as case studies might have sharpened the causal logic developed to identify involvement and disengagement decisions. Alternative design structures are often recommended over a chosen research strategy, but the issue here is whether substantial alteration in conclusions might have been the result. This is hard to say.

The status of intervention as a key concept to understand contemporary world politics is rising. As a tool of influence, it represents overt and covert involvement by bigger states into the economic, social, and political processes of weaker countries. It is a bulwark of power politics. With the end of the Cold War and new domains of conflict eruption, sovereignty and security demands are unlikely to disappear from the international scene. Today, humanitarian and collective intervention are the frequently debated issues in official international circles; but military force is still a significant part of the action. In essence, this book is timely for its emphasis on military force and contributes to our knowledge about dangers behind protracted conflict involvement. This should be useful in the current world environment of multiple conflicts and attendant, ample temptations for intervention.

University of Denver

KAREN A. FESTE

Morality and American Foreign Policy. By Robert W. McElroy. Princeton: Princeton University Press, 1992. 194p. \$24.95.

Traditions and Values in Politics and Diplomacy: Theory and Practice. By Kenneth W. Thompson. Baton Rouge: Louisiana State University Press, 1992. 353p. \$37.50 cloth, \$12.95 paper.

Though the contrasts between Robert W. McElroy's Morality and American Foreign Policy and Kenneth W. Thompson's Traditions and Values in Politics and Diplomacy are both real and apparent, that the contrasts between these two readable and knowledgeable books are not far greater than they are illustrates much of the difficulty with the state of theory in the field of international relations.

McElroy's work is a dissertational effort to clarify the digms in international relations as well as build a "pathway" between them. It is a least 11. way" between them. It is a laudable first book. Decrying the undoubtedly still great distance between ethics (or philosophy generally) and the study of international affairs, McElroy suggests properly if not originally that the consideration of morality is inexorably bound to the consideration if not the reality of international progress. There is more than a hint of purposiveness here, although unfortunately the philosophical clarion to such purposiveness is Immanuel Kant whose notions of the universalization of international norms and a kind of golden rule of international behavior reflect all too vividly the usual Kantian requirement of intersubjectivity. It is no surprise, either within Kant or McElroy, that substantive unanimity imports a methodology of interstate contractuality and that the search for a peaceful and "regular pattern of interactions," therefore, still lacks a convincing pathway. The dialectic, drawn from Kant's "law of states" and Kant's "cosmopolitan law" that would protect citizens within states, is categorically obedient to the classificatory in ways that stifle progress along intellectual and real world pathways.

How, then, to break out? As McElroy far less ashamedly has morality a part of foreign affairs than Thomp-

son, McElroy places responsibility upon the individual actor. It is the "moral politician" (Kant again) who must triumph over the "political moralist," the latter given to expediency and the former given to such departures from traditional realism as a) Herbert Hoover's 1921 Russian relief effort, b) Richard Nixon's destruction of America's chemical and biological weapons, and c) Jimmy Carter's Panama Canal Treaty. By contrast, there is Dresden, although here McElroy is too ready to write of circumstances, the slowing of the Eastward infantry drive in 1944, etc., than he is of the theoretical and actual differences between moral and amoral choices.

In short, the weakness of McElroy's first book is not in its scholarship or its writing. It is in its inability to yet define a theory that would suggest how to propel the moral norm towards intellectual and international progress. Descriptiveness too often subverts prescriptiveness. McElroy settles for explanations of why the clarity of the norm—and why better rates of moral compliance occur when national security is less involved—predict the moral choice. There is a link between the moral choice and the demands of someone's "real" world to be sure, but it is not only involved with, but dependent upon, the underlying linkage of international relations theory with both reality and the deepest intellectual understandings of how both morality and reality are known.

With Thompson, the gap between morality and reality is wider still, Thompson's ambivalence about the place of morality in international affairs being obvious from the start. Substantively, this is a rich book, a bevy of seedbeds for moral discourse being planted in discussions of nuclear security, disarmament, human rights, and the conflicts over values that marked the decisions of Lincoln, Eisenhower, Kennedy, Johnson, and Nixon. There are attempts to sew together the morality versus reality threads that weave their way through the book in chapters on history as end point or beginning, national decline, and the openness of history when seen from any contemporary perspective.

But Thompson's problem with perspectives, briefly stated, is his lack of systematic differentiation. His discussions of intellectual contrasts fail to deal with the epistemological, that is the "forms of knowledge," differentiations that underlie all ethical positions. Thompson's oft written-of education, the University of Chicago's morally skeptical Morgenthau sharing top billing with the ethical admonitions of Neibuhr, apparently did not lead Thompson to an understanding of the role of epistemology in theory. Thompson properly decries the lack of continuity among "the seemingly endless repetition of intellectual and philosophical debates." But even his worthy developmental portrayal of the movement from international law, through political, to institutional approaches within international relations studies, does not bind the development to commensurable contrasts between methodological and epistemological approaches. As a result, the thoughtful portrayal of international realism never discovers the roots of the almost static analysis of qualitatively similar variables of human nature, state-to-state relationships, undifferentiated kinds of power, and the like. As a further result, the less thoughtful but still credible depiction of idealism finds the principle intellectual tension in international affairs to lie within the dualism of each human nature, not the differentiation between and among various human natures. Even the description of Lincoln's Sanitary Fair speech contrast between a man's living off the "product of his labor" or "the product of other men's labors" would leave one to think the tension's options equally weighted. Did Lincoln think that? Does Thompson?

What is so frustrating is that Thompson, at least at times, comes so close to seeing things, as they must be seen if theoretical advancement is to occur, from the intellectual top. His marvelous description of the maintenance of tensions within the foreign policy of Richard Nixon, the best segment of the book, credits Nixon's own deep understanding of that tension for the success of the China strategy and other Nixon triumphs. But in Thompson's general discussion of history, his proper scoring of Fukuyama's wrongness about Hegel is dulled by his own wrongness about Hegel. Thompson's earlier attack on Hegel's drive for the imposition of the state on a population fails to recognize the primacy of the ethical state within Hegel's prescription. Yes, there is a deterministic rationality to Hegel, but it is an ascending, differentiated rationality that Hegel strives for, and Hegel deals with it through a dialectic of tension that is epistemologically distinguishable from the dialectic of Kant.

Not surprisingly, Hegel is suspicious of liberalism, more so than Kant, but that squares with Hegel's dialectic dealings with ever more differentiated cognitive variables. Kant's dialectic steadfastly held to the classificatory imperative. Kant's dialectic is a dutiful first cousin to the Categorical Imperative that was at the heart of all of Kant's thinking and, not surprisingly, therefore, Kant saw perpetual peace as growing out of the contractual form that McElroy identified. Hegel's peace is the product of the engrossment of the ever more complex intellectual and ultimately real world variables that somehow reconcile today's reality and tomorrow's thrust of the historical Idea.

Perhaps the greatest dialectic, for international relations theory as well as for the real world of international relations, is the never ending conflict of Kant-like and Hegel-like dialectics themselves. First and foremost, both German idealists dealt with the core epistemological issues of the nature of knowledge, as well as the limits to knowledge. It is on their epistemological writings, not the substantive writings on politics or peace, that international relations theory should be built. Kant did not believe in the synthetic, differentiated cognition beyond the most restrictive notions of mathematics, natural science, and personal morality. As a result, the human purposiveness of his vision of international affairs, like his vision of the improvement of knowledge, was overshadowed by the hand of Providence. Hegel's belief in the human capacity to understand ever more differentiated, or synthetic, qualities of knowledge, along with his belief in the expansion of the limits of knowledge and morality, was the core of his optimism. That Kant would reject the coming into objective consciousness of an ever-improving moral vision, and that Hegel would depict the dialectic as a movement of consciousness towards just such a vision are both understandable and commensurable. That proponents of the realist and the idealist visions of international relations would differ over the cognitive reconcilability of the two very different cognitive entities of reality and hope is also understandable and commensurable. Indeed, it is the essence of the contrast between the realist and the idealist perspectives.

It is this contrast, I would hope, that the later books of McElroy, and even Thompson, would speak to. If they do, the progress of understanding in international relations will be as assured as the contrast between the traumas of the present world and the peacefulness of the world's future.

University of South Carolina

WILLIAM P. KREML

Democracy and Foreign Policy: The Fallacy of Political Realism. By Miroslav Nincic. New York: Columbia University Press, 1992. 200p. \$37.50.

In this book, Miroslav Nincic examines, explores, and refutes a notion promulgated by such proponents of classical "realism" as Hans J. Morganthau and George F. Kennan. The notion holds, in Nincic's words, that "democratic foreign policy suffers from having to accommodate the sentiments of the public and its representatives, sentiments that are grounded in a combination of factual ignorance and emotive drives that clash with the tenets by which international affairs are best managed" (p. 152).

In a nicely-organized and fluidly-written argument, Nincic focuses on the American experience to examine the impact of public opinion, of Congressional consideration, and of electoral politics on the development and conduct of foreign policy. He argues that realist arguments essentially suggest three rules: ideals should not be confused with interests, interests and power should be "brought into proper balance," and "the actual conduct of foreign affairs should proceed in a measured, consistent fashion" (pp. 22–23). He concludes that, in general, democracy does quite well by these (rather mushy) criteria.

Public opinion—"the mentality of the masses"—is really quite reasonable and coherent, he argues. While the average American may not be terribly well-informed about foreign policy, the public is not "particularly disorganized, unstable, or extreme regarding foreign affairs" (p. 45). In general, it appears to be "moderate" and pragmatic, and its "normative goals are actually quite close to the foreign policy preferences of the leadership groups from which policy makers are generally drawn" (p. 52).

He reaches a similar conclusion about Congress. Although he acknowledges that "instances of misguided comportment by the nation's legislators do exist," he concludes that "examples of considerable Congressional wisdom can also be found" (p. 64). And while conceding that electoral politics can sometimes adversely affect the conduct of foreign policy, he argues that there are also positive benefits in elections as they enhance the exchange of ideas and hold political leaders accountable for foreign policy failures (p. 119).

He also concludes that there has often been a tendency in American foreign policy for leaders to exaggerate, for domestic political reasons, the degree to which there is an outside threat. In this, he may be overstating the case somewhat. If the threat was indeed exaggerated, it seems to me that political leaders largely believed their own exaggerations.

In the final chapter, Nincic wades into the decadeslong debate—for which the classic realists bear eternal responsibility—about what is, after all, the "national interest." As he demonstrates once again, the more one struggles with this concept, the more one becomes enmeshed in muddled vapidities and "limp tautologies" (p. 158).

He concedes that the concept might have some objective validity if it is very narrowly defined as a quest for physical survival, economic health, and the continuance of the society's basic normative order (p. 161). But even that concession is questionable. The Soviet Union appears recently to have peacefully decided that its national interest lay in its own physical disintegration and in a radical restructuring of its basic normative order. Similarly, many leaders, such as the Ayatollah Khomeini in Iran, would consider the economic health of their country to be of only very secondary importance. Nincic concludes that, in general, "a national interest emerges only from an authentically democratic aggregation of domestic preferences" (p. 168). That may be about the best way to deal with the issue (apart from abandoning it entirely), though it is not clear why the aggregation has necessarily to be "democratic."

With his intentional focus on the American case, Nincic specifically eschews a comparative perspective (p. 24). In some important respects, however, his argument might have been strengthened with a bit of comparison. That is, while he argues convincingly that American foreign policy has shown a fair amount of moderation, coherence, and wisdom, his case might be stronger if that experience were juxtaposed with the alternative.

While democratic governments have made their share of foreign policy blunders, these, it might well be argued, pale in comparison to the foreign policy disasters non-democratic countries have experienced under such leaders as Hitler, Mussolini, Kim Il-Sung, Khomeini, Nasser, and Saddam Hussein. And one might also look at the quality of the people democracies have generally put forward to run their foreign policy, comparing them to the similar products of non-democratic societies. The overall record for non-democracies, after all, is fairly abysmal. Rebecca West may exaggerate somewhat when she observes in Black Lamb and Grey Falcon (1941) that in 645 years of rule the Hapsburg family produced "no genius, only two rulers of ability ... countless dullards, and not a few imbeciles and lunatics," but she is not that far off the mark. In such a comparison, it would seem, democracies do rather well. And a consideration of this sort might have served to enhance Nincic's basic argument.

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From Confrontation to Cooperation: Resolving Ethnic and Regional Conflict. By Jay Rothman. Newbury Park: Sage, 1992. 231p. \$51.00 cloth, \$24.50 paper.

In the late 1960s and early 1970s several scholars of international relations developed conflict management training workshops, the purpose of which was to support a process towards peace in the context of intractable conflicts. John Burton, Leonard Doob, and Herbert Kelman, among others, conducted "controlled communication" or problem solving workshops with high-level representatives of groups involved in protracted communal disputes. John Rothman's book presents an extension of those early action/research projects. Applying principles found in the conflict analysis and conflict

management field, he examines the Israeli-Palestinian conflict to illustrate the utility of a particular conflict management method which he has developed. Rothman is a scholar-practitioner at the Leonard Davis Institute for International Relations at the Hebrew University of Jerusalem, who has conducted a variety of workshops in the Middle East with Israelis and Palestinians. His book presents an effort to combine conflict theory and practice and to apply them to the particularly intractable Israeli-Palestinian conflict, with an eye to their generalizability to other similar conflicts.

Rothman's book, however, does more than describe his conflict management method. In an important effort to span what some believe to be an erroneous theory-practice divide, the book discusses the interrelationship between practical methods and theoretical perspectives, both in the international relations and conflict resolution fields. The author challenges the traditional international relations approach which places primary emphasis on national interest concerns, and he recommends turning politics "on its head" in order to study ethnic and other "primary" communities from the inside.

Rothman suggests a kind of bottom-up analysis as a complement to the study of nation-states, and he argues for his approach not so much on a theoretical basis, but primarily as a practical matter. He points to the recent explosion of ethnic and regional conflicts as a clear indicator that the major actors of concern in the world are not only nation-states, but also ethnic identity communities which are seeking to preserve their existence. In order to gain an understanding of these communities, they must be explored "from within," that is, from the perspective of the members of those groups. In response to potential criticism that he focuses too heavily on "low politics," Rothman claims that it is the over-focusing on the "high politics" of nation-states and national interest that has allowed the international relations field to fail to deal effectively with current world events.

Rothman also provides an important challenge to the conflict management field. At first glance, the move away from a confrontational, adversarial process of dealing with conflict toward more cooperative modes as articulated by Rothman will appear as an old refrain to scholars in the conflict analysis and resolution field. However, while many conflict theorists focus on the importance of clearing up parties' misperceptions of each other (which Rothman labels an empirical approach to conflict) and on the potentially transformative effect of having each party "step into the shoes" of the other (labeled a reflexive approach), this book adds a new twist to the process of transformation. The author asserts that parties also misperceive or fail to examine their own underlying assumptions and ontological perspectives. For this reason, an essential piece of the process of conflict management is a stage in which the parties become something akin to critical theorists whose task it is to probe the rules and patterns that determine their own beliefs about themselves, the other party, and the conflict, to critique them, and then to transform the existing terms of the dispute. From this perspective the salient focus of analysis for parties to disputes, as well as for scholars, is less on events or the distribution of resources, and more on transactions and relationships between ethnic or regional communities as key variables. Conflict is rooted in troubled relationships and the failure to meet "universal human needs" of all parties such as security, control, meaning, and identity.

The implications of this position for intervention are somewhat surprising and intriguing. The conflict management method developed by Rothman involves a three stage process which, unlike most other conflict resolution methods, includes a stage in which disputants are encouraged to frame the conflict in adversarial terms. Rather than encouraging adversaries to first vent frustrations and then find common ground, as is the case in many models of intervention, his model calls on parties to first describe the conflict to each other in the very adversarial terms which are presumably contributing to friction. The goal is to provide a comparison for the later stages of reflexive analysis and integrative bargaining. In each of these three stages, the model has participants describe the conflict, identify causes, and generate and evaluate solutions. It is the process of proceeding through these stages and finally of making comparisons among them that the transformative potential lies, according to Rothman.

The structure and content of the book reflect both the "bottom up" type of analysis recommended by the author, as well as his implicit blending of theory and practice. The book is divided into three parts, each of which articulates the themes of the book in a different way. The first of these provides a brief "cultural history" of the development of Israeli and Palestinian nationalism, describes and evaluates four major conflict management approaches, and argues for a model for conflict management, called the ARI (adversarial, reflexive, integrative) Conflict Management Framework, which encompasses all of these approaches. In Part II, the reader is presented with three applications of the model to actual disputes, two of which are workshop applications and a third which utilizes the ARI framework as an analytical tool. The final section of the book presents a set of materials intended for use by conflict management practitioners in the development of training workshops, including an intervention and training script, two case studies, and a conflict simulation exercise. Over 60% of the book is committed to discussion of the applications presented in Parts II and III.

The primary weakness of this otherwise valuable book is its lack of attention to the issue of getting parties to the table. The ARI Conflict Management Framework assumes that representatives of disputing communities have already agreed to negotiate or participate in a cooperative process. One is left wondering whether this method might be used in some way to convince parties to participate in a cooperative process or whether ARI practitioners must wait for the arrival of the right conditions before embarking on problem solving workshops. This is a problem for all such conflict resolution approaches to international and ethnic conflict, and it is unfortunately not tackled head on in this book. At the time of this writing an Israeli-Palestinian negotiation process has resulted in the first agreement on the road towards Palestinian independence. This book raises the question of whether Arafat's willingness to take part in a conciliatory negotiation is best explained from an IR national interest perspective or a Rothman-like bottom-up perspective, or some combination of these approaches. Hopefully, works like From Confrontation to Cooperation will encourage scholars to investigate such

This book should be of great interest to both international relations scholars as well as conflict management academics and practitioners. In the post-Cold War era, scholars of international relations are increasingly focusing their attention on entities other than nation states in an effort to understand the often chaotic and virulent ethnic and regional conflicts which have erupted as nations such as the Soviet Union and Yugoslavia have broken apart. This work should provide scholars with a very useful jumping off point for approaching such conflicts in new ways.

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China's Just World: The Morality of Chinese Foreign Policy. By Chih-Yu Shih. Boulder: Lynne Rienner, 1993. 244p. \$37.00.

Shih, of National Taiwan University, endeavors in this book to shed light on why the People's Republic of China does what it does in foreign policy. Shih's argument rests upon the premise that up until the 1980s, the Chinese government based its legitimacy and power on a superior moral vision, which vision explains the contradictions of Chinese foreign policy. As illustration, one interesting inference is that the Chinese had several foreign policy themes, and thereby were able to enact moral dramas in which tangible resources or advantages accrued from the pursuit of one theme could be (in fact, might have been just so that they could be) publicly and virtuously sacrificed for the sake of another theme whose importance was now seen to be paramount.

Shih knows that this approach demands the synthesis of numerous disparate strands of theory in international politics: national role conception, domestic politics, statist theory, political culture, realism and neo-realism. This approach is seen as superior to the power politics paradigm which could never reconcile Chinese power (or, rather, lack thereof) with Chinese foreign policy rhetoric and action. Shih infers the "moral scripts" that China played with the Soviet Union, the United States, Japan, and the Third World. These comprise the bulk of the book's chapters. He shows how China repeatedly eschewed tangible economic and political gains with these nations for the sake of moral purity.

The book only partially achieves its aims due to two interrelated areas of concern: one, the problematic theoretical distinction between Shih's psychocultural model and a power politics model not constructed of straw; and two, the uneven persuasiveness of the "script" chapters. It seems to me that a strong moral vision, whatever its particular merits and cultural origins, is the primary mainstay of regimes that are vulnerable both at home and abroad. What other power resource does such a regime possess? Moral legitimacy first and foremost keeps the domestic wolves at bay. Solid domestic support simultaneously upholds and requires moral dramas to be enacted abroad, which dramas can be seen as successful despite (and perhaps because of) negative feedback from the international arena. I see "strong moral vision" as one possible route to consolidation and balancing of power both domestically and externally, and not as an endeavor separable from or antithetical to such concerns. Shih's approach may broaden the operationalization of what is meant by power, but that claim is extant in the literature that seeks to modify and enrich the straw man version of power politics, for example,

that of Etheredge on the dramatic requirements of American national scripts, or the research emanating from Putnam's view of the "two level game" of domestic/international politics.

The "script" chapters often lack persuasiveness because Shih insists on demonstrating that the psychocultural approach is superior to a power politics approach. Thus he contrasts what China did with what it should have done if it were interested in tangible power gains rather than moral purity. However, as discussed above, intangible power gains in international politics can be as real to certain regimes as tangible power gains—and perhaps even more important if one factors in the domestic political scene. Although factions in the Chinese political elite are alluded to, there is no attempt to systematically tie in the resultant domestic fortunes of those who advocated certain foreign policies, or how foreign policy might have been used as a weapon or a

carrot in factional politics.

Because of Shih's determination to find a contrast between his approach and power politics, he ends up with very questionable interpretations of certain events. For example, he explains Mao's strident anti-revisionist rhetoric against Khruschev as "the result of a cognitive need for consistency on anti-imperialist logic" (p. 68), though on the very next page he describes China's "furious response to Soviet betrayal" due to Khruschev's "attempt in 1958 to put China under Soviet military control," and later suggests that "they (the Chinese) wanted to be the enemy of the Soviet Union' (p. 79). I think we can explain the stridency of China's anti-revisionist rhetoric as both a function of its need to clearly condemn the moral impurity of the Soviets (thereby enhancing its own purity) and a need to protect its more traditionally defined national interests from a nation that had already shown itself hostile to those interests. There is no theoretically necessary dichotomy here. Examples of such painfully contorted reasoning that presumably exclude the possibility of a power politics explanation of Chinese foreign policy abound in this volume, and seriously detract from it. The most persuasive of the four "script" chapters is, in my opinion, the Japanese script, and readers interested in the prospects of closer Sino-Japanese ties should find much of use there.

Shih's final chapter is simultaneously the best and the most provocative. He argues that China, through its own bungling, can no longer act as a moral regime, and now, after the Cold War, finds itself acting as an out-of-step realpolitik regime in a world where neorealism is ascendant. According to Shih, the Great Leap Forward destroyed the Chinese people's faith in the moral superiority of their leaders. This led to a severance of the linkage between internal and external politics, as China's external behavior became irrelevant in judging the domestic political legitimacy of any faction. This severance invited a return to legalist/statist principles which "emphasize the wealth and strength of a nation, not its moral standing in the world" (p. 202). China is therefore eager to join the economically interdependent world, but is not ready to sacrifice national independence and autonomy. The neorealist vision of international cooperation is based upon shared moral and legal norms, which make possible some relinquishing of national autonomy. China is neither capitalist nor democratic, and thus cannot fully cooperate, nor persuade Western powers to fully cooperate with it. The resulting

mistrust leads to confrontation (such as over human rights) in which China must show its power—now defined in confrontational power politics terms. And unless China can show its people it is respected in the world for its power—since its moral purity will no longer suffice—the people will respect the regime even less than they already do, which could lead to severe internal

crisis. Shih predicts that China will use force in the near term (over the Nanshas or Taiwan) for this very reason. Such a display would also be consonant with the psychocultural Chinese penchant for the dramatic in foreign policy.

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ARTICLES

CHOOSING JUSTICE: SOCRATES' MODEL CITY AND THE PRACTICE OF DIALECTIC

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laucon's demand to be shown the inherent choiceworthiness of justice exposes the limits of dialectical argument. Acknowledging these limits, Socrates proposes that his interlocutors join him in an alternative activity, making a city in speech. This model city proves to be "entirely opposite" to existing cities, above all (as Socrates observes) because it restricts the practice of dialectical argument to those who first demonstrate a capacity for synopsis, that is, for seeing things as a whole. Socrates holds that one must be able to see things as a whole in order to benefit from the use of dialectic. I interpret the political institutions of Socrates' model city accordingly, as being instrumental to the practice of dialectic. Hence, I reject the prevalent readings of the Republic, which present these institutions either as a blueprint for public policy or as a parody of political idealism. Instead, I suggest that the interlocutors' discussion of censorship, the noble lie, and communism is propaedeutic, fostering the synoptic capacity necessary to benefit from the practice of dialectic, including dialectic aimed at revealing the choiceworthiness of justice.

ridway through his conversation with Thrasymachus in book 1 of Plato's Republic, Socrates is persuaded to postpone his quest for a precise definition of justice, for "what Thrasymachus is saying now seems to me to be much more important" (347e2-3).1 Thrasymachus, we recall, had abandoned his original thesis—that justice consists in the advantage of the stronger—and has taken up a new position, contending that the unjust life is stronger and better than the just life. This shift prompts Socrates to turn to his young companion, Glaucon, and pose two questions: "Which way, Glaucon, will you choose? And which seems argued the more truly to you" (347e4-6)? The second question is by no means superfluous.² Socrates evidently anticipates that Glaucon's preference may rest upon something other than the better argument. Indeed, it is not so much clear thinking as native breeding that Socrates credits when he praises Glaucon's inclination toward justice (tên physin . . . tropou, 367e6-368b2). Quality of breeding is an admirable thing, of course; but it is hardly to be mistaken for mature human excellence. Nor is Glaucon's tropos—his present "bent" or inclination-indicative of a firm or decisive choice for justice. On the contrary, Glaucon remains fascinated by the tyrannical, or perfectly unjust, life. Moreover,

locutors' rambling consideration of justice. Justice, he suggests, is intelligible as a tertiary kind of good, desired not for its own sake but only for its consequences—consequences that might be acquired even more efficiently by other means. Moreover, as Socrates testifies, Glaucon is "always and in everything superlatively courageous" (aei te dê andreiotatos . . . pros hapanta, 357a2–3). This superlative courage or manliness (the Greek permits either translation) is perhaps most evident in Glaucon's taking up the cause of injustice. For he thereby puts his own admirable nature in jeopardy in hope of acquiring mature human excellence. Still, it is one thing to recognize such courage as a prerequisite in the quest for mature human excellence and quite another to tout it as a sufficient criterion by which to decide whether or not justice counts as that excellence. Yet Glaucon declares (putatively for the sake of argument) that he will henceforth accept justice as human excellence only if it can be shown to profit a "real man" (hôs alêthôs andra, 359b2). The real man, as Glaucon conceives him, is not shackled by the bonds of necessity that tie others to the civic community. It is inferior human beings who are compelled to seek justice as an alternative to what they most fear (suffering injustice) and what they naturally desire justice in itself. Socrates must show the power of justice when it is present all by itself in the soul, apart from its wages and consequences (358b4–7, d1–2). Nay, Socrates must reverse the reputations of the just man and the unjust man to ensure that the judgment of their lives will be rendered correctly and not settled for the sake of consequences alone (360e1–6, 361b5–d3).

Glaucon claims to be unimpressed by conventional teachings in favor of justice. So besides providing the indispensable root of extraordinary courage, Glaucon's manly spirit evidently encourages an exaggerated sense of his own freedom from mere conventionality. He needs reminding (as Socrates points out) that the prisoners in the cave of political culture are "like us" (515a5). In view of Glaucon's exaggerated sense of independence, we may expect that beneath the veneer of his bold challenge to conventional justice lies a core of unexamined conventionality. Glaucon, we may say, does not sufficiently know himself. Adeimantus, his fearless yet less enterprising brother, is more perceptive than Glaucon in this regard. Undaunted by his brother's dazzling account of the superiority of injustice, Adeimantus coolly notes that Glaucon has failed to identify "the very thing that most needs to be said" (362d5). Adeimantus elaborates this most needful consideration, which will in good measure determine the course of the interlocutors' subsequent conversation. In so doing, Adeimantus observes that Glaucon's challenge to Socrates fails to take into account the influence of customary teachings concerning virtue and vice on the souls of the youths who hear them (365a6–b1). As a result, Adeimantus suggests, Glaucon fails to realize that even he is not yet in a position to make a meaningful choice between justice and injustice—no matter what Socrates says by way of response to his challenge.

According to Adeimantus, the customary teachings concerning virtue and vice subvert the soul's capacity for acquiring mature human excellence. Due to a distortion of soul induced by such teachings, we too may be ill equipped to make the choice that Glaucon longs to make and that we too must make if we are to move beyond dispositional inclination toward mature excellence. It is with this counterproductive education in view that Adeimantus bids Socrates "not only to show us by rational argument that justice is mightier than injustice, but also to show what it is that each by itself makes [poiousa] of the one who possesses it" (367b2–5, e1–5). Though less dazzling than his brother's challenge, Adeimantus' charge is in fact more radical. For Glaucon's challenge merely calls for a static account of justice's superiority, an account revealing justice as pleasing to the human soul as currently disposed. But Adeimantus correctly discerns that Glaucon's desire to make an unprejudiced and fully informed choice between justice and injustice requires a dynamic account, an account recognizing that the choiceworthiness of justice depends upon its unique capacity for healing the deformity from which the conventionally educated soul currently suffers.

SYNOPSIS AS A CURE FOR PSYCHIC DEFORMITY

What, then, according to Adeimantus, is the conventionally induced deformity of soul that stands in need of healing? Adeimantus begins to elaborate the nature of this deformity by noting the counterproductivity of the arguments customarily offered to encourage justice. Parents and poets (who are so often the architects of civilization) conspire in citing for the edification of youngsters the most tangible and appealing consequences that can possibly be ascribed to justice. As it turns out, however, such tangibly appealing consequences do not belong exclusively (or even necessarily) to justice. So the speeches made in support of justice with the best of intentions only inflame a callow desire for things that are, if anything, more easily obtainable by foul means than by fair. When youngsters raised in this fashion encounter arguments of a different cast contending that injustice is shameful only by opinion and that the gods themselves are willing to overlook one's crimes in exchange for ceremonial bribes, they realize, Adeimantus says, that "one ought to turn wholly toward appearances" (363e5-365c3). It is the exclusivity or one-sidedness of this "turn" that deforms the soul. For a soul turned wholly toward appearances is indeed like a captive compelled to gaze at shadows cast upon the wall of a cave. The human soul in such a condition is deprived of its capacity for reflection upon the linkage, be it ever so tenuous, between shadow and substance. When appearance becomes everything, appearances no longer mean anything. Once freed of any mooring in reality, appearance no longer offers even an oblique clue to what truly is. Appearance then exists as nothing more than an invitation to the projection of fantasy and a temptation to willful manipulation. As a result of the soul's turn wholly toward appearances, reason becomes cramped and deformed. Reason ceases its search for the true and the good in considering the merits of justice and injustice; instead, it is wholly subsumed in "calculation" (logizomenos), specifically in that calculation which concludes that injustice is mightier than justice always and everywhere as long as one has the capacity to manipulate appearances (366a6-7). From this perspective the comparison of just and unjust lives becomes a forgone conclusion rather than the preparation for a decisive choice. Thus Adeimantus perceives that one's soul must be healed—its whole orientation toward appearances corrected before one is prepared to make a judgment regarding human excellence. He perceives, moreover, that it is unlikely that any further argument could provide such healing (366b3).

It is, of course, paradoxical to identify Glaucon as a victim of the conventional, soul-deforming process of

childrearing that is described by Adeimantus, for it seems that much of what we find appealing in Glaucon derives from his unyielding rejection of convention. But it would be premature, to say the least, to describe Glaucon's scorn for social bonds as sufficient to effect the genuine turnabout, or periagôgê, that Socrates later identifies as essential to the soul's pursuit of wisdom (518c4-d1, 520c5-8). Yet Glaucon hardly seems guilty of the fixation upon appearances condemned by Adeimantus. If anything, Glaucon may be said to hold appearances (i.e., what seems to be, as distinguished from what truly is) in utter contempt (357a4-b4, 361c4-d3). But one wonders whether the scorn and contempt that one observes in Glaucon's rejection of conventionality is a mark of liberation or is itself an advanced symptom of the psychic distortion Adeimantus has described. Perhaps Glaucon's demand to hear justice praised "in itself and for itself" (auto kath' hauto, 358d2) is merely the inversion typical of psychological denial, in which he transposes the theme Adeimantus finds characteristic of traditional childrearing. Consideration of this possibility might remain purely speculative were it not for the fact that Socrates, too, indicates the inadequacy of Glaucon's contemptuous turn from appearances. For Socrates identifies as "the most beautiful or noblest" (kallistôi) not the kind of good that Glaucon describes as desirable "in itself and for itself" (auto hautou heneka, 357b6) but the kind that is to be loved "both on account of itself and on account of the things coming into being from it" (kai di' auto kai dia ta gignomena ap' auto, 358a1-2). Socrates thereby suggests that Glaucon's turn toward the thing itself might well be an impediment to the discovery of the finest, noblest, and most beautiful things, whose discernment requires a comprehensive glimpse of the thing itself and what comes into being from it. Socrates thus indicates that if Glaucon is accurately to determine the character of human excellence, he must first develop the capacity for synopsis, or comprehensive vision. This is the same capacity that Socrates later explains is necessary if one is to benefit from dialectical argument (537b8-539d7). Moreover, Socrates describes Glaucon's ideals of perfect justice and injustice as "statues," that is, mere images (360e1–361d6). Socrates thus suggests that by heaping contempt upon appearances in his probing for reality, Glaucon will never get beyond the shadowy realm of images. The reality he supposes he has discovered turns out to be just another figment upon the wall of the cave. On this basis, I hazard to say, we may conclude that Adeimantus' diagnosis indeed applies to Glaucon. Glaucon's contempt for appearances does not correct but simply inverts the warp from which the human soul suffers in the cave of political culture. Although this warp is endemic to the political cave, the case of Glaucon reveals that neither is its cure to be found in the mere rejection of political culture. The soul must receive political culture (i.e., the culture of politeia) but not in the manner of a cavedweller. Is there any way for this to occur?

Although Socrates says that he "had something in mind to say" in answer to Glaucon's challenge, the addition of Adeimantus' speech lands Socrates in a state of genuine perplexity or aporia (cf. 362d1–2 and 367e6–368b4). Socrates realizes that a logos accounting for the superiority of justice is needed and yet that no further argument can benefit Glaucon as long as he remains in his present condition of synoptic incapacity. If anything, the operation of dialectical argument will inevitably deteriorate into eristics (as happened between Socrates and Thrasymachus) if the distinctions one makes fail to correspond to the proper articulations of the whole (454a1-9). If we are not merely to quarrel but are really "to get things sorted out" (dialegesthai), we must not lose sight of the forest for the trees. Yet this is precisely what happens to Glaucon. His contemptuous turning against political convention (and against appearance generally) resembles nothing else more than "the spinning of a shard" (521c5). The shard, or ostrakon, a fragment of smashed pottery, perfectly symbolizes Glaucon's condition. For his contemptuous disengagement from political culture leaves him an isolated piece of a shattered whole. As a shard, Glaucon cannot adequately contemplate the human good. Glaucon must develop a capacity for synopsis (appreciating himself and others as members of a greater whole) if he is to heal his own soul and to attain a position from which he can accurately discern the nature of human excellence. But if dialectic is ultimately needed to reveal the truth about justice and injustice and yet would degenerate into eristics if practiced at present, how is Socrates to proceed? The salvation of Glaucon's soul-the preservation and enhancement of his capacity for human excellence—evidently depends upon Socrates' success in a propaedeutic effort that differs from and yet must prepare for the actual practice of dialectic.

Socrates' response to the challenge posed by the speeches of Glaucon and Adeimantus is to postpone further attempts at dialectic. Instead, he proposes that his interlocutors join him in the nondialectical one may even say, poetic (poiômen, 369c9)—exercise of making a city in speech. It is hardly coincidental, then, that one finds no form of the word dialegesthai in the text again until the opening of book 5, where it is spoken to denote the division of the city's women in accordance with the eidetic structure of the whole political community (see 454a4-d1). The absence of dialegesthai from the interlocutors' own conversation in books 2-4 mirrors the postponement of dialectic that Socrates requires of the citizens of his model city, which he eventually credits for making this city "completely opposite" to existing cities (497e5-498c4). The interlocutors' forbearance from dialectic in the course of founding the model city is complemented by their approval of certain controversial political institutions (censorship, the noble lie, and communism) which approval, as I shall suggest, helps to foster their synoptic capacities and thus to prepare for the beneficial practice of dialectic.

POSTPONING AND PROVIDING FOR DIALECTIC: THE SOCRATIC TURN TO POLITICS

Though Socrates' interlocutors want to see for themselves the power of justice in or on the human soul, they evidently do not appreciate sufficiently the difficulty of attaining an adequate view of the soul. To attempt such an inquiry is no slight matter. In Socrates' frank estimation, it requires a keen observer (368c7–8). So it would be futile for one who is "not at all sharp-sighted" (368d3-4) to try to investigate this question immediately. But if such a person, someone who was unable to distinguish certain small letters, were able to find elsewhere the same letters "bigger and in a bigger place," he might well consider it a godsend. Supposing that justice pertains not only to a single man but also to a "whole city" and that the city is bigger than a single man, Socrates suggests that his interlocutors behold a city as it comes into being in speech. For then their inquiry could proceed by "investigating the similarity of the larger in the idea of the smaller" (369a1-3).

Socrates' investigative strategy is puzzling. How can this strategy succeed, one wonders, except on the assumption that the two sets of letters are in fact the same? And how is this assumption to be verified if the inquirer has taken the trouble to consider the larger letters only because he cannot make out the smaller? This conundrum vitiates any attempt to construe Socrates' turn to politics as a *method* by which even the intellectually myopic can identify justice in the soul. The activity of making a city in speech is not an investigative methodology but rather a therapeutic exercise. Socratic politics are a calisthenic, not a crutch. They seek to foster, rather than to make unnecessary, the sharp-sightedness required for the success of subsequent research. If successful, the therapeutic exercise of making a city in speech will make it unnecessary to rely upon unsubstantiated parallels or correspondences. Specifically, the interlocutors' foundation of the model city will fortify their own capacities for synopsis, for seeing things as a whole. This result will, of course, be most evident in Glaucon (as we shall see), because his bold vision is at once the most penetrating and yet the least synoptic of any of Socrates' interlocutors.

As they undertake the exercise of founding a city in speech, the interlocutors agree that justice will be present in a "whole city" (holês poleôs, 368e2–3). But the precise requirements of civic wholeness are not immediately clear. The political community, by reason of serving the purposes of human living, necessarily embraces an abundance and multiplicity of human goods. But because we neither know what is the greatest of these goods nor have a basis upon which to choose reliably among the multiplicity of apparent goods, we cannot yet be sure we know when the city has achieved completeness or wholeness (thus isôs, 371e11). Being unable to discriminate among goods, one really has no alternative but to

welcome them all and then to try to determine in what manner and to what extent they might fit together. By making a city in speech, one is thus encouraged to consider the possibility of order among the multiplicity of human goods. Such an exercise cultivates the capacity for synopsis. So it is, I shall suggest, that the practices of Socrates' conversational community will heal Glaucon's soul, providing what the political culture of no existing city could provide.

Socratic Censorship and the Genesis of Political Order

Socrates and Adeimantus undertake the foundation of the first city, which Socrates describes as truthful (alêthinê, 372e7). He means that nothing here is concealed. In other words, there is in this city an unproblematic relationship between what appears and what really is. One reason for this is that the men and women of this first city hardly desire anything beyond the plain necessities that life itself demands. They eat and dress as simply as possible. They are industrious in their work, songful in prayer, and delighted in intercourse but wary of overpopulation, so as to avoid poverty and war (372a5-c1). We may say that in such circumstances, there resides truth and nothing but the truth. But we cannot say that the whole truth is present in this city. For Glaucon's peremptory rejection of the complacent lifestyle of its residents rebuts any interpretation that construes the "truthful" city to be wholly true to human nature—at least to human nature as we know it, which includes the very spiritedness epitomized here by Glaucon's reaction. It would be utopian in the most pejorative sense of the term to attempt to construct a city on the basis of the suppression of such spiritedness, or thymos. Such a city would indeed be "fit for pigs," rather than for human beings (372d4-5). A political community that is to be wholly truthful must somehow comprehend Glaucon as a member.

But what is it that the city must become in order to comprehend Glaucon as a member? The price of including spiritedness, at least in the short run, is the introduction of unnecessary desires. Glaucon desires what he calls "relish" (opsou, 372c2) as a supplement to the indispensable staples of human life. Lacking as yet any better understanding of the true relish of life, he insists upon the inclusion of the full array of goods and services commonly accepted as pertaining to an elegant lifestyle. Socrates accedes to Glaucon's demand because "nothing hinders it" (372e8). That is to say, nothing in the character of the city hinders it. Reflecting its founders' lack of wisdom, the city is in no position to exclude any good or service as long as its provision is supervised by some skilled professional. This city knows only one principle: one man, one art (370b4-6).4 Technical expertise thus holds a monopoly on authority in the first city. There is, consequently, no hierarchy or order among the myriad of consumer goods that the city permits. Like the

inflamed appetite it is obliged to admit, the city is itself limitless. It possesses as yet no principle according to which its order or even its own completion may be determined (371e9–11). In short, the attempt to supply Glaucon's demand for relish produces nothing more than a "promiscuous heap, stuffed and multitudinous" (373b3–4).

Even Glaucon's final demand—for an expert militia-merely results in the further accretion of specialists recruited for the service of limitless appetite. It may be that the city needs such warriors, but what is there to keep these watchdogs from turning wolf and devouring their fellow citizens? The prospect of civil war, confronting the founders at the culmination of a protracted period of civic expansion, vividly indicates that the city's wholeness is not to be sought in the unrestricted increase of the goods and services that it includes. This wholeness evidently depends instead upon the establishment of a principled relationship among the city's diverse parts. The Greek word for a principled relationship or authoritative order of this sort is politeia. Absent a politeia, the city remains too miserable a thing (even in the throes of its great crisis) to hold Glaucon's attention; he expresses no interest in the search for a means to defend against the warriors' abuse of power. We may even surmise that Glaucon would find wolfish warriors (who are, at least, real men) more attractive than the sheeplike city. So it is Glaucon's brother, Adeimantus, who dutifully joins Socrates in seeking an education appropriate to civic guardians (376d4).

What is most remarkable about the ensuing discussion of education is its establishment of a politeia for the city. A politeia emerges as the founders censor the practice of technical expertise in order to ensure that the city's warriors' will become true and virtuous guardians (395c4-d6).5 Something beyond the founders' exhaustive and exhausting survey of artisans and consumers finally comes into sight in the polity, which arises only with the introduction of censorship. This censorship is conducted in accordance with certain theological typoi (outlines or impressions) that are formulated with a view both to the formation of politically hygienic opinions and to the truth. The poetic tales that violate these typoi incite politically dangerous self-indulgence and "are not true, either" (378c1). It is the proper business of the city's founders to know these typoi even if the task of filling them in must be turned over to poets willing to work within their limits (379a1-4). Socrates and Adeimantus identify four guidelines that are to characterize the tales told by the poets to the city's youth:

- 1. Divinity is the cause of only that which conspires for the good (380c8–9).
- 2. Particular divinities do not deceive us by altering their proper forms (*eidei*) and assuming alien ones (380d1–6, 383a3–5).
- 3. The realm of the divine Hades is not so terrifying as to warrant the choice of a shameful life over a noble death (386a6–c2, 387b5–6).
- 4. Heroes, if they are truly the progeny of divinities,

will not lose their heads in regard to grief, vengeance, levity, dishonesty, drink, sex, food, or money (387d4–392a2).

As Socrates reveals, the practice of censorship does not stem from any independent understanding of an order among the multiplicity of human goods (392a3-11). It derives, instead, from the founders' subordination of other claims to these elements (nay, these bare outlines) of theology as they attempt to ensure the goodness of the city's guardians, which they take to be the most reliable safeguard of civic wholeness. Thus, Socrates and Adeimantus hold that even if there were to appear "a man enabled by wisdom to represent all things" (hypo sophias . . . mimeisthai panta chrêmata), he should be promptly escorted to the city's frontier and sent away; for such an expert would not "harmonize with our politeia" (397d10-398a7). Evidently, politeia (mentioned here explicitly for the first time in the dialogue) comes into sight only with the discovery of a principle of organization that outranks technical expertise. Yet surely it is paradoxical that Socrates would banish the very man he credits with wisdom. Granting that the mimetic expert (i.e., the all-wise and unsubordinated poet) would not "fit in," we may still wonder why it is the poet, rather than the politeia, that must go. Why, in other words, sacrifice wisdom for the sake of political order? And if the wisdom of expert proficiency is no longer sufficient to warrant citizenship in the model city, what takes its place? What is the nature of this new principle and how does it justify the exclusion of a wise man?

It is not easy to see that the statesman has authority over the poet, much less to identify the foundation of such authority. Yet the existence of such authority would imply that the statesman's recognition of theological guidelines—the only things that the founders as yet claim to understand—outranks the poet's wisdom concerning "all things." But in what sense, if any, is the statesman's knowledge superior to the poet's? As Socrates has formulated it, the comparison would seem to favor the poet. After all, the founders of the model city are said to recognize outlines; their knowledge is far from complete. The poet's wisdom, by contrast, touches upon all things. Hence its epic scope. Poetic mimesis thus appears as the natural rival of philosophy. For its reach over the totality of things is easily mistaken for comprehensive knowledge. But this seems to be Socrates' point: although mimetic expertise is exhaustive in its reach, it remains piecemeal in its comprehension. Despite its dazzling capacity for representing anything and everything (an arrogant king, an angry priest, a charming girl, an impertinent fool, a thunderous mob, etc.), the one thing merely mimetic expertise cannot imagine is the governing principle that prescribes the proper place of each of these beings and so provides definition to their community as a whole.

Socrates determines that the poet is properly subordinate to the statesman, despite the fact that the statesman's knowledge is much less exhaustive than Choosing Justice June 1994

that of the poet, because the knowledge the statesman possesses is at least a knowledge of something whole, even if it is by no means finished or complete. Thus, Socrates distinguishes his own enterprise of making a model city from traditional poetry (which is antagonistic to philosophy) by expressly repudiating within his model city the mimetic pretension to exhaustive knowledge. Socrates thus selects as a benediction for his model city Hesiod's line "Half is more somehow than all" (pantos, 466c2-3; cf. Hesiod, Works and Days 40). Hesiod is deemed here to be really wise (tôi onti ên sophos, 466c2)—to possess real wisdom, that is, as distinguished from the merely exhaustive (pantodapon, 398a1) technical wisdom of the mimetic expert. Real wisdom, which is comprehensive, discerns precisely what is indiscernible in "all." It is in regard to this defect that we may say that half is more somehow than all, for order and wholeness are lacking in a mere totality. For example, a division, disciplined and properly mustered, is more somehow than an entire army routed and scattered in the field. Half is more somehow than all because half, as a ratio, already measures—and thus somehow conveys—the significance of the whole. A random heap (as Socrates regards the feverish city to be) may well exhaust all that a city comprises, but it does so without being in any way able to comprehend it. As nothing more than an exhaustive survey of a totality, "all" fails to preserve or to account for the order of the whole.

As a consequence of the founders' imposition of censorship, the model city has undergone an extraordinary change. Thanks to the founders' care in ensuring that its warriors receive an education conducive to civic harmony, the model city has acquired a principle of order. The presence of this politeia evidently makes the city sufficiently intriguing to draw Glaucon back into the conversation. Glaucon had stretched himself toward the satisfaction of overwrought, limitless, and fundamentally private appetites in his longing for something more than a merely porcine existence. Yet in so doing Glaucon runs the risk of alienating himself from any possible human community; thus he doubts that the expression "everyone" includes himself (egô . . . kindyneuô ektos tôn pantôn, 398c7-8). But when a politeia (which is likewise not to be found in the midst of a mere "all") does arise among the interlocutors, Glaucon is able to integrate himself into it. Specifically, he places his own musical know-how at Socrates' disposal in considering which harmonies and melodies are appropriate in the songs to be taught to the city's youth. Glaucon agrees with Socrates that harmony and rhythm must correspond to the words of a song and that these words must fall within the typoi already specified (398d4-10). The politeia, then, not the art of music, proves to be the founders' guide to what is truly harmonious or musical. Glaucon accordingly joins Socrates in purging all but the Dorian and Phrygian modes, which properly represent the steadfast endurance and measured sobriety characteristic of a really courageous or manly man (ontos andreiou,

399a6). The other modes, which from a technical standpoint are no less harmonious than these, are taken away because they are useless to a real man and thus do not harmonize with the city's *politeia*. Glaucon thus discovers that this own manliness, which had caused him to distrust social convention and to isolate himself from community, in fact "fits in" or harmonizes with the model city's *politeia*.

The presence of politeia enables Glaucon to contemplate the purified city as a whole and so as something clearly different from the city of luxuriance. In the city of luxuriance, there was nothing to integrate or to inform the diffusiveness of Glaucon's longing for something more than the life of a pig; nor was there anything to delimit the city's own indeterminacy and incompleteness. Glaucon's erotic longing was scattered into a mob of inflamed and limitless desires. Though the purgation of the model city deprives Glaucon of the vicarious satisfaction of his feverish desires (see, e.g., 404b4-e6), it nevertheless informs, reconstructs, and thus holds the prospect of satisfying Glaucon's fundamental eroticism. Plato, the consummate dramatic artist, here delineates a genuine development of character in Glaucon. Only if we keep in mind the distinction between eros and limitless desire, however, will we see that Glaucon's new and surprising sobermindedness (399e7) truly is "in character." Socrates indicates the relevance of this distinction for our consideration of Glaucon by likening the human soul to another "Glaucon," namely, the mythical sea Glaucos, whose original nature is hardly visible because of the mutilations of the waves and the accretions of shells and seaweed (611c7-d7). Inflamed and limitless desires are the mutilations and accretions which presently obscure the original nature of eros. In its unperverted nature, eros is a quickening of the soul responsive to evidence of the beauty of cosmic order and, consequently, is itself measured and musical (403a7–8). It is an erotic quickening of this sort, provoked by the presence of politeia, that leads Glaucon to overcome his isolation and to join the others in "being sober-minded." Indeed, Glaucon's membership in the polity of inquiry, including his participation in the purge of the city, is more truly an expression of eros than is his most extreme ultimatum demanding the satisfaction of limitless desire.

There is surely, then, no "abstraction from eros" in the present passage. What occurs is better understood as a first step toward healing or recomposing the mutilation of eros, a mutilation that poses a formidable impediment to the development of Glaucon's capacity for synopsis. The model city's politeia, which emerges with the founders' institution of censorship, facilitates the reassembly of the scattered fragments of Glaucon's eros. As a result, Glaucon now recognizes the real man as a member of the model city and thus as belonging to a genuine politeia, rather than as a maverick proudly freebooting the thralls of Leviathan. Glaucon realizes that he belongs among the city's founders as a member of the polity of inquiry. Gone, then, are the qualms concerning his

inclusion among the others. Instead, we find Glaucon swearing repeatedly in consternation at the discovery of the limits of his capacity to perform his share of the founders' work (400a4, c6)!

Socratic censorship gives form and integrity to the political community primarily by determining what is fitting for the formation of warriors into true civic guardians. Censorship thus counters the danger of civil war, harmonizing the city and adumbrating its integral wholeness. Glaucon's reflection upon Socratic censorship induces a psychic reformation congenial to, but not yet sufficient for, synopsis. Synopsis requires more than a sense of belonging or a recognition of membership. Even among members, there is still the danger that partisanship will undermine synopsis. So before entrusting the statesmanly responsibilities of political office to the best of the warriors, Socrates will suggest that they must first pass certain tests designed to challenge their judgment and their power to resist temptations to place partisan interest ahead of the common good. It is likewise not enough that Socrates has overcome the isolation of Glaucon. Although Glaucon now acknowledges his incorporation as a member of the polity of inquiry, he must still be induced to care for the good of this polity in a nonpartisan way. We shall see that the noble lie, which Socrates contrives to address the need for nonpartisan rulers in his model city, will also help to prepare these rulers-and Glaucon himself—for the practice of dialectic.

Nonpartisanship and the Noble Lie

Anything incapable of sustaining itself is not a whole but is merely a part of something else. To be wholly complete, then, the model city must possess the dynamic capacity to sustain itself. So the founders must be discerning recruiters of trustworthy rulers, who must themselves be discerning recruiters of trustworthy rulers. If the city's rulers should allow partisan considerations to bias their judgment in recruiting their successors, they would undermine their city and subvert the common good. Socrates accordingly notes that guardians in the complete sense of the term (teleous phylakas) will do their job not out of a concern limited to the interest of some particular part of the city but rather with a view to the good of the city as a whole (428c11-d7). A complete or perfect guardian, in other words, must possess a capacity for nonpartisan synopsis. So the founders must select as rulers those among the guardians who demonstrate this capacity. But who among the guardians can be said to possess a capacity for synopsis? Evidently no one, for Socrates says that he will only sketch an answer to this question in the initial foundation of his city; the precise selection of the most perfect guardians will be undertaken later (414a5-7, 503b4–5). Socrates' sketch—his first, rough cut at the task of civic recruitment-merely outlines the trials that are intended to reveal who among the guardians is most reliable in his care for the city.

Socrates would refuse to entrust the city to any

candidate who is inclined to be forgetful of his responsibilities, to despair at setbacks, to shrink from terrors, or to be seduced by pleasures. He designs the guardians' trials accordingly, to pose precisely these temptations. Nevertheless, Socrates acknowledges one source of temptation for which he does not construct a test. He recognizes that in addition to other dangers, his cadets are liable to "being robbed" of their convictions by speech (logos, 413b6). Yet the examination he designs to test for this possibility involves not words but deeds (erga, 413c5-d2).10 In such a case, one searches in vain for an unobtrusive measure. Of course the definitive trial of one's liability to corruption by speech would involve exposure to an expert rhetorician or dialectician. But the administration of a trial by logos would interfere with the proper development of the very trait it is designed to reveal. It is in order to avoid compromising the proper development of his guardians that Socrates eschews dialectical examination. Socrates expressly credits this postponement of dialectic as the feature that makes the model city "altogether opposite" (pan tounantion) to any other (497e5-498b3).

Although Socrates' warriors are in other ways tested "as gold is in fire" (413e1-2, 503a5-6), their prudently enforced innocence with respect to dialectic indicates that they are not yet perfect guardians in the most precise sense. Premature practice of dialectic would threaten to undermine the wholesome development of the guardians' nature (538a9–539d7). Nevertheless, these guardians cannot attain completeness—nor can their philosophical nature achieve its fruition—apart from the practice of dialectic (532a1– 2). So the founders' postponement of dialectic may be said to highlight the gap between the rulers initially recruited "in outline" and the most precise guardians, the philosopher kings, who are later said to be the proper rulers of the model city (503b4-5). Being in need (en deonti, 414b8-9) of something to bridge this gap, Socrates introduces the noble lie. It is the lie's function, in other words, to establish the continuity of these developmental stages in the maturation of the perfect civic guardian. Thus, the lie aims first and foremost to propagate complete guardians, not to propagandize the nonphilosophical masses. With this prospect in view, Socrates declares that he will undertake to persuade "first and foremost the rulers themselves" (malista . . . prôton, 414c1-d3).11

Socrates' lie, though admittedly a deception, is not meant to swindle. Before describing his own lie, Socrates reminds his interlocutors of the lies (tôn pseudôn) they had discussed earlier in connection with the guardians' elementary education (414b9). These lies were seen as providing a foundation for human education (376e2–377a7). Socrates' lie is meant to be "one of those lies." As such, Socrates indicates, his lie is a "lie in words," not a "lie in the soul." A lie in the soul, he observes, causes one to be deceived about what really is; as a result, such a lie induces amathia, that is, a condition of soul adverse to learning (peri ta onta . . . amathê einai, 382b2–3). But a lie in words uses falsehood as a medium of education

and so actually enhances the soul's capacity for learning. 12 The lie in words is useful when we are ignorant of the whole truth or when those with whom one speaks are too immature or are otherwise indisposed to assimilate the truth directly (382c6-d3). Such a lie is by no means alien to the truth. On the contrary, Socrates indicates a metaphysical linkage between the lie in words and the truth, remarking that the false and the true represent two aspects of singular eidos of words used in the education of youth (376e6–11). Socrates thus envisions a singular and yet twofold eidetic structure, which integrates certain false and true logoi. The singularity of Socrates' eidos of words may be said to represent the essential continuity of moments in the unfolding of human education. Such an eidos serves as the precondition for the experience of learning. Hence, the appearance of fundamental opposition between truth and falsehood may be recognized as merely an illusion that results whenever one assumes a standpoint outside the province of human learning.

It is with the prospect of just such learning in mind, however, that Socrates describes his lie as noble (gennaion, 414b9). Socrates' word gennaion may be translated more precisely as "generous," provided we understand this in what lexicographers call its archaic sense, namely, as something both abundantly beneficent and true bred. The beneficence of this noble lie resides in its contribution to the education of perfect civic guardians. This contribution comes by means of a specific misrepresentation concerning the character of the citizens' breeding. And in this regard, the adjective gennaion is again well chosen. For it signifies the venerable pedigree as well as the specific theme of autochthonic tales such as the one Socrates contrives in the present passage. Thus, Socrates notes that his lie is "nothing novel, but a Phoenician thing." The allusion may be meant to recall the Phoenician Cadmus, who founded Thebes with the aid of warriors allegedly sprung from the earth; it may refer, at the same time, to the Phoenixlike recurrence across time and place of such autochthonic tales. It may refer, in recalling this same Phoenix, to the specific feat of assembling a living whole out of elements as diffuse as ashes or dust. In any case, Socrates' description of the lie as gennaion and Phoenician prepares for its detailed formulation, which not only recapitulates the Theban claim of autochthony but in some sense represents the best or truest-bred example of such a falsehood or myth. Specifically, Socrates will undertake to persuade his citizens, and especially his rulers, that what they imagine (edokoun, 414d5) to be their rearing and education is only a dream: "I shall attempt to persuade first the rulers themselves and the warriors, and after them the rest of the city, that all these things that they imagined they suffered and had happen to them in our rearing and education were as dreams. While in truth they were at that time being formed and reared within the earth underground . . . and when their fashioning was finally completed, the earth, being their mother, delivered them up" (414d4-6).

But why would anyone attempt to persuade someone that he is dreaming, except to cause him, in some sense of the word, to awaken? Such an awakening is, of course, just what one needs if he is to aspire to a higher plane of intellectual perception. The ramifications of this aspiration are implicit in Socrates' notion of the nature of dreaming; Socrates understands dreaming as a condition in which one believes that a likeness is not a likeness but the thing itself (476c4-8). We may infer that the awakening that Socrates intends to encourage would involve the rulers' ceasing to mistake what they imagine education to be for what education really is. Socrates notes that the citizens imagine their rearing and education to be something that they suffer and that happens to them (paschein te kai gignesthai peri autous, 414d5-6); in short, they underestimate the importance of their own contribution in becoming educated. Socrates does not explain why human beings (particularly those who live in well-governed polities) tend to think of their own development as something that happens on the perimeter of their selves, rather than stemming more naturally from their innermost being. We may surmise, however, that the influence of political institutions leaves human beings a little awestruck. (Perhaps this is why Adeimantus takes it for granted that the city is "bigger" than a single man, while Socrates merely raises the question of their relative greatness [368e5-6].) Recognizing this problem, we may say that what is needed, particularly in good polities, is a means of keeping human beings from supposing that their own human nature is nothing but raw material molded by the conventions of an all-powerful culture. The myth of autochthony answers this need in a surprisingly direct manner. In Socrates' rendition, the finished character of mature human beings is presented as the exclusive work of nature and nature's god (415a4-7). Thus, in "the remainder of the myth" (to loipon tou mythou), Socrates states that it is a divinity that forms the citizens, a divinity who mixes gold in the genesis of those sufficient to rule, silver in the auxiliaries, and iron and bronze in the farmers and the city's other artisans (415a1-7). On this account, civic nurture plays no role at all. Socrates' lie represents the apotheosis of nature, I suggest, in order to combat tendencies toward the apotheosis of the state.

Clearly, it is false to assert that nature in the fullest sense of the word is restricted to genesis or nativity. Any account that presents human nativity as equivalent to human maturity is a lie. Nevertheless, Socrates' lie performs a needed service. It keeps the founders from becoming victims, so to speak, of their own success. For the founder's remarkable achievement in rearing their spirited and philosophical guardians will prove to be counterproductive if it leaves the impression that the statesman's dictate is the essential factor in human development. Rulers especially must be disabused of any such notion. Socrates accordingly offers the noble lie as a means of awakening first and foremost the rulers themselves from the dream (or perhaps better, the dogmatic

slumber) of conventionalism. Of course, one may object that this account of the noble lie is somewhat overwrought. For it may be expected that philosophically inclined rulers will on their own be "first and foremost" to see through the illusion of conventionalism. This is true, but it does not go far enough. Socrates goes further. For he predicts that the philosophical youngster will not only see through the illusion of conventionalism but will turn from conventionalism to its opposite (paranomos . . . ek nomimou, 539a3). Socrates divines that such a youngster will leave off honoring the ancestral traditions he was taught to honor by his political community once he gains so much as a glimpse of what is (538a9-10). Under the stress of an intellectual scrutiny that would reveal the shortcomings of any civic institution, a philosophical soul might well cleave to a more pristine idea of good and evil that can be practiced in any human community. The essential natures of other qualities will likewise be misconstrued as purely abstract principles or patterns, which would only be sullied by contact with whatever comes into being. From this pristine perspective it appears that nothing that comes into being (ta gignomena) could ever contribute anything to our understanding of what truly is. Souls inclined to stretch toward such an understanding would develop contempt for ta gignomena, including the institutions of their native land. The disillusionment of conventionalism in this way ends in antinomianism if not in outright criminality. In one blow, the city is deprived of properly solicitous rulers and the philosopher (subsequently identified as the perfect guardian) is rendered incapable of the cosmic synopsis prerequisite to his maturation in dialectic.

Antinomianism invites a form of partisanship that we may distinguish from familial or kinship-based partisanship; this form of partisanship is indeed a hazard inherent in the philosophical soul. Socrates indicates that once the philosophical nature catches sight of all time and all being, human life will hardly seem any big thing (486a8-10). The winged souls of philosophic natures "hasten to spend their time always aloft and are unwilling to do what pertains to human beings" (517c8-9). This bias in favor of the class interest of those concerned exclusively with the things "aloft" is indicative of what we may call a hyperian partisanship. Socrates' concern for the lack of steadfastness in the philosophical nature thus invites a question: Do the winged souls of philosophers truly soar, or are they merely flighty? The superciliousness of the protophilosopher does not sort well with the generosity characteristic of the truly erotic soul. For the latter "always yearns for the whole and for all things both divine and human" (486a5-7, emphasis mine; see also 402a7-d5). If the philosophical nature is not stabilized by a proper nurture and education, its native intellectual keenness will distort its judgment concerning the cosmic as well as the civic polity and ultimately tear it away even from philosophy (491b7-9). The noble lie guards against antinomianism and hyperian partisanship by sidestepping altogether the precariousness of an all-too-easily disillusioned conventionalism. The myth of autochthony motivates loyalty to *politeia* in a manner that avoids the counterproductive ascription of sovereignty to political convention. The sheer *earthiness* of the lie's account of human development helps to keep the feet of Socrates' immature philosophers planted firmly on the ground.

Of course, the political value of the noble lie is self-evident. By playing down the role of civic nurture in favor of the role of authochthonic nativity, the lie ensures that citizens will feel bound to the motherland more closely than could be effected by any notion of convention or social contract. The lie transforms conventional justice into an obligation of piety, fortifying a sense of belonging rather than merely appealing to a merely instrumental and reciprocal obligation. In deference to the divinely sanctioned and natural authority of functional diversity, the rulers "will in no way show mercy if their own offspring is born with an admixture of bronze or iron; instead, they will render the honor appropriate to the child's nature and push it away to the craftsmen and farmers" (415b6-c2). Blood, as we say, is thicker than water; but earth is thicker than either. So the rulers place the interests of their motherland ahead of their own partisan attachment to kith and kin when determining the occupational assignments of their fellow citizens; as a consequence, they will better safeguard the divisions within the city that underlie and fortify its integrity as a whole. It is no coincidence, then, that the task of civic recruitment (which calls for nothing less than this whole-illuminating division of parts) is enjoined as the rulers' "first and foremost" responsibility (prôton kai malista, 415b3-4). This formulation echoes Socrates' emphasis on persuading first and foremost the rulers themselves of the noble lie. For the sorting out of human and occupational diversity fortifies the city's constitutional integrity and, at the same time, adumbrates the work of dialectic—and so foreshadows Socrates' judgment that consummate civic guardians need to be philosophers (503b4-5). Even as the lie promotes its immediate political purpose, it is also helping to prepare philosophical and spirited youths for the exercise of dialectic.

Bred on the noble lie, Socrates' philosophical rulers will spurn the cloud-worshiping idolatry of Aristophanes' antinomian intellectuals. Inasmuch as the noble lie helps advance the appreciation of the cosmic significance of ta gignomena, it helps to stabilize the intellectual facility characteristic of philosophical natures. Because the founders will not countenance the alienation of the city's most promising youngsters from ta gignomena, the intellectual advance of these youngsters will more likely culminate in a comprehensive grasp of the cosmos as a whole. On the basis of this cosmic synopsis, they will be able to undertake the dialectical examination of their own upbringing and ancestral beliefs-including the noble lie itselfwithout becoming careless or contemptuous of their city's institutions or their fellow citizens. Thus Socrates' choice of words is precise when he says that it is as "nurselings" (trophimoi, 520d6), that is, as

beneficiaries of the city's nurture, that his philosophically inclined youngsters will see that they are not a class apart but are themselves members of a cosmic polity, a polity that transcends yet comprehends the civic polity.

Glaucon andreiotatos, most manly and courageous always and everywhere, finds the noble lie appealing because its underlying message is that one need not kowtow to nomos to be loyal to politeia. The lie counters the tendency of well-reared human beings to become captivated by good nomoi. The thralls of nomos will never acquire a truly mature human excellence even under the most salutary conditions; if given the chance, they would choose to be tyrants (619b7–d1). Glaucon and the best of the youths of the model city will see the falseness of the lie. Yet they will also see its nobility—its generosity in providing or preserving a way to the truth. Once Glaucon sees that a lie—a thing far different from the pristine truth—can serve an essential educational purpose, he can begin to overcome his feelings of contempt for appearances. He, and the city's philosophical youth, can begin to recognize the counterproductive immaturity of their own hyperian zeal for proceeding directly or immediately to the truth auto kath' hauto. Glaucon's support of the noble lie represents a growing appreciation of the importance of images and appearances—and even falsehoods—for the seeker of truth. Glaucon and other philosophical youth are likely, as a result, to take a less haughty view of the human beings who occupy the realm of appearances. This more moderate approach is prerequisite to Glaucon's own transcendence of hyperian partisanship. It is necessary, moreover, if he is ever to acquire a mature righteousness or human excellence.

As we have seen, Socrates devises his noble lie because his city requires rulers whose judgment is nonpartisan. The lie helps philosophical rulers to cultivate a capacity for synopsis no more distorted by concern for the "things aloft" than for kith and kin. But, before Socrates is willing to regard the foundation of his model city as complete, he addresses one more obstacle—perhaps the most difficult and surely the most intimate obstacle—to the beneficial practice of dialectic. This obstacle derives from the peculiar sense of isolation that we feel in our own bodies.

Socratic Communism and the Battle against Erotic Idiosyncracy

With the completion of the guardians' education in music and gymnastics and with the additional safe-guard of the noble lie, one might think that the foundation of the model city would be complete. But Socrates warns Glaucon that one must still be on guard lest the auxiliaries, who are meant to be the city's watchdogs, turn wolf and attack the flock (416a2–b4). The education that has been given to these men aims to provide them with the most important safeguard (416b1–c4). But Socrates observes that "someone intelligent would say that, in

addition to this education, one must provide them with dwellings and such other substance as will not stop them from being the best possible guardians" (416c5-d1). Socrates accordingly proposes that the guardians shall possess "no private substance whatever, unless it is entirely necessary" (416d5-6). What is necessary for guardians is different, of course, from what is necessary for farmers. Private ownership of land is for farmers an instrument of membership in the polity. The possession of such property encourages efficient agricultural cultivation and thus ensures that farmers will not cease being farmers (417a6–7). But Socrates' warriors are to be civic guardians, not farmers. While the private ownership of boots, toothbrushes, and other such necessities might fit perfectly well into the guardian's role in the civic politeia, the possession of private estates, houses, and money would only be a hindrance to the warriors' distinctive work of caring for the city as a whole and so must be proscribed. In the model city, property (that which is one's own) consists in nothing else than the equipment appropriate for performing one's part in the politeia.

Although Glaucon endorses Socrates' proposal, his brother Adeimantus ventures a criticism. We have not heard from Adeimantus since he joined Socrates in banning insubordinate poets from the city. Now it is apparent that Adeimantus has some unresolved difficulties with synopsis. Adeimantus wonders how Socrates would defend himself if someone were to charge that he had denied happiness to his guardians, the very men "whose city it is in truth," inasmuch as they are not permitted to possess great and beautiful houses or to have the opportunity of private sacrifices to the gods or because they cannot embark upon trips abroad in private or make presents to girlfriends (419a1-420a8). 14 The problem, according to Adeimantus, is that the guardians are more like foreign mercenaries than the rulers of any recognizable city. This is a telling criticism, at least as far as Adeimantus himself is concerned. For in the course of its formulation, Adeimantus reveals his own presumption that the city belongs to its rulers. He has forgotten that the truth about politeia is quite the reverse: it is not the city that belongs as private property to the guardians but the guardians who, along with the other citizens, belong as members to the city. Adeimantus needs to be reminded of his responsibilities as a member of the team of interlocutors founding the model city. For the work of answering such indictments, Socrates indicates, is not Socrates' private burden (as Adeimantus had suggested) but is rather a task shared in common with his cofounders (cf. 419a2 and 420b1). Adeimantus' fretting over the lack of private gratifications suggests that he, at least, has not yet benefited sufficiently from Socrates' propaideia.

Adeimantus' problem is that he is not attending to the model city as a whole. His concern for the role of private indulgence in the constitution of individual happiness stands in the way of his successful practice of synopsis. So Socrates, in response, reminds him that the founders' task is not to seek the exceptional happiness of any tribe within the city but rather to consider how it is that the city itself may be, as much as possible, a whole (holê, 420b5–8; holên, 421b6). It is in such a city (i.e., a whole city, holês poleôs) that the interlocutors had thought they might find justice (368e3, 420b8–9). The political institutions of the model city serve the discovery of justice by constituting the civic community as a whole. This purpose, as Socrates reminds Adeimantus, must be kept clearly in mind if one is properly to appreciate the founders' communistic proposals.

Adeimantus accepts Socrates' reminder (421c7). He agrees with Socrates that the most beautiful sign of civic improvement—the noblest limit of civic enlargement—is the achievement and maintenance of political wholeness or integrity. The interlocutors agree, moreover, that if the guardians' education and rearing ("the one great, or rather sufficient, thing") is safeguarded, the guardians themselves will be equipped to discern all that is further required for the city to reach the fullness of its growth. These further requirements are left undetailed for the time being. Yet they too must be comprehended by any synoptic view of the civic politeia. Thus Socrates makes a passing reference to these requirements, noting simply that the possession of women, matings, and procreation should all be arranged to conform as closely as possible to the proverb "friends' affairs are common" (koina ta philon, 423e5-424a2). Socrates postpones the elaboration of this proposal for good reason. Not only will the details of such a proposal stir up a hornet's nest of opposition, but its elaboration as a whole will involve the very dialectical examination (dielesthai, 449d3-450a2; dialegesthai, 454a4-d1) that he has been so carefully postponing. The three waves of book 5, which Socrates and his interlocutors will confront and eventually survive, are illustrative of the dangers that anyone engaged in a trial by dialectic necessarily faces. But Socrates must still doubt whether his interlocutors are sufficiently prepared to take the risks involved in such a dialectical examination (450d8-451b8). Adeimantus' anxiety concerning private indulgence suggests, as much as anything, that more preparation is needed. It is as a final stage in his dialectical propaideia, we may infer, that Socrates offers this muted foreshadowing of the communization of family life.

Like the other controversial political institutions that we have already considered, Socrates' connubial communism—just so far as it is developed through the completion of the foundation (ôikismenê, 427c6–d1) of the model city—contributes to the fortification of the interlocutors' capacity for synopsis. (In recognition of the distinction between this propaedeutic and the actual practice of dialectic, I reserve discussion of the barnyard analogies and the other features of connubial communism as detailed in book 5 for another occasion, when the whole matter of the reintroduction of dialectical examination may be appropriately considered.) So we must concentrate our investigation here upon the only clue concerning the

character of Socrates' connubial communism that is provided prior to the completion of the city's foundation, namely that the possession of women, matings, and procreation should be arranged to conform as closely as possible to the proverb "friends' affairs are common." We should begin by noting that Socrates introduces his endorsement of this proverb—and its surprising application to sexual mating and procreation—as a counter to the tendency toward self-indulgence and self-centeredness, which is implicit in the indictment brought by Adeimantus.

Beside the more spirited and hyperian sources of alienation or partisanship already mentioned—which seem to be at the root of Glaucon's characteristic difficulties—each of us is continuously confronted by that most tangible reminder of human separateness: his or her own body. As is clear from the differences between the truthful city and the feverish city, Adeimantus is temperamentally much more solicitous of the human body and its natural requirements than is his brother. Indeed, for the mass of human beings, the indisputable separateness of the human body and the exclusivity of individual sensory impressions suffice to demonstrate that the body is radically private. Narrowly focussing upon the indulgence of the bodily senses, human beings become even more credulous of assertions that our bodies and what we do with them are nothing but "our own affair." (Perhaps it is for this reason that Plato makes the conversation reported in the Republic occur as an alternative activity to the feast and all-night orgy taking place concurrently in the Piraeus. The interlocutors' "being moderate" both in speech and in deed necessarily precedes their consideration of the significance of the body.) Now the claim of individualistic self-indulgence is especially compelling in the most physically intimate relationship of all, the sexual mating of man and woman. Yet if mankind is even more a coupling animal than a political animal, it would seem that the opportunity for synopsis in connection with the connubial politeia presents itself ahead of all others. If one fails to achieve synopsis in this matter, it is quite unlikely that he or she will do much better in the case of civic or cosmic synopsis. So Socrates has good reason to speak in earnest when he proposes that sexual matters be arranged to accord with the proverb "friends' affairs are common". For nowhere are the problematic status of the separateness of our bodies or the obstacle this poses to the achievement of synopsis more evident than in connection with sexual matters.

There is, of course, nothing quite like sex—and not merely in terms of intensity of pleasure. For other bodily appetites yield to perfectly appropriate private satisfactions. Consider, for example, the appetite of hunger. There is no breach of civic membership, nor is there anything partisan, when a guardian ingests and thus privatizes the moderate ration of food that he eats. The exclusive ingestion and assimilation of this food is precisely what should take place if the guardian is to be a guardian. In fact, Socrates makes a point of endorsing the Homeric practice of award-

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ing the best cut of meat as a prize to the best warrior. Such an award is especially fitting, Socrates says, because the best warriors thus derive both honor and an increase in strength at the same time (468c10-d6). This remarkable combination of results is possible because the purpose served by hunger is to facilitate the strength or health of one's own body. Food, which exists to satisfy hunger, can be used for the purpose for which it exists only by being made private. In this regard, sex is rather more complicated. Of course, the pleasure obtained in the satisfaction of sexual appetite is as exclusively private as any other sensory impression. Nevertheless, the chief purpose of sexual intercourse is procreation; and the work of procreation is by no means a private or exclusively individual affair. In procreation, the intercourse of two (the Greeks called this intercourse "being-together" or synousia) is needed to generate a one. The coming into being of any single human body, whether male or female, presupposes the synousia of male and female. We may go so far as to say that it is impossible to understand the body apart from its role in a connubial partnership. For the elemental fact of sexual differentiation and complementarity is unintelligible in any other way. We shall never appreciate the body rightly if we consider it reductively, as nothing but an idiosyncratic bundle of nerves. If, instead of such reductionism, one grasps the body as a whole—and this is perhaps the primordial instance of synopsis—one cannot fail to observe that the body is naturally either male or female and thus the natural complement of a body of the opposite sex. The synousia of male and female thus constitutes a whole toward which each is erotically drawn. Yet sexual individualism treats the body as if it were nothing but an instrument of sensation. Erotic idiosyncracy thus undermines the intelligibility of the body and obscures the synopsis of the connubial partnership as a whole. The sexual differentiation of the human body-its inflection into virile male and fertile female—suggests that at its most fundamental the human body is not simply "one's own affair"; the body is a sign and at the same time the instrument of a determinate and whole-seeking erôs, rather than of a limitless and merely self-indulgent appetite. Hence, we may say that the assertion of a purely private prerogative in sexual matters is indicative of the failure synoptically to grasp the connubial significance of the sexual character of the human body. Though we do not yet know the precise details of Socrates' reformed nomos for sexual mating, we may expect that it will be intended to combat self-centeredness by encouraging more attention to the connubial significance, as distinguished from the physiological separateness, of the human body.

The assertion of individual prerogative in sexual matters obstructs the achievement of a synoptic view of the human body and so obscures the *politeia* that properly obtains in all the relations (not just the sexual ones) between man and woman. In short, erotic idiosyncracy undermines any prospect for friendship between men and women. Connubial

communism may be necessary, from Socrates' perspective, because there are not sufficient safeguards against erotic idiosyncracy in the sexual matings undertaken currently in the context of private family life. Exclusivity in the mutual espousal of man and woman may be understood as pointing toward connubial synopsis, but only if one is already so inclined. For someone differently inclined (e.g., toward somatic self-centeredness and erotic idiosyncracy), marital exclusivity alone will not counter the prejudice that sex exists for the sake of individual satisfaction. Of course, the most congenial institution for the erotic individualist is the "singles bar." In a world in which such an institution had itself become normative, the paradoxical proposal of sexual exclusivity might well encourage a reconsideration of connubial synopsis. 15 But as far as Socrates and his interlocutors are concerned, sexual exclusivity remains the norm, and the privacy of family life conforms itself all too easily to erotic idiosyncracy. Inasmuch as erotic idiosyncracy undermines the achievement of connubial synopsis, there is little doubt but that it will also corrupt and disable the soul's capacity for grasping as a whole any superordinate community of which man and woman are a part. Socrates' assault on this form of individualism thus aims to orient his interlocutors toward the wholeness of the connubial partnership and any other partnerships, such as the political community, to which the connubial partnership belongs. As long as the connubial significance of the human body is neglected, the dialectical examination of questions such as whether the just or the unjust are happier will only endanger the excellence of the human soul. In the interest of preparing for such an exercise of dialectic, however, Socrates would have us replace individualism in our understanding of sexual matters with an emphasis on sexual community. His strategy is evidently successful, at least to this extent: when Adeimantus formulates his second thoughts on the question of connubial communism and the rest of the women's drama, he speaks synoptically of Socrates' stealing a "whole eidos of argument" and expresses his view that "the whole community of women and children makes the whole difference to a politeia coming into being rightly or not" (449c2, 449d3-6).

CONCLUSION

Glaucon is in a precarious position, as we meet him in the opening of Plato's *Republic*. His native inclination is skewed toward justice but he is, nevertheless, fascinated by injustice. Perturbed by the inconclusiveness of Socrates' response to Thrasymachus' defense of the unjust life, Glaucon resolves to place his own native decency in jeopardy in order to acquire a mature human excellence. Spurning the conventionalsim of Thrasymachus, Glaucon argues that a real man—the true individual, who feels himself in no way beholden to the political community—will find injustice naturally superior to justice. This, at least

as Glaucon sees it, is the argument that must be answered if any of us is to choose justice for its own sake, rather than merely being forced by circumstances to accept justice as the price paid to avoid suffering injustice. He accordingly challenges Socrates to show the superiority of justice in a single soul, apart from all that a reputation for justice might convey.

But as Adeimantus suggests, our souls have been distorted by the influence of traditional patterns of childrearing. As a consequence of this distortion we are not in a condition to make the sovereign choice that Glaucon assumes as his prerogative. What is lacking in our souls, moreover, cannot be supplied by the mere addition of further argument. Adeimantus indicates that the soul "turned wholly to appearances" suffers from a distorted view of the question of justice and injustice; a soul thus twisted will always find that injustice is superior to justice, no matter the argument. So this psychic distortion must be corrected before the bearing of any further argument upon the choice between justice and injustice can be determined. Nor will the deformity of a soul turned wholly to appearances be remedied by turning exclusively toward "what is." Such a turn (like the one attempted by Glaucon at the opening of book 2) does not correct but merely inverts the warp of the distorted soul. What is needed, instead, is for the soul to develop an ability to comprehend as a whole both what is and what appears. Socrates expressly identifies such a whole as "the most beautiful" kind of good (358a1-3). The comprehension of this good would entail the synoptic capacity that Socrates declares to be prerequisite to the beneficial practice of dialectical argument.

In order to facilitate the development of this necessary synopsis and to forestall any further fruitless or even counterproductive dialectic concerning justice, Socrates proposes that his interlocutors join him in making a model city. Socrates' political proposals are not meant, then, as a blueprint for public policy; nor is it their principal intention to critique or to parody political idealism. Each of the city's controversial institutions contributes to the remediation of psychic distortion and fosters the soul's capacity for synopsis.

Censorship fosters an appreciation of membership. The feverish desires of the inflamed city represent the diffusiveness of the individual soul's longing in a condition anterior to the achievement of constitutional synopsis. The civic polity itself emerges only with the introduction of censorship. Glaucon, who regarded his individual self as "apart from everyone," discovers to his own surprise that he is growing more sober-minded as he affirms the principles of Socratic censorship. Thanks to this censorship, Glaucon comes to see himself as a part of the whole rather than apart from all. It bears repeating that Socrates' proposal for censorship is not a call to arms against the freedom of speech and expression found in presently existing political communities. To respond to such a call would only distract us from the point of Socrates' exercise. Socrates' discussion of censorship is not meant as a public policy proposal but as therapy for better comprehending constitutional polity as a whole.

But synopsis requires more than an appreciation of membership, for membership still allows for the possibility of partisanship. Socrates contrives his noble lie to counter partisan tendencies, "especially [among] the rulers themselves," that inordinately bias judgment either in favor of kith and kin or "the things aloft." The noble lie-a medicinal lie in words, as opposed to the hateful lie in the soul—helps the rulers to perform their task of civic recruitment in a wholesome (i.e., a whole-revealing and whole-preserving) manner. It also provides a medium essential for continuity in the procession of philosophical education. Apart from the integration of this learning experience, even well habituated youngsters will end in repudiating the very persons and customs that sustained their first experiences of education and will turn tyrant at the first opportunity. The lie is thus the linchpin that holds together the city of warriors with the beautiful city of philosopher kings. It underlies and holds the key to the integrity of Socrates' model city through the major stages of its development.

Socratic communism addresses certain tendencies toward self-indulgent individualism, which are rooted in the separateness of one's own body. The inappropriateness of private possession of lands, houses, and money by civic guardians is indicated first of all. Although such things are needed by farmers and householders in the course of performing their civic role, these possessions would only distract civic guardians from their proper jobs. But communism culminates, for Socrates, in an assault upon erotic idiosyncracy. The self-centeredness of erotic idiosyncracy is countered by Socrates' application of the proverb "friends' affairs are common." Above all, the intelligibility of the human body itself depends upon an appreciation of its connubial significance in procreation. To attain this perspective we must, of course, transcend the reductive perspective that views the body as nothing but an individual bundle of nerves. Bodily sensation, we may grant, is exclusive and hardly communicable. But the body itself, which bears unimpeachable evidence of the complimentarity of male and female, is only mistakenly regarded as a private affair. The objective of Socratic communism is to counter this mistaken tendency and to illuminate the connubial (and thus the civic and even the cosmic) significance of the body. The tendency to treat the human body as idiosyncratic renders the body unintelligible and poses an impediment to connubial, civic, and cosmic synopsis. Once again the point of Socrates' correction of the distorted view is neither to promote nor to discourage social change but rather to foster the synopsis prerequisite to the deliberate and reasoned adoption of a polity in one's soul.

The investigative strategy of Plato's Republic suggests that it is prudent, before choosing a polity for one's soul, to get some experience by establishing a

polity for something less precious—for example, a city in speech. The foundation of the model city thus serves the purpose of fostering the interlocutors' synoptic capacity as a prerequisite of their mature choice of life. Is it fitting, then, to maintain that Socrates' polity is the best city? If the function of the political community is to assist persons in achieving virtue, then it seems that it could count as such—though it never see the light of day. The existence or possibility of this city is irrelevant and can be seen to be so once one has understood what its true purpose is.

Notes

- 1. Citations in this essay to the *Platonis opera* (Plato 1900–1907) follow the customary Stephanus notation. Unless indicated otherwise, all citations are to the *Republic*. The translations are my own.
- 2. Thus I am in agreement with Strauss when he notes that in interpreting Plato "one cannot take seriously enough the law of logographic necessity. Nothing is accidental in a Platonic dialogue; everything is necessary at the place where it occurs" (1964, 60). It should be understood that "everything" includes the dramatic action as well as the arguments of the dialogue. Those who ignore dramatic evidence in their haste to dissect "Plato's" arguments labor in vain. But I also agree with Klein that "we [readers] are one of the elements of the dialogue and perhaps the most important one" (1965, 9). In this sense, the completion (i.e., the perfection) of the work intended by Plato is always up to us and so is never quite beyond the reach of chance. Consequently I would say that freedom and necessity are in perfect balance whenever learning takes place through the medium of a Platonic dialogue and the author's intention is thus fulfilled.
- 3. In Plato's *Laches*, Socrates explores the bearing of courage on the character and the pursuit of human excellence. For further discussion of this dialogue, see Dobbs 1986.
- 4. The definition of what exactly constitutes one job is, for the time being, deduced from the singularity of a given art. Later, the founders find that the two arts of music and gymnastics constitute one pair appropriately assigned to those who are to undertake the job of civic guard (411e4). It follows that technê no longer provides a sufficient indication of singularity of occupation. The founders' recognition of the integrity of this pair of arts thus represents a preliminary achievement of synopsis. Nevertheless, a certain unwitting dogmatism, which implicitly assumes that certain jobs or practices are unintegrable, has led even careful students of the Republic to maintain that Socrates' proposal for a philosopher king "rejects in its very formulation the principle of one man, one job" upon which the model city is based (see, e.g., Benardete 1989, 180). If nothing else, Socrates' appreciation of the questionableness of what constitutes one or makes for integrity (524e2-525a5) should give us pause before dismissing the integrity of the philosopher king on such simplistic grounds, as Strauss, for example, rightly perceives (1964, 101). For an interpretation that attempts to make the case for the integrity of Socrates' philosopher kings, see Dobbs 1985b.
- 5. Socrates thus notes that it is not only mimetic experts or insubordinate poets who are to be censored. Eventually, every craftsman is supervised to determine who is best able to follow the track of beauty and goodness of form; only those with this ability are permitted to remain in the city (401c4–d3). Others will be ejected, notwithstanding their specialization. Although the pastry chef, the relish maker, and the courtesan were quite welcome and appropriate in the city of luxuriance, all these specialists must now be purged. For their products and services contribute nothing to the education of the warrior youth, which is necessary if the city is to possess a

politeia. We may say that the rule one man, one job or art still prevails. But the meaning of oneness or integrity is enlarged beyond the mere consideration of specialization. Integrity of craft (and even integrity of function) is determined by harmonization with the politeia of the model city.

6. The truly epic pretentiousness of the mimetic expert is clear enough in the text before us. But for further testimonials, consider the whole of Plato's Ion and, in particular, the following speech of Niceratus from Xenophon's Symposium: "You are aware, of course, that the supremely wise Homer has made poems on nearly every human subject. So anyone of you who wishes to become a skilled householder, politician, or general—or to be like Achilles, Ajax, Nestor, or Odysseus—should attend to me because I know all these things!" (4.6). (My thanks to J. Budziszewski for calling my attention to this passage and for many helpful remarks on an earlier draft of this essay.)

- 7. The true significance of Socrates' citation of this line has eluded readers, I suspect, because Hesiod is taken to be asserting that "the half is more somehow than the whole." Such a rendering is linguistically tenable, yet it yields no sense. For there is, I maintain, no way that half is more than the whole. I suggest that in interpreting Hesiod's dictum, we instead take our bearings from the distinction between piecemeal exhaustiveness and comprehensive understanding, which I argue is crucial to Socrates' discussion of censorship. Then we may see that Hesiod's word pantos should be rendered as "all" rather than as "whole." Half is indeed "more somehow than all" in that a mere totality lacks the order already adumbrated in the recognition of half. By approaching the interpretation of this line in this way, I maintain, the reader is able both to make sense of Hesiod and to appreciate the appropriateness of Socrates' citation.
- 8. Thus I hold that the synopsis implicit in recognizing that "half is more somehow than all" does not require that one first come to know all the beings. The kind of reasoning that contemporary psychologists and philosophers of science identify as "pattern recognition" entails the possibility of cosmic synopsis apart from an exhaustive survey of all cases (see, e.g., Hanson 1965, 85–92). Hence, I cannot agree with the Speusippean interpretation of "cosmic synopsis" cited by Brann (1967, 116) and followed by many other scholars.

 9. Compare Strauss's observation that "The Republic could
- 9. Compare Strauss's observation that "The Republic could unqualifiedly abstract from erôs only if it could abstract from philosophy" (1964, 112).
- 10. Adam overlooks the significance of Socrates' exclusive reliance upon works or deeds (erga, 413c8) in conducting his tests. Hence, he considers it "a curious fact that Plato's klopê [the loss of true opinion by theft, including theft by speech] still leaves a loophole by which vicious poetry may creep in again. On the general question, Plato does well to insist on the educational value of temptation; the theory and practice of modern times recognizes it in connection with bia [force], but experience too often shows that klopê and goêteia [wizardry] mean playing with fire" (1902, 1:191–92). Of course Adam is quite right "on the general question." Experience does show that such things mean playing with fire, as Socrates clearly agrees (537e1–539d7). It is for precisely this reason, I contend, that Socrates restricts the testing of his warriors to works (erga), not words.
- 11. So it is by no means Socrates' leading concern to dupe farmers and artisans (and nonphilosophers generally) into servile obedience. Yet one finds that the vast majority of commentators on this passage (citing them by name would serve no useful purpose) vainly invoke anachronistic political sensibilities to denounce Socrates' lie as a propagandistic outrage. Strauss, Bloom, and others influenced by their interpretation, must be considered separately, however. For they at least do not assume the superiority of the modern sensibility (Bloom 1991, 93–94; Strauss 1964, 102). On the contrary, they go so far as to endorse the lie. But they do so exclusively on grounds that the need for propaganda is inescapable. So their account, too, ignores the pedagogical relevance of the lie for the rulers themselves. As Strauss sees it, political order will always depend upon deception, inas-

much as the multitude is necessarily unphilosophical and therefore unwilling to consent to be governed by the truth. Thus he writes, "It is therefore with special regard to the ruled . that Socrates introduces at this point the noble lie" (1964, 102). It is on the authority of this interpretation, we may surmise, that Bloom, in his translation of the Republic, tones down Socrates' express statement that it is first and foremost the rulers themselves who are to be persuaded. (Bloom's rendering of malista as "in the best case" [414c1] sounds hopeful rather than emphatic. Moreover, he elides the intensifying pronoun autous, which Socrates repeatedly employs [414c1, d2] to stress that it is the rulers themselves who are the principal target of the deception. Bloom's rendering of this key passage thus tends to overstate the tension between truth and the political community and between philosophy and the citizenship of the best ruler). Voegelin, by contrast, deserves credit for taking seriously Socrates' indication that it is the rulers themselves who are to be persuaded by the lie (1957, 104-8). This is not to say, however, that what Voegelin takes to be lesson of the lie is exactly what Socrates had in mind.

12. For Socrates it is this contrast between being deceived and being swindled that is fundamental. Some lies are medicinal in contributing to the soul's healing and recovery of a capacity for learning, others are poisonous in inducing a condition of soul antithetical to the experience of learning. This opposition, not the over-parsed distinction of lie and myth, is most critical from Socrates' point of view. In fact, Socrates twice refers to the noble lie expressly as a mythos (415a2, c8).

13. For a discussion of the importance of this distinction for classic natural right, see Dobbs 1994.

- 14. Adeimantus' criticism is indicative of a synoptic defi-ciency, so it should not be confused with Aristotle's contention that Socrates deprives the guardians of happiness in his own interest (*Politics* 1264b15–17). The crucial difference—a difference that elevates Aristotle's treatment of the Republic far above the critical assessment it has typically received from its commentators—is that Aristotle's critique issues from a truly synoptic appreciation of Socrates' undertaking in the Republic. For further discussion, see Dobbs 1985a, 29-34.
- 15. Compare Harry Neumann's observation: "Only in Socratic communities would happy marriage be more erotic

than adultery. . . . Thus Socrates in Plato's Republic advocates a policy which publicly forbids normal marriage and family life" (1973, 452).

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HEIDEGGER ON FREEDOM: POLITICAL NOT METAPHYSICAL

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This essay analyzes modern and postmodern concepts of freedom and contrasts them to a Heideggerian understanding. Positive, negative, and what might be called Foucaultian or Nietzschean liberty are demonstrated to bear a common trait. In such modern and postmodern formulations, freedom is consistently identified with a form of mastery. This identification of freedom with mastery, I argue, encourages ecological abuse, supports the dangerous prerogatives of statist sovereignty, and strengthens the resilience of patriarchy. The political significance of Heidegger's alternative vision is addressed.

n 1958, Isaiah Berlin delivered his now famous essay "Two Concepts of Liberty" (Berlin 1969, Chap. 3). From the hundreds of senses of the words liberty and freedom that historians of ideas have recorded, Berlin selects two for analysis: "positive" and "negative" liberty. Such timely conceptual distinctions, Berlin insists, have significant political ramifications. Attending to ideas in order critically to assess their meaning may prevent their acquiring an "unchecked momentum and an irresistible power over multitudes of men that may grow too violent to be affected by rational criticism" (p. 119). Berlin is reflecting on Heine's observation of the relationship between Rousseau's thought and Robespierre's deeds and on Heine's prediction that the romanticism of Fichte and Schelling might one day turn the Germans against the liberal culture of the West. More to the point, Berlin had the then closing Iron Curtain on his mind, and he would critically evaluate positive liberty as its ideological foundation.

With a similar concern for the social and political effects of our thinking, I propose to reexamine the concept of liberty. My worry, however, is not with past fascist threats or looming totalitarian menaces to liberalism. I am more concerned with liberalism's very success. To be specific, I am worried about the danger posed to our social and political life, our world and our earth, by the prevalent notions of freedom that underlie Western political culture. My intent here is not to add to the prodigious debate surrounding Berlin's seminal essay but to employ his rather facile schema, without the criticism it deserves, for other purposes. The analysis will not offset positive with negative liberty, but positive liberty, negative liberty, and their postmodern counterpart, with an understanding of freedom for which Martin Heidegger serves as the spokesperson.

Heidegger is seldom acknowledged for his concern with freedom. There are chiefly two reasons. First, Heidegger articulates freedom in a way that takes us beyond traditional formulations—formulations to which he remains consistently critical. As such, either his discussions are considered overly idiosyncratic and hence irrelevant to standard debates or his perspective (particularly given his understanding of technology) is held to leave little room for liberty of

any ilk. Assessing Heidegger's relation to freedom as the Western world's preeminent political value, one recent commentator would thus dismiss him out of hand, observing curtly, "You can't be a very effective spokesman for freedom when your philosophy tells you that it doesn't exist" (Crews 1992, 33). I believe, however, that freedom constitutes the core of Heidegger's thought. Indeed, Heidegger himself insists that "the question of the essence of human freedom is the fundamental question of philosophy, with even the question of Being entwined in it" (Heidegger 1976–89, 31:300). What follows is an effort to demonstrate the meaning and political relevance of this claim.

The second reason accounting for the frequent dismissal of Heidegger's contribution to our understanding of freedom is his own political biography. For many, Heidegger's work is irredeemably tarnished by his fervent support of national socialism while serving as rector of Freiburg University beginning in 1933 and by his stubborn reluctance after the war to come to terms with the meaning of this involvement. Heidegger's political biography and its relation to his philosophy has proven material for volumes of discussion and diatribe. Here I have chosen to skirt this issue-maintaining a theorist's prerogative to derive from a philosophy political sensibilities that its author has not earned and might well disown. The political implications I draw from Heidegger's understanding of freedom, to be sure, are in conflict with the more infamous aspects of his life. Yet they are congruent, I would argue, with the thrust of his philosophy as a whole. Moreover, my effort remains faithful to the basic orientation of Heidegger's own investigations. In philosophy, Heidegger observes, discovering necessarily means shaping (1985a, 38). Like all works of interpretive political philosophy, mine shapes a certain world out of something discovered in another's text. My concern, then, is not "with 'Heidegger', but with what he thinks about" (Blitz 1981, 16). I argue that in thinking about human freedom, Heidegger, after some missteps, demonstrates the need for a fundamental rearticulation of what in the West we find most essential to our historical and political being. How to voice this rearticulation of freedom within

one's own political life is, of course, neither for Heidegger nor for his biographers to decide.

FREEDOM—NEGATIVE, POSITIVE, AND POSTMODERN

Negative liberty, simply put, is freedom from constraint. It signifies the (political) space accorded the individual to pursue desires unhindered by the impositions of others. Its particulary modern formulation, stemming from the Renaissance and Reformation, is the notion of privacy—a sacrosanct area of personal freedom over which the individual has complete jurisdiction and which demands protection from all external interference, whether that of the church, state, or society. Negative liberty denotes the individual's unchallenged control over his immediate environment, enabling the exclusion of others from its trespassing. Berlin identifies negative liberty as the core of liberalism. Positive liberty, in contrast, is not a freedom from but a freedom to. It denotes a freedom to do. But positive liberty entails doing not only what one desires, unhindered by external constraints, but what one should desire, unhindered by internal constraints such as irrational drives, weaknesses of character, false consciousness, or even shortsighted judgment. Free will is only truly free, in other words, when it actualizes the individual's "objective" interests. Hence, positive liberty necessitates both the discovery of one's proper needs or desires and the capacity to see them fulfilled. If, with Rousseau, we discover our true interests to coincide with the general will, then we may find ourselves in the curious position of being forced to be positively free. If, with Marx, we find our true interests to be determined by our nature as species beings, then attaining positive liberty consists in achieving radical equality and fraternity such that each gives according to his abilities and receives according to his needs. And finally, if, with the Nazi ideologues, we hold our true interests to lie with the expansion and rise to power of the Volk, then we may experience our positive liberty only in the life and death struggle for Lebensraum. Positive liberty, in short, is freedom to be the most one can be. Attaining this goal-often a group effort-entails both the discovery of one's higher self and the extension of its mastery over one's lower (deluded) self.² Berlin therefore identifies positive liberty with self-mastery. As an isolated ideal, selfmastery would appear difficult to disparage. The problem is that the selves being mastered within the social whole are generally understood to be identical in desires, needs, and reason. Historically, Berlin notes, the quest for positive liberty has therefore led to a "prescribed form of life" that often serves as "a specious disguise for brutal tyranny" (1969, 131).

Berlin, it comes as no surprise, opts for negative liberty. He advocates for the pluralism it assumes and fosters. Positive liberty, he acknowledges, has its place, but it is a place of subordination to its negative

counterpart. The positive liberty to participate in government is consequently identified not as an end-in-itself (citizens' realizing their full political nature) but rather as the only tried and true means of protecting negative liberties. Here Berlin appears to echo Samuel Johnson's reported view that "political liberty is good only so far as it produces private liberty" (Boswell 1979, 140). Yet Berlin is not a radical libertarian. He means to argue against the conflation of meanings and values, and he deprecates the tendency to place negative liberty upon an unassailable pedestal. Freedom is freedom, and it is good. But it is not the only value we have or should have. Justice is also good, and its instantiation might at time require a diminution of (negative) liberty. To underline the point, Berlin observes that we force children to go to school. In so doing, we constrain their freedom for the purpose of elevating another value, namely the diminution of ignorance. To say that in forcing children to attend school we are not actually curtailing their freedom because their freedom is effectively increased by the growth of their knowledge (of possible choices to be made, etc.) is only dangerously to conflate the meanings of positive and negative liberty.3 One senses that in this regard Berlin would agree with Edmund Burke. "Liberty," Burke writes in his "Letter to the Sheriffs of Bristol," must be limited in order to be possessed" (Burke 1960, 211).

The concepts of negative and positive liberty, Berlin proposes, gave rise to "the great clash of ideologies" that dominated his time. In 1958, Berlin could write that the view of those who reject negative liberty and favor its positive counterpart "rules over half our world" (1969, 138, 141). Concerns are significantly different today. History has rerouted the Marxist yearnings for positive liberty and swept away their institutional perversions. Moreover, as if the momentous political events of our times were not enough, events within the intellectual community have further undermined the viability of positive liberty. Whereas Berlin feared that the rational self was endangered by collective prescriptions for its mastery, postmodernists have declared this self an outright fiction. The notion of a fixed rationality that typically grounded the search for a mastered self has become thoroughly suspect. Rationalities may exist but not a rationality that transcends culture, gender, history and power. In the postmodern world, the rational self has been deconstructed. The pursuit of positive liberty is thus set adrift without anchor. But the postmodern world has left negative liberty in equally dire straits. As Michel Foucault best demonstrates, contemporary power does not limit itself to prohibiting the fulfillment of desires: it engages in the stimulation and production of desire itself. Power, Foucault insists, should no longer be viewed solely as a juridical and repressive force, symbolized by a king who constrains the movements of his subjects. Increasingly, biopower manifests itself as an insidiously creative force, evidenced in the social and political apparatuses and technological networks that shape lives (see Thiele 1986). The postmodern worry is that both the individual and the social body have become the vehicles and chief effects of power—objects of stimulation, manipulation, and production. Power today is in the business of creating subjects, not merely constraining them.

Berlin affirms that negative liberty entails an actual political or public space within which unconstrained thought and action may occur. Freedom is not simply the ability to do as one wishes—a faulty conclusion Berlin attributes to John Stuart Mill. Berlin reasons that if freedom is a function only of desire and not of (external) empirical possibility, then any contraction of desire becomes ipso facto an increase in freedom. His worry is that tyrants could suppress us to the extent that certain desires would atrophy. Under the desire-dependent definition of negative liberty, we would then be considered free(r) because we would feel ourselves to free(r), expressing little or no inclination for abolished political spaces, opportunities, and rights. Freedom—Berlin might have paraphrased Rosa Luxemburg—is truly freedom only if it allows one actually to think, speak, or act differently. The point is well taken. Any political freedom worth its name must allow for the thinking and saying—and to a lesser extent the doing-of what contests the popular and the prescribed. But the postmodern world presents a more complex picture, which, while not obviating the perennial concern of linking liberty to opportunity, casts any straightforward relationship in doubt. Not only the repression of desire but its manipulation and hyperproduction are increasingly in evidence today. The image Berlin supplies to illustrate his point is that of the ascetic philosopher slave Epictetus, who mistakenly imagines himself free because he has learned severely to restrict his wants. If we are to hark back to first-century Rome for illustrations of contemporary threats to liberty, however, then the debauched Nero perhaps better serves our purpose. A surfeit of desire and seduction that militates against meaningful community and politics better limns the dangers of postmodern (Western) patterns of unrestrained production and mediadriven consumption. Our subjectification in a technological world—the administration of our identities as desirers, producers, and consumers—is insufficiently addressed by the vocabulary of negative lib-

How, then, are we to formulate the question of liberty in the postmodern world, supplanting or supplementing the negative liberty which allows personal control over a private domain? Michel Foucault calls for resistance to the productive forces of power that manipulate and produce our identities. Freedom today, he maintains, is to be discovered in the ongoing struggle against the techniques of subjectification. Foucault insists that freedom is less a thing to be secured, like the individual rights and opportunities of Berlin's negative liberty, than an activity to be engaged. "Liberty," Foucault insists, "is a practice," it is "what must be exercised" (1984, 245). With this in mind, Foucault preferred not to speak of an "essential freedom" to be safeguarded so much as an

"agonism" inherent in all social and political relations (1983, 222). For Rousseau (and to a certain extent for Cicero, Montesquieu, Locke, and Kant), freedom entailed subordination to a law of one's own making. Foucault adopts the Nietzschean premise that the self, not solely the law, must be of one's own making. With the self no longer given (as the gift of God, nature, or history), freedom is manifest only through its "invention." Hence, we are admonished "to create ourselves as a work of art" (1980, 10-11; 1983, 237). Foucault, however, insists that freedom of the creative self arises only in the social and political arenas wherein our identities become the spoils of battle. His genealogical efforts to destabilize and contest modern modes of subjectification are consequently aimed at giving "new impetus, as far and wide as possible, to the undefined work of freedom" (1984, 46). Effectively, Foucault politicizes Nietzsche's philosophic struggle of self-invention.

Foucault's understanding of freedom bears features of both negative and positive liberty. Foucault adopts the "exercise concept" of positive liberty, wherein freedom is found not in the absence of external hindrances (the "opportunity concept" of negative liberty) but in an activity itself.4 For Foucault, however, this activity is not tantamount to the actualization of a true or higher self. Like the negative libertarians, he resists defending the nature of freedom or its practice as a function of the individual's completed (rational, moral, or political) development. Foucaultian freedom entails the struggle to remain free from definitions of the self that prescribe its telos. Rather, freedom is found in the construction of a protean self as it evolves by way of the dynamic clash of productive power and resistance. Let us call this Foucaultian (and Nietzschean) understanding of freedom the *postmodern* concept of liberty. In contrast to the freedom from (constraints) of negative liberty and the freedom to (master a rationally or collectively defined self) of positive liberty, postmodern liberty might be thought of as a freedom in. Primarily, this freedom is actualized in the struggle of self-creation. As a student of Nietzsche's tragic school of thought, Foucault celebrates this freedom in the seemingly futile and endless attempt to create a self that necessarily remains forever enmeshed in webs of power. Hence, Foucault calls for a "hyper- and pessimistic" activism" (1983,232). Postmodern liberty, by this count, is found in the heroic endeavor to create a self as a work of art, or it is not found at all.

Commenting on Berlin's thesis, Alan Ryan notes, "The simplest characterization of positive libertarians is that they identify freedom with control" (1979, 5). Ryan's remark is apt, for positive liberty refers to (one's higher self's) control of one's desires, needs, and (ultimately) of one's world. What Ryan fails to observe, however, is that negative libertarians are also animated by a pursuit of control. For negative liberty, we recall, identifies the individual's control of his immediate environment, of his private life and space. The key difference is that for negative libertarians, sovereignty is limited to a putatively personal

realm of objects and relations. While positive libertarians advocate mastery of a transcendent self through collectively mediated, self-given law, negative libertarians advocate mastery of an empirical self over a private domain. According to Foucault and postmodern thinkers in general, the transcendent self is too mythical and the empirical self too malleable for positive and negative concepts of liberty to cut much ice—hence the need for self-invention, a creative resistance to the productive power that envelopes us. But herein, I would argue, postmodern liberty displays its greatest affinity to positive and negative liberty by way of its own, rather subtle pursuit of control.

The postmodern liberation from teleological ends is achieved only by way of the subject's reinvestment in an ideal of artistic mastery. The mastery of the ungrounded but creative self over its own constitution now becomes the Sisyphean task. This move is perhaps best understood as the postmodern radicalization of the self-mastery sought by the Stoic philosophers, whom Berlin correctly identifies as epitomizing positive liberty and whom Foucault chooses to focus upon in his last works. Stoic self-mastery entails the disciplined sculpting of the self into an impervious image of natural law. Postmodern libertarians dismiss natural law (along with gods and transcendent norms). But the nucleus of the Stoic task remains intact. The postmodern attempt to mold the self into a work of art posits the self as a malleable object of production. The task, Foucault states, is to resist the hegemonic "techniques and practices" of power that have constituted the self in the West in order to chart the conditions of an alternative transformation (1993, 224). This autoconstitution of the self is to be achieved by means of the "stylized repetition of acts" (Butler 1990, 140). Fire must be fought with fire. We must become technicians of our own being-dandies of a sort-using our life to fashion ourselves into admirable aesthetic works. Admittedly, Foucault denies that this aesthetic self can gain complete sovereignty within the webs of contemporary biopower. Contingency is everywhere in evidence. Citing Nietzsche, Foucault acknowledges that the individual harbors "not an immortal soul but many mortal ones" and that these are "unable to be mastered by the powers of synthesis" (1984, 94). But, as with Nietzsche, the tragicoheroic effort is made nonetheless. Foucault would have us gain our freedom by moving beyond the "subjected sovereignties" of humanism-beyond "the subject as a pseudosovereign"-to a self whose true sovereignty is marked by the absence of inhibitions in the effort to define its identity (1977, 221-22). In the face of the ubiquitous threat of normalization, the postmodern retaliates with willful self-creation. Will-topower takes on the persona of the master artist turned upon himself even if, in postmodern times, he is a Picasso, rather than a Rembrandt.

Like William Connolly, I am arguing that "mastery is the route to freedom" for both "individualists" (negative libertarians) and "collectivists" (positive

libertarians) (1991, 29). I differ from Connolly, however, in proposing a residual pursuit of mastery, albeit aestheticized, among the Nietzschean and Foucaultian theorists of freedom.⁵ Foucault, like most postmoderns, acknowledges his debt to Heidegger, who is claimed as the "essential philosopher" determining his "entire philosophical development." At the same time, Foucault recognizes that Nietzsche "outweighed" Heidegger in influence (1985, 8-9). My argument is that Foucault's understanding of freedom-like his work in general-evidences the burden of Nietzsche (see Thiele 1990, 1991). Despite the differences between positive liberty, negative liberty, and postmodern liberty (and there are many important differences indeed), the identification of freedom with mastery remains central to all. Moreover, whether the mastery in question is of the higher self over the lower self and its desires and needs (as with positive liberty); or the mastery of the empirically demonstrable self over its private domain (as with negative liberty); or the mastery of the aestheticagonistic self over its contested, protean constitution (as with postmodern liberty)—each form of liberty tends to loose its quest for mastery upon the world.

FREEDOM—NOT METAPHYSICAL

During his politically active career as rector of Freiburg University under the Nazi regime, Heidegger adopted a positive concept of liberty. In line with Nazi ideology, which he fervently propagated during his brief tenure, Heidegger situates the self within a Volk that circumscribes and defines its identity. Thus, Heidegger bore out Heine's prediction of German romanticism's turning the nation against liberal culture. Addressing the question of academic freedom after the imposition of the Nazi Gleichschaltung, which sought to organize students and faculty according to the Führerprinzip in an effort to integrate the university into the party and state, Rector Heidegger applauds the demise of negative liberty and celebrates its positive counterpart: "This will is a true will in that the German student body, through the new Student Law, places itself under the law of its own essence and in this way for the first time determines that essence. To give the law to oneself is the highest freedom. The much celebrated 'academic freedom' is being banished from the German university; for this freedom was not genuine, since it was only negative. It meant primarily freedom from concern, arbitrariness of intentions and inclinations, lack of restraint in what was done and left undone. The concept of the freedom of the German student is now brought back to its truth" (1985b, 475-76). Here positive freedom is found in subservience to a law of one's own making. But it is not the self as an individual who makes the law but the self as a member of an organic people. One wills the general will as it grows out of a folk and is given voice through the party and the Führer.

After his rectorship, Heidegger would scale back

and eventually abandon his advocacy of a (folkish) positive liberty without, however, moving in the direction of negative liberty. He rejects both the latter notion of liberty as "unfettered arbitrariness" and the former notion of liberty as "the constraint of mere laws," including self-given laws (1977b, 25). Heidegger develops a new concept of freedom that effectively extends and reworks his prerectorial approach. By the early forties, he would write: "Freedom is not what common sense is content to let pass under that name: the random ability to do as we please, to go this way or that in our choice. Freedom is not licence in what we do or do not do. Nor, on the other hand, is freedom a mere readiness to do something requisite and necessary and thus in a sense 'actual'. Over and above all this ('negative' and 'positive' freedom) freedom is a participation in the revealment of what-is-as-such" (1949, 307). In important respects, this understanding of freedom resembles Nietzsche's and Foucault's. Heidegger now posits freedom as an activity, event, or happening. Importantly, however, he rejects the postmodern notion of liberty as the mastery of a creative, artistic self. Rather, freedom is proposed as a disclosive "letting-be." I shall argue that Heidegger's development of this disclosive freedom presents us with more than just another philosophical profile of a concept already overburdened with meanings. Heidegger's mature understanding of freedom is radically distinct from its modern, metaphysical forerunners and their postmodern derivations; for it offers dignity in a freedom that celebrates guardianship rather than mastery.

Let us begin with a rehearsal of the key features of Heidegger's understanding of human being (Dasein). In distinction to all other beings, human being is interrogatively occupied with its own existence. Existing (as an ontic or factual category) is something humans share with all beings. Only human being, however, is ontologically disposed to its life, orienting itself in questioning to its own being. In questioning the Being of its own being, human being cannot help but question that which serves as the ground for all being, for beings as a whole. In short, Heidegger understands human being as that for which Being itself, the Being of beings (das Sein des Seienden), comes into question. He writes in Being and Time: "Dasein is an entity which does not just occur among other entities. Rather it is ontically distinguished by the fact that, in its very Being, that Being is an issue for it . . . Understanding of Being is itself a definite characteristic of Dasein's Being. Dasein is ontically distinctive in that it is ontological" (1962, 32). The distinctiveness of human being is not self-consciousness but consciousness of a self that questions its own ground, and ipso facto questions the ground of being in general.

Metaphysics, beginning with Plato's theory of the forms, projects Being as an enduring and unchanging thing. As such it may be represented, by an idea, in our minds. Rejecting this metaphysical orientation, Heidegger proposes Being as nonrepresentable. It is

tentatively defined as unconcealment or presencing. We never actually perceive unconcealment or presencing. What is directly available to human being are beings, that which is present-at-hand and ready-tohand. The actual presencing of that which is present remains hidden and is made available only by ontological questioning. The essence of human being, then, is the provision of a place for Being to become manifest as the questionable and hidden ground of all that is. Heidegger adds that the unconcealedness of beings is a "happening" (1971, 54). Human being is the site of this happening. Dasein, we remember, literally means "there-being." Human being is the there where the happening of the disclosure of Being (as nothingness) occurs. In this sense, human being may be posited (to employ Heidegger's locution) as the "abode" of Being (1981, 244). Perhaps better, human being is an abode in preparation for Being, an "openness." In human openness, Being becomes manifest-not as some newly discovered thing that lies before us (as a object) or within us (as an idea or form) but as the mystery of its own hiddenness within beings. Because Being reveals itself only through human being, Heidegger dubs human being the persona, or "mask," of Being (1968, 62). Only through its mask is Being's presencing discerned. Hence, within the openness of human being, Being shows itself as masked. Outside the openness of human being, Being does not come to presence (as masked) at all.

In distinction to all metaphysical theorizing, Heidegger insists that Being is not something primarily of substance or form. Being is not a thing at all, but the no-thing-"the pure 'Other' than what-is"-that grounds all things (Heidegger 1949, 360). Even if we could rip away the mask of Being, therefore, no discernible features would be revealed lying beneath; for Being is nothing extant. Yet it is precisely the disclosure of Being's no-thingness or hiddenness that human being assumes as its essential task. Heidegger explains by way of a visual metaphor: "Everyday opinion sees in the shadow only the lack of light, if not light's complete denial. In truth, however, the shadow is a manifest, though impenetrable, testimony to the concealed emitting of light. In keeping with this concept of shadow, we experience the incalculable as that which, withdrawn from representation, is nevertheless manifest in whatever is, pointing to Being, which remains concealed" (1977b, 154). As I shall argue, freedom is also experienced as that which, though nonrepresentable, is nonetheless manifest in our capacity to disclose the hidden Being of beings.

Shadows offer the necessary contrast and background for that which is lit. What remains in concealment allows the unconcealed its distinctiveness. It follows that the illuminated opening of human being is largely defined by its shadows. Heidegger understands the perimeter marking the boundaries of our disclosive capacities to be intrinsic to human being and human freedom. Dissociating his from a common understanding of freedom as unrestricted movement, he writes: "It is misguided to think one understands freedom most purely in its essence if one isolates it as a free-floating arbitrariness. Moreover, the task is precisely the reverse, to conceive freedom in its finitude and to see that, by proving boundedness, one has neither impaired freedom nor curtailed its essence" (Heidegger 1984, 196). Every act of freedom is a foreclosing of alternatives and possibilities. Freedom, therefore, is not an unbounded and ungrounded power to do, move, or create. Rather, freedom is freedom to reveal what is. But what is, most fundamentally, comes to presence only insofar as it remains illuminated in contrast to what remains hidden. In other words, only the no-thing that is Being allows human being the disclosure of beings or things. In this tensioned disclosure, freedom pertains: "Without the original manifest character of Nothing there is no self-hood and no freedom. . . . Nothing is that which makes the revelation of what-is as such possible for our human existence" (Hiedegger 1949, 339-40). Human being displays its freedom to the extent that it remains open to the inexhaustible mystery of Being in its bounded disclosure of beings. Heidegger writes: "Freedom is the condition of possibility of the disclosure of the Being of beings, of the understanding of Being." (Heidegger 1976–89, 31:303).

Heidegger, particularly in his early writings, referred to the openness of Dasein with the term resoluteness (Entschlossenheit, lit. "unclosedness"). Heidegger's commentators have often taken his early concern with resoluteness to imply a hard-willed, decisionistic orientation—a Nietzschean or existentialist enamoredness with the power to fashion one's world, where, as Sartre observes, one is "condemned to be free" owing to the burden of total responsibility for the self that one shoulders. Yet the notion of resoluteness, Heidegger at least retrospectively affirms, does not imply Promethean choices. Indeed, it marks the renouncing of such willfulness: "The resoluteness intended in Being and Time is not the deliberate action of a subject, but the opening up of human being, out of its captivity in that which is, to the openness of Being" (Heidegger 1971, 67). Hence, Heidegger maintains that "to question is to will to know. He who wills, he who puts his whole existence into a will, is resolved. . . . But the essence of resolve lies in the opening, the coming-out-of-cover of human being-there into the clearing of being . . . its relation to being is one of letting-be" (1987, 20-21). The name Heidegger gives to the resolute willing of openness to Being is Gelassenheit, a term borrowed from Meister Eckhardt that literally means "lettingbe" or "releasement." Human freedom, for Heidegger, particularly after his "turning" of the mid-1930s, is fundamentally and foremost a letting-be.

Openess to Being is effectively an opening of the self to the fundamental interrogation of (its) being. This resolute openness manifests human freedom, for to be occupied with the question of one's own being means "to understand oneself in one's own factual freedom" (Heidegger 1982, 276). This is not to say, however, that human freedom is equivalent to

self-absorption. As Heidegger makes clear, we are speaking of the concern for oneself as a human being, that is, as a being-in-the-world-with-others: "To be free is to understand oneself from out of one's own capacity-to-be; but 'oneself' and 'one's own' are not understood individually or egoistically. . . . They are understood in the basic possibilities of transcending Dasein, in the capacity-to-be-with with others, in the capacity-to-be by extant things" (1984, 214). In making its own being an issue, human being opens itself to the question of beings as a whole, which entails opening itself interrogatively to a populated world. Now fundamentally to interrogate this world demands a forgoing of pervasive efforts at its mastery; for fundamental questioning ceases the moment the presumptions and prejudices that necessarily precede such attempts at domination or control arise. To be sure, the disclosure of Being ensues from all human activities and experiences, including human efforts at attaining mastery. But herein Being, as well as the nature of human freedom, becomes obscured. It is revealed only as forgotten. To reveal something as forgotten is to reveal something as no longer in question, as no longer fundamentally questionable. That we are, as Heidegger states, called upon to be the shepherds of Being indicates that we find our freedom not in a forgetful mastery but in a mindful, interrogative caretaking, in concernfully and questioningly letting the Being of beings be. Freedom is therefore most primordially manifest in our care-full being-in-the-world-with-others.

Disclosive freedom, Heidegger insists, is not merely one value among others. Nor either, is freedom a *meta*value, the "value of all values" that allows one to choose between and secure other values (Cranston, 1967, 42). "Freedom," Heidegger writes,

is not a particular thing among others, not something lined up as part of a row, but rather it prescribes and permeates the totality of beings as a whole. If we are to investigate freedom as the ground of possibility of human being, then its essence is more primordial than man. Man is only a guardian of freedom, . . . human freedom signifies now no longer: freedom as a property of man, but the reverse: man as a possibility of freedom. Human freedom is the freedom which invades and sustains man, thereby rendering man possible. (1976–89, 31:134–135; see also 1985a, 9)

Traditional Western thinking posits freedom as the autonomous subject's most valued asset, as its capacity to define and control what it confronts. Heidegger, contrariwise, understands freedom as that which exposes human being to the undefinable and unmasterable, to Being. But what is beyond one's power to master or define is also beyond one's power to calculate and assess. Freedom, therefore, is that unique capacity of human being that allows a reaching beyond calculation and valuation.

Heidegger observes that

through the characterization of something as "a value" what is so valued is robbed of its worth. That is to say, by the assessment of something as a value what is valued is admitted only as an object of man's estimation. But what

a thing is in its Being is not exhausted by its being an object. . . . Every valuing, even where it values positively, is a subjectivizing. It does not let beings: be. Rather, valuing lets beings: be valid—solely as the objects of its doing . . . When one proclaims "God" the altogether "highest value," this is a degradation of God's essence. Here, as elsewhere, thinking in values is the greatest blasphemy imaginable against Being. (1976, 228)

Everything Heidegger says here of God or Being might also be said of freedom. Once freedom becomes a value—even the highest value—it ceases to identify that which enables us to partake of the mystery of Being. Subjecting freedom to metaphysical or representational thought-allowing its designation as a value—is an effective assertion of the self's capacity to evaluate and control its relation to Being conceptually if not physically. According to Heidegger, however, "freedom's incomprehensibility consists in the fact that it resists com-prehension . . . because freedom transposes us into the occurrence of Being, not in the mere representation of it' (1985a, 162). Freedom is the gift that allows human being to glance beyond the self-beyond being and its mastery in thought, word, or deed. For Heidegger-and in stark contrast to the Western metaphysical tradition—freedom is not a value, but beyond valuation; freedom is evidenced not in decisionistic willfulness but through careful nonwilling; freedom is not an unbounded power to do but a discovery and disclosing of one's place within bounds.

FREEDOM—POLITICAL

There is no direct, unavoidable and obvious translation of Heidegger's ontological perspective on freedom into a specific political practise. At the same time, our self-understandings as individuals and members of collectivities have important political ramifications. In attempting to answer the question, What shall be done? the discovering and maintaining of a sense of self becomes an essential part of any doing. Our actions in the world stem from, feed into, and flow through our identities. If, then, we accept with Heidegger that "the highest dignity of [man's] essence . . . lies in keeping watch over the unconcealment-and with it, from the first, the concealment-of all coming to presence on this earth" (1977b, 32), it follows that our daily comportment would reflect a certain worldly solicitude. Were modern humanity to discover its dignity in freedom understood as an interrogative disclosure rather than a willful mastery, then significant changes in political culture would be foreseen.

Humanity is now threatened by the ecological limits of a world that it has unceasingly sought to master. I am suggesting that the dangerous self-confidence expressed in humanity's technological exploitation of itself and its world is at least partly explained by the fact that human freedom has been consistently equated with the power to control. Freedom continues to be the Western world's most

cherished value. Any change on that score seems unlikely. The political relevance of Heidegger's philosophy is that it allows us to maintain our love of freedom while reining in its more pernicious effects. My argument is not that we must reject out of hand hard-won and still insufficiently propagated positive and negative liberties nor that we must completely shun the aestheticized liberty of postmodern times. But we do need to recognize the significance of disclosive freedom. The task, in other words, is not to bury the past and its achievements but to recognize their dangers while preparing to receive the future. And our future as a species appears doubtful indeed if human dignity can be experienced in nothing but mastery.

Still, the Heideggerian alternative may appear all too passive for many. For does not disclosive freedom reduce us to impotent observers of fate? Is it not simply a recipe for lassitude at the end of humanity's turbulent history of ingenuity, creativity, and destruction? If so, then such freedom would be (as the ballad says) "just another word for nothing left to lose" and nothing to be done. Saint Augustine declared it a great liberty to be able not to sin and a greater liberty to be unable to sin. Heidegger believed that there is indeed much freedom in being able not to will. But he insists that there is no freedom gained in being unable to will. To escape willful mastery, a resolute will is still necessary. Great effort is required (to prepare the self) to let beings be. Just as freedom in resoluteness is not arbitrary willfulness, so freedom in letting-be is not a doing nothing. Disclosive freedom is always the freedom resolutely to will openness to Being. And this openness does not preclude but rather invites human activity and intellect. In turn, letting-be is not tantamount to abandonment of the world or isolation of the self. Quite the opposite: it entails the formation of dynamic worldly relationships, relationships all the more dynamic because they are no longer constrained by the limitations of a subject/object dichotomy.

Heidegger writes: "the freedom to reveal something overt lets whatever 'is' at the moment be what it is. Freedom reveals itself as the 'letting-be' of what-is. The phrase we are now using, namely the 'letting-be' of what-is, does not, however, refer to indifference and neglect, but to the very opposite of them. To let something be is in fact to have something to do with it. . . . To let what-is be what it is means participating in something overt and its overtness" (1949, 305–6). "To free," Heidegger observes elsewhere, "really means to spare. The sparing itself consists not only in the fact that we do not harm the one whom we spare. Real sparing is something positive and takes place when we leave something beforehand in its own nature, when we return it specifically to its being, when we 'free' it in the real sense of the word into a preserve of peace" (1971, 149). The practice of freedom, then, is not the passivity of the fatalist. Indeed, Heidegger distinguishes between fate (Schicksal) and destiny (Geschick). Fate compels us to the "inevitableness of an unalterable course"; but nothing in this world, not even the dominance of modern technology, is a fate for human being (Heidegger 1977b, 25). Rather, the historical disclosure of Being, including its current form as technological enframing (Gestell), is a destining. Destiny does not compel, it calls. Human freedom is evidenced in the listening and responding to the destining of Being, which calls us actively to participate in revealing. There is no question of blind obedience, nor is resignation our appointed lot. And the reason, Heidegger states explicitly, is that we are free. The experience of beings in their Being, which for Heidegger delimits the realm of human freedom, should frame our disposition neither pessimistically nor optimistically. Most fundamentally, it yields a tragic experience, that is to say, an experience of resolute action tempered by the wise recognition of limits (Heidegger 1975, 44).

The problem, then, is not the diminution of our creative forces as a species. The problem is that historically these forces have far too often been exercised in the struggle for mastery and domination. This, I believe, is the proper response to critics who interpret Heidegger to promote a sort of political quietism. The typical question is, "How can we think of democracy without imputing to man the minimal will and mastery that Heidegger denies him because will and mastery in some sense already contain the seeds of the world of technology conceived of as the "will to will?" (Ferry and Renaut 1990, 17). The answer is straightforward: Heidegger does not deny human being a "minimal" will and mastery, but he does seek to constrain its unbridled hegemony in the modern world.

The explicitly political nature of Heidegger's understanding of freedom (to repeat) is not unequivocal. One might say that it is political by default, in that its antimetaphysical nature leaves it no better characterization. Alternatively, its political import might best be demonstrated by its capacity meaningfully (though not univocally) to address current political concerns, such as the need for environmental care and technological restraint, the dangerous prerogatives of national sovereignty, the distinctions between public and private concerns, and the development of a nonpatriarchal society. I shall attempt to illustrate my point by critically examining some of the support Heidegger's notion of disclosive freedom finds—and gives—to more explicitly political theorizing.

Disclosive Freedom and Ecological Politics

Freedom in the Western tradition has been identified since Adam with dominion over the earth, and there is little in positive and negative liberty (or its postmodern counterpart) that militates against this. Historically, positive libertarians have celebrated the domination of nature as a testimony to the prerogatives of human rationality. Like the domination of the lower self (or of "inferior" peoples and classes), the domination of the earth often serves positive libertarians as an attestation of human freedom. Negative

libertarians, in turn, have fostered such domination in no less degree owing to their preoccupation with the exclusive personal rights of property. Nature, incapable of bearing rights of its own, relinquishes all claim to protection, respect, or care to the extent it is not accorded sanctuary under the aegis of private property. But private property-understood in a Lockean sense as the extension of the self('s labor)—is to be disposed of as its owner sees fit, unconstrained by anything save perhaps the obligation of efficient exploitation. (Thus Locke would justify the expropiation of native American lands without recompense owing to the natives' inadequate exploitation of them. According to Locke, leaving lands unexploited was equivalent to wasting them. And in wasting their lands, the natives forfeited all rights of ownership and defense [Locke 1960, 336-7; see also Glausser 1990].) Postmodern liberty has been around too short a time to have earned any historical indictment on ecological grounds. Yet my sense is that postmodern liberty, which valorizes the powers of artistic creation and heroic struggle, is more likely to depreciate than celebrate qualities of guardianship. The justifiable postmodern concern with the panoptic social administration of selves has a tendency to foster a radically individualistic (even narcissistic) reaction. With the primary postmodern worry being the dangerous encroachments of "pastoral power," any linking of freedom to stewardship would prove problematic (Foucault 1979, 1981). In short, without an understanding of liberty divorced from the will to mastery, the postmodern struggle against subjectification is as unlikely as negative or positive liberty to result in a worldly caring.

Heidegger's philosophy of freedom, on the other hand, buttresses ecological concerns at large. Michael Zimmerman writes that

Heidegger's critique of anthropocentric humanism, his call for humanity to learn to "let things be," his notion that humanity is involved in a "play" or "dance" with earth, sky, and gods, his meditation of the possibility of an authentic mode of "dwelling" on the earth, his complaint that industrial technology is laying waste to the earth, his emphasis on the importance of local place and "homeland," his claim that humanity should guard and preserve things, instead of dominating them—all these aspects of Heidegger's thought help to support the claim that he is a major deep ecological theorist. (1990, 242–43)⁸

There is much to what Zimmerman says here, and one might add to his list Heidegger's understanding of *Dasein* as care and his definition of human being as the "shepherd of Being." However, one must be wary of straightforward translations of Heideggerian philosophy into ecological practice. For human being, Heidegger insists, is not the shepherd of beings (i.e., the guardian of natural life) but the shepherd of Being. Such guardianship, Heidegger explicitly states, "is not fixated upon something existent" (1971, 184). In turn, while Heidegger does suggest that truth (*Wahrheit*) is to be experienced as preservation and that "preservation belongs to the herds-

man," he is also quick to point out that this understanding has "little to do with bucolic idylls and Nature mysticism"; for being a herdsman is only possible if one "continues to hold the place of nothingness." Quite out of keeping with romanticized visions of a seamless pastoral life, the shepherding of Being places one anxiously on the abyss of groundlessness. In short, Heidegger's is a ontological, not a naturalistic, project. A questioning relation to Being may indeed erupt from a solicitous wonder at the vast diversity and beauty of a natural ecosystem. But it may also result from gazing at stars, studying quantum physics, or raising a child in the inner city.

Heidegger, as Zimmerman notes, supports a nonanthropocentric approach to the earth and world. This is absolutely true and has obvious ecological merit. But Heidegger does not suggest the replacement of anthropocentrism with biocentrism—a belief, held by deep ecologists, that relegates the human species the same status as all other organisms.9 Despite his fervent attack on subjectivism and humanism, Heidegger firmly maintains human exceptionalism on account of the human capacity for freedom: "The question of Being and its variations and possibilities is at heart the correctly understood question of man. Compared with the duration of cosmic galaxies, human existence and its history is certainly quite fleeting, only a 'moment'. But this transiency is nevertheless the highest mode of Being when it becomes an existing out of and towards freedom" (1984, 18). Zimmerman hedges his praise of Heidegger's ecological credentials at this point, citing this passage as indication that Heidegger "overestimated our importance" compared to that of the rest of nature (1983, 121). But Zimmerman overlooks Heidegger's statement a few paragraphs earlier that such fundamental questioning is "far removed from any noisy self-importance concerning the life of one's own soul or that of others" (1984, 16-17). As a being that experiences freedom through its participation in the disclosure of all that is, human being does gain special status in Heidegger's universe. For the disclosive capacities of human beings are, so far as we know, unique: "It is man, open toward Being, who alone lets Being arrive as presence" (Heidegger 1969, 31-32; see also 1962, 28, 35). But Heidegger was explicit that the ontological priority and worthiness of human being in no way determines that the natural world—or the material world at large—exists (solely) for our benefit. 10 Indeed, disclosive freedom flourishes only in the absence of such a possessive, egoistic, and utilitarian assumption. Human being is the highest being only to the extent that human being gains release from all self-aggrandizing subjectivism. If there is a claim of greatness to our being, then it arises not primarily from our mastery of the earth but from the unique human capacity to dwell and disclose in a way that preserves.

Along these lines, one might say that Heidegger supports what today would be called a *bioregional* approach to environmental care. He proposes that intimate ties to soil and locality serve as the antidote

for the large-scale centralized manipulation and extortion of nature. Heidegger envisions living and working with, rather than against, natural processes. Hence he contrasts high-technology agribusiness to the work of the peasant, which "does not challenge the soil of the field. In the sowing of the grain [he] places the seed in the keeping of the forces of growth and watches over its increase" (Heidegger 1977b, 15). Unfortunately, Heidegger tends to romanticize the peasantry. He ignores the fact that small-scale farming may be every bit as intent on the unsustainable exploitation of land as large-scale operations. If the peasant proves less ecologically destructive, it may be owing only to the relatively small size of his enterprise. Indeed, to the extent that poverty forces peasants to cultivate smaller and smaller plots of land more intensively, environmental devastation looms. In any case, neither peasants nor business people are born with specifically ecological dispositions. Care for the land—and for people—must be fostered culturally. Heidegger is right in thinking that this care is fostered much better within small farming communities than within large-scale agribusiness bent on ungoverned technological growth. Still, we had best eschew the bucolic romanticism that Heidegger found irresistible; for the rootedness in soil that Heidegger longed for has a seamy political side. In its thrall one might, like Heidegger, succumb to a dangerous infatuation with the power of blood and soil that neatly aligns itself with fascism. In turn, agrarian provincialism may militate against environmental oversight on a global scale. Yet many of the most pressing environmental threats today remain impervious to anything but a globally regulated redress. In any case, the global management of certain ecological concerns does not necessarily disrupt our caring for community or bioregion. Indeed, the environmental slogan Think Globally, Act Locally is based on the general compatibility—even synergy—of global and local concerns. There are no eternal demarcations of one's place of dwelling. Rootedeness, for some, will pertain to a valley or watershed; for some, to a city or nation; and for some, to the earth itself. Heidegger's own preferences for Swabian peasant culture need not constrict our own habitat.

In concluding our critical exploration of Heidegger's ecological credentials, we must acknowledge that our relation to the earth will always entail preservative as well as exploitative and destructive measures. At a minimum, we learn from Heidegger that a balance must be reestablished—a balance most obviously and most dangerously lost in the modern world. Ecological concerns have erupted in contemporary times largely as a result of the increasingly obvious limits to human growth. In effect, Heidegger offers us a philosophy of limits. He describes our freedom as dependent upon rather than curtailed by our worldly boundaries and horizons. With this in mind, disclosive freedom may well provide the necessary corrective to the frenzied technological activity that threatens the earth with its unbounded scope.

Sovereignty and the Politics of Public and Private

In one of her most difficult essays, "What Is Freedom?" Hannah Arendt takes issue with traditional Western notions of liberty (1968, chap. 4). For Arendt, the nature of freedom has been obscured owing to prevailing philosophical concepts structuring the political realm. These concepts identify freedom as a function of (the mastery of) will. In contrast, Arendt observes the freedom is far from being the (will)power to achieve or control one's desires. She indicates (in all probability to the reader's confusion) that action, to be truly free, "must be free from motive on one side [and] from its intended goal as a predictable effect on the other" (p. 151). In direct opposition to the common notion of freedom as the opportunity to fulfill one's wants, Arendt strips from freedom both the final attainment of a goal and the initial motivations for its pursuit. The reason is that freedom, for Arendt, is less an individual achievement than a public event. Freedom is manifest in the open spaces of the political realm where the actions of citizens intersect. But action and interaction (as opposed to behavior) are inherently unstable and unpredictable. What humans discover when they act in concert is that freedom is manifest in the very novelty of the results. Far from being controllable and predictable, action, Arendt states, is closer to the nature of a miracle. In action, by way of freedom, the new and the unforeseen occurs.

The pursuit of sovereignty, which orients most Western political thinking about the individual and the state, is identified with the effort to achieve control over the outcome of action. Far from being the prerequisite for freedom, Arendt finds that sovereignty signals its demise:

Under human conditions, which are determined by the fact that not man but men live on the earth, freedom and sovereignty are so little identical that they cannot even exist simultaneously. Where men wish to be sovereign, as individuals or as organized groups, they must submit to the oppression of the will, be this the individual will with which I force myself, or the "general will" of an organized group. If men wish to be free, it is precisely sovereignty they must renounce. (1968, 165)

Arendt's discussion of freedom is nothing if not shocking for the Western mind. I believe it is most fruitfully viewed as a political reconstruction of Heidegger's notion of disclosive freedom. Heidegger, like Arendt, indicates that freedom evaporates once it is possessed by a sovereign power. Indeed, freedom is not freedom until it possesses us. Like Arendt's conception, disclosive freedom is inherently antipodal to sovereignty and inherently unpredictable. It is revealing of the world and of the new.

Perhaps the aspect of Arendt's political thought most criticized is her distinction between the public and the private realms. We recall that for Arendt the public realm is the birthplace of freedom. Only in the political space devoted not to the necessities of existence but rather to the discovery and preservation of freedom is politics to be found and safeguarded. Freedom therefore lies in stark and unbridgeable contrast to the continuing and unavoidable processes that sustain life in the darkness of privacy. Arendt claims that the divorce between freedom and necessity—the public and the private—is irreconcilable and complete. Anything that issues from life's needs cannot be considered the stuff of politics. The provision of services and goods, the maintenance of households and the exercise of administration, therefore pertains only to the realm of necessity. The public speaking of great words and doing of great deeds maintains a monopoly on freedom. Arendt has presented us with a stunning vision of politics. The wondrous novelty of birth she celebrates is revived in the heroic human capacity for political beginnings. The consequence of her position, however, is that everything that falls between the novelty of birth and the sporadic explosions of freedom evidenced in great speeches and deeds (i.e. the vast bulk of human life) is relegated to the unredeemable realm of necessity.

In the 1844 Manuscripts, Marx states that man separates himself from the animals by finding his freedom in production beyond physical need (1978, 76). Arendt, despite her virulent attack on Marx's designation of man as an animal laborans, effectively extends this enlightenment position. But freedom is now found not in the realm of surplus production but in what might be called the realm of surplus speech and action. Politics and freedom, for Arendt, are manifest only in that human capacity for novelty and creation of word and deed ungrounded in and distinct from the demands of everyday life. Maintenance and preservation, through labor, administration, and general caretaking, is the sorry if necessary task of the unfree. Arendt, like Foucault, celebrates contingency and dismisses sovereignty in the exercise of freedom. And Arendt, like Foucault, insists on the heroic nature of political life. Her standards of heroism, however, come directly from the Greeks rather than via Nietzsche. Consequently, Arendt's criteria for freedom, in contrast to Foucault's, shift from selfcreative to world-creative engagements. Nonetheless, her dismissal of the possibility of a freedom exercised in caretaking is equally unrelenting.

We observe that Heidegger's notion of disclosive freedom philosophically buttresses Arendt's political theorizing. Perhaps more importantly, his understanding may also remedy the shortcomings of Arendt's analysis. Arendt wishes to claim that the divorce between freedom and necessity—the public and the private—is irreconcilable and complete. Anything that speaks to the demanding necessities of life does not partake of freedom and ipso facto cannot be considered the stuff of politics. But as Heidegger demonstrates, freedom is evidenced whenever and wherever we open ourselves to the world of beings. Political freedom, then, is evidenced whenever speech and action disclose the world of beings, including our social being-in-the-world, publicly. There seems little reason to exclude from this category, as Arendt unfortunately does, most aspects of

worldly life simply because they pertain to the unheroic realm of social, physical, or economic need. Changing a baby's diapers or the oil in one's car, going to the marketplace or out of town for a holiday, do not in themselves evidence much political freedom, to be sure. But the provision of day-care facilities and parental leave from work, the promulgation of laws for the mandatory recycling of motor oil, the regulation of the sales of goods according to health and environmental standards, and the provision and maintenance of transportation infrastructure are important political concerns. What makes something political—in these cases and in general—has less to do with whether it pertains to our (basic) needs than to whether and how these needs are collectively addressed and fulfilled. Politics, in other words, may be in evidence whenever the world of beings and the with-world of human beings remain open to public interrogation and disclosure. Political freedom, then, emerges most fundamentally in the caretaking of this public realm. And such caretaking, accomplished in words and deeds, is both preservative and creative in nature. Arendt teaches us that sovereignty, personal or national in scope, is antagonistic to freedom. In the same vein, Heidegger teaches us that grand words and deeds, if carried out in the willful and forgetful pursuit of mastery, may also preclude freedom's greatest exercise. In today's world, where the caretaking of local and global ecologies becomes an increasingly important task, such a Heideggerian understanding of freedom bears obvious political significance.

Being, Doing, and Gender Identity

The boundary between private and public realms is. permeable, and the relationship between private and public realms is very much a political one. With this in mind, feminist theorists have offered sustained critical analyses of the ideological foundations of negative liberty. Liberalism, grounded in the concept of negative liberty, evidences an overriding concern for establishing boundaries between individuals in order to proscribe illegitimate interference. Consequently, liberals are primarily concerned with securing personal independence and rights. Many feminists claim that these concerns belie liberalism's masculinist tendencies, most evident in its neglect of what many describe as the predominantly (though not uniquely) feminine concern with establishing and maintaining relationships and community. Nancy Chodorow, relying on the work of feminist anthropologists, psychoanalysts, and object relations theorists, offers an explanation of this disjunction based on the early socialization of children. A girl's sexuality and sense of self, Chodorow theorizes, is "ascribed" to her through her relationship with her caretaker mother. She comes to understand her identity as expressive of her nature rather than explicitly dependent on a performative act. A boy, on the other hand, grows up in most societies with his same-sex parent much in absence and infrequently in the

caretaker's role. His sexuality and sense of self are therefore much less the product of an identification with the nurturing parent. Rather, a boy's identity must be "achieved" in distinction from and perhaps in opposition to—the mother. Achieving a stable male identity becomes a matter of asserting independence from the caretaker mother in a defensive establishing of ego boundaries. Moreover, boys' socialization accentuates the performative character of the sexual act. Thus feminine identity is socialized as a being, Chodorow observes, while male identity, constantly in need of demonstration, is socialized as a doing.

The political ramifications of this psychology are not difficult to foresee. The marked differences in the development of male and female identities in early childhood produce a disjunctive and hierarchical social structure. Male values of independence, competitive achievement and aggressiveness oppose, dominate, and are reinforced by female values of dependence, caretaking, and passivity. Chodorow concludes that "until masculine identity does not depend on men's proving themselves, their doing will be a reaction to insecurity rather than a creative exercise of their humanity, and woman's being, far from being an easy and positive acceptance of self, will be a resignation to inferiority" (1989, 44). Chodorow's research has stimulated many popular works that question the psychological predispositions structuring our social and political lives (e.g., Gilligan 1982; Belenky et al 1986). Without affirming the suspect generalizations of these efforts, I would submit that their resonance with such a wide audience speaks to the increasing instability of traditional (liberal and patriarchal) understandings of political and social identity. The prevalent notions of (negative) liberty that ground these understandings evidence their limits.

Heidegger's understanding of freedom as a disclosive letting-be informs and buttresses this feminist psychology and political theorizing. In a patriarchal world that narrowly conceives freedom as sovereignty, relations of mastery will continue to delimit human identity and define human dignity. As long as our understanding of freedom is restricted to the exclusion of others from our personal space (or, as with positive liberty, to the mastery of ourselves and our world), then our politics will remain one in which the individual meets the world and the other in a singular struggle for self-assertion. Alternatively, if precisely our relatedness to the world and to others begets our freedom, then the struggle for understanding comes to fore. At this juncture, philosophy and psychology most clearly cross paths, and the cultural and political implications of transformed understandings of freedom and dignity emerge. I am suggesting that patriarchal culture will continue to infect politics to the extent that freedom is understood and exercised solely as a masterful, independent doing rather than a disclosive, interpretive way of being.

The practice of disclosive freedom will not obviate

the interminable struggles of identity to which human beings are heir. The point is that these struggles may be waged interpretively, for understanding, as well as antagonistically, for mastery. The struggle for mastery need not entail an appreciation of the situated self, and seldom entails an understanding of the other, except insofar as it enables the other's exploitation. 11 On the other hand, the struggle for understanding typically entails the disclosure of the self and the other in the context of a shared world. It follows that disclosive freedom—the medium of this interpretive struggle—entails rather than curtails in its own actualization the participation in freedom of that which lies beyond the self. In these terms, the significance of disclosive freedom for a democratic politics emerges.

CONCLUSION

Twenty-five hundred years ago Pericles admonished the Athenians in his Funeral Oration, "Make up your minds that happiness depends on being free, and freedom depends on being courageous" (Thucydides 1954, 149-50). In fifth-century Athens, freedom already had become the most cherished value. The pursuit of this value, however, quickly became a mask for imperial mastery. The development of Western freedom has evinced numerous variations of this theme. Hegel could write with typical aplomb that "the history of the world is none other than the progress of the consciousness of freedom" (1956, 19). Hegel was not mistaken in his assessment of the tenor of history, for he correctly discerned the historical origins of Western freedom in the dialectic of master and slave. 12 At the end of history, Hegel announces, there is universal acknowledgement not that some are free and others in bondage but that man as such is free. And yet, firmly situated within the Western metaphysical tradition, Hegel could not see beyond its perimeter. Consequently, Hegel maintains that modern freedom is gained only by exchanging the mastery of some over the lives of others for humankind's presumed mastery over history itself.

The Western development of the consciousness of freedom, from its inception, remains in the service of such quests for mastery. Only the historical subjects and objects of domination change—from the domination of the master over the slave in ancient Greece, to that of the higher over the lower self with the Stoics, to Rousseau's (Marx's) romantic collectivization of this ideal in the reign of the general will (proletarian consciousness) over the individual, to liberalism's claim of the individual's sovereign control over a private realm, to postmodern mastery wherein the existence (let alone maintenance) of any such borders to the self are denied, while the ironic attempt is made to transform self-control into a full-fledged self-creation. I am proposing that we ponder a freedom no longer wedded to masterymastery being defined as willful control of the world, the other, a private domain, or the self (be that self understood as transcendental but repressed or awaiting its protean constitution).

Negative liberty is apt to degenerate into license and the breakdown of community; positive liberty, into tyranny and totalitarianism; postmodern liberty, into apathy and despair in the face of a ubiquitous web of power or (alternatively) into the isolated self-aggrandizement of one who in the face of inescapable normalization nihilistically embraces an unbounded self-invention. Disclosive freedom, no doubt, has its own pathologies and susceptibilities. Openness to the mystery of Being might degenerate into fatalism, the reining in of will might deteriorate into passivity before the powers that be. I am not proposing, then, that disclosive freedom wholly substitute for negative, positive, and postmodern liberties. Rather, the task is one of problematizing, supplementing, and perhaps sublimating their practise. Just as Berlin believes that positive liberty (as selfgovernment) should be viewed primarily as a means to protect negative liberty, so one might argue conversely that negative, positive, and postmodern liberties should be viewed as necessary means to protect and foster the conditions favorable to the exercise of disclosive freedom. And what Berlin concedes of positive liberty I would unbegrudgingly affirm for negative and postmodern liberties, as well: their exercise easily leads to dangerous abuses and perversions; yet held in balance they remain "essential for a decent existence." Heidegger, unfortunately, was reluctant to make such concessions. Still, he does not completely impugn the practical worth of prevalent notions of freedom. Rather, he observes their limitations and notes specifically regarding negative and positive liberty and their Nietzschean alternative that disclosive freedom stands over and above them from a philosophic perspective.

I have insufficiently indicated the political implications of disclosive freedom. My sense is that its political implications are necessarily ambiguous. This ambiguity is, to be sure, unsettling. But we should not be dissuaded from the exploration and practise of disclosive freedom for that reason alone. Isaiah Berlin, in closing his essay on the two concepts of liberty, asserts that the principle underlying negative liberty-perhaps only the "late fruit of our declining capitalist civilization"-is no less valid because the duration of its historical ascendency can not be guaranteed (1969, 172). The "relative validity" of our convictions, Berlin insists, should not dissuade us from upholding them. Indeed, he maintains that it would be a sign of a dangerous political immaturity to demur in the face of such uncertainty. I submit that the decaying fruits of our ecologically declining civilization have germinated the seeds of a disclosive freedom. This freedom, properly cultivated, offers us dignity and stamina in the political struggle against the "unchecked momentum" and "irresistible power" of a technologically driven way of life. Whether disclosive freedom will find a foothold in the postmodern world and, if so, just what its historical role and longevity might be surely remains beyond our

power to determine. But, like Berlin, I would argue that the convictions and experiences that underlie it, philosophical and worldly in nature, are on this count no less valid.

Notes

This paper was originally delivered at the 1992 American Political Science Association Annual Meeting in Chicago. I would like to thank my discussant, Stephen K. White, as well as Fred Dallmayr and other audience members for their comments. Funding for this paper was provided by the Social Science and Humanities Research Council of Canada.

1. I am not completely alone in this belief. Fred Dallmayr also finds that Heidegger's thought "from the earliest to the latest writings is permeated by the endeavor to grapple with the category of 'freedom'—in a manner that decisively moves beyond the confines of traditional liberalism and libertarianism" (1984, 207; see also Haar 1989).

2. Berlin notes that in theory negative liberty might have similar psychological requirements, demanding knowledge of which self is not to be interfered with and constrained. Historically, however, this has seldom been the case. Those promoting negative liberty have simply identified the empirically demonstrative self as the true self. Hobbes offers the most straightforward example of this understanding: "Liberty, or Freedome, signifieth (properly) the absence of Opposition; (by Opposition, I mean externall Impediments of motion;) and may be applyed no lesse to Irrational, and Inanimate creatures, than to Rationall" (1968, 261-62)

3. Even John Stuart Mill, Berlin notes, is guilty on this count. Berlin's views on this issue have been hotly disputed. Many have straightforwardly concluded against him that "to give up, for the sake of justice some freedom to act is not to

diminish liberty" (Reed 1980;376).
4. I take this characterization of Berlin's two concepts from Taylor 1979

5. Connolly also worries that Heidegger facilitates his being taken prisoner by the "communitarian" camp, who see freedom as an "attunement" to the world (1991, 225). Heidegger's talk of attunement (Gestimmtsein), however, refers not to faith in the "higher direction" of nature or the cosmos but (as we shall see) to an appreciation of profound mystery

6. Heidegger's turning, or Kehre, signals his shift from a focus on the heroic capacity of human being to disclose Being to a focus on human being's participatory relationship to Being's disclosure. In an insightful examination, Michel Haar (1989) suggests that human freedom becomes enveloped and overpowered by the "narcissism of Being" burgeoning in Heidegger's later writings. Haar's concern is based on the assumed equivalence of human autonomy and human freedom. However, once we accept that disclosive freedom is better described in terms of contextual participation than in terms of sovereign possession, Haar's worry about the "dispossession" of freedom by Being appears misplaced.
7. Jerry Weinberger (1992), like many others who are

polemically engaged with Heidegger, mistakenly assumes that the technological stamping of the age is equivalent to a pessimistic technological determinism. Heidegger, however, turns the tables on those who make such charges. He writes, "All attempts to reckon existing reality morphologically, psychologically, in terms of decline and loss, in terms of fate, catastrophe, and destruction, are merely technological behavior" (Heidegger 1977b, 48).

8. In the same vein, Charles Taylor writes that "Heidegger's understanding of language, its telos, and the human essence can be the basis of an ecological politics, founded on something deeper than an instrumental calculation of the conditions of our survival (though that itself ought to be enough to alarm us). It can be the basis of in one sense a 'deep' ecology" (1992, 266).

9. Heidegger is frequently misinterpreted on this score (see, e.g., Devall and Sessions 1985, 99). A literal biocentrism, if conceivable in practice, would certainly be undesirable. The AIDS virus and the useful bacteria of our digestive tracts are most interesting specimens of life. But that we should cease to exploit the latter or combat the former seems to me as silly as it is unnatural. Nature, as Nietzsche was fond of reminding us, is all about exploitation and battles to the death, and we should not be arguing for a world in which viruses have the same "rights" to life and growth as children (see Thiele n.d.).
10. We read, for example: "That all functional relations are

grounded ontologically in a for-the-sake-of in no way decides whether, ontically, all beings are as beings for the sake of the human Dasein" (Heidegger 1982, 295).

11. The best account of this dynamic that I am aware of is Todorov 1984.

12. One might argue that the Western valorization of freedom is primarily the product of slavery, that the value of freedom has developed out of the desire to escape the master's power. Hegel and Nietzsche both make this point in different ways with a respective historical and psychological intent. Recently (and with a sociological slant), Orlando Patterson has cogently reformulated the argument that "freedom was generated from the experience of slavery" (1991, xiii).

13. In a recent interview, Berlin states:

The only reason for which I have been suspected of defending negative liberty against positive and saying that it is more civilized is that I do think that the concept of positive liberty, which is of course essential to a decent existence, has been more often abused or perverted than that of negative liberty. . . . Certainly the weak must be protected against the strong. . . . Negative liberty must be curtailed if positive liberty is to be sufficiently realized; there must be a balance between the two, about which no clear principles can be

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POLITICS AND THE ENVIRONMENT: NONLINEAR INSTABILITIES DOMINATE

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his analysis investigates a formal nonlinear systems model characterizing longitudinal change in the environment as a function of oscillating partisan control of the White House. It is assumed that one political party will tend to favor help for the environment despite some economic costs, whereas the other party will generally favor economic growth over environmental concerns. These policy changes affect the environment interactively with both public concern for particular environmental problems and the economic costs relating to environmental repair. This interaction with policy changes causes a disruption in the continuously evolving balance between the social factors that damage the environment and the environment's own ability to recover. The disruptive potential to the environment is considerably ameliorated with a reduction in the electoral cycling.

The relationship between presidential elections in the United States and the degradation of the environment is not thoroughly understood. In this analysis, the relationship is characterized as a nonlinear interaction between oscillating environmental policy positions due to changes in partisan control of the White House and two other critical inputs. These inputs are public concern for the environment and the economic costs of environmental cleanup.

Broadly, this addresses a contextual question relating to political structure. The current theoretical connection between the dual cycles of partisan control of the White House and public concern for certain issues finds direct correspondence with recent research on cycles in the public mood by Stimson (1991). The joint effect of electoral structure and political context on this cycling is an extension of a thematic approach to the study of political parties as pursued by Beck (1974), Huckfeldt (1983), and Sprague (1981), in which party activities are seen as agents influenced by the structure of various social constraints. These constraints, combined with normal party activities, often produce nonobvious by-products. I investigate this phenomenon here with regard to environmental policies. The results of the current investigation suggest that the interaction of the inputs just mentioned can lead to a surprisingly high potential for significant, and perhaps dangerous, volatility in the quality of the environment.

The relationship between democratic political structures and the environment is a relatively new area of research. It is easy to see how a rigidly communist or dictatorial society could inflict substantial environmental damage locally (or, in some instances, even globally), since public pressure for such governments to be environmentally conscientious is typically lacking. But environmental damage occurs in democratic societies as well, and the level of this damage can be severe indeed, despite ostensibly 'popular checks" against blatant abuse.

During the past 20 years, there has been an increasing global awareness of the immediacy of our planetary environmental worries. In part, this is evidenced by the existence of international meetings dealing with the environment, such as the June 1992 meeting in Rio de Janeiro of the United Nations Conference on Environment and Development. However, what has become increasingly obvious in international negotiations relating to the environment is that the dominant consideration of each of the participants is the likely impact on their own domestic politics. It is not well understood how the organizational structure of each society's politics may influence longitudinal change in national environmental policies and thus long-term change in environmental degradation. In short, understanding the domestic aspects of environmental policies goes well beyond obtaining recent survey data of the general public's attitudes. If serious damage is being done to the environment, understanding how governmental structure, particularly democratic political structure, contributes to the outcome is critical to our being able to minimize that damage in the future. The focus here is on an aspect of the electoral structure of presidential politics in the United States as an example of this relationship between democratic political structures and environmental damage.

Crucial to this discussion is the understanding that in democratic societies in general (and certainly in the United States), environmentalism is subject to the same fate of political trade-offs as is any other concern. The typical environmental trade-off is economic, as measured in terms of losses in gross domestic product or jobs. Indeed, this is the focus of a recent analysis of the economic consequences of global warming by Schelling (1992). In one respect, the current investigation is an attempt to extend the discussions between environmentalists and economists to a broader range of social scientists as well, including political scientists who are interested in democratic electoral politics.

This analysis proceeds by developing a model of change in environmental degradation that is structured by electoral change, public mood, and economic cost constraints. Simulations are then performed on the model that reveal its more basic dynamic properties by systematically changing one input at a time while holding all others constant. An analysis of some of the global properties of the model is then conducted over a continuous and realistically wide range of potential parameter values important to the process. Substantive conclusions are then drawn from this characterization of the political-environmental process.

THE EXTENSION FROM ECONOMICS TO POLITICS

Most of the social science literature on environmental degradation points to economic consequences of particular environmental change (e.g., global warming, ozone destruction, toxic contamination), including governmental responses to these changes. Particularly far-reaching in this regard is Schelling's (1992) analysis of global warming. (Other prominent examples of the environmental–economic connection include Darmstadter 1991, Moulton and Richards 1990, Nordhaus 1991a, 1991b, and Poterba 1991).

On a more political level, one focus of the extant literature has been on the bureaucratic or regulatory responses to competing political and environmental demands. Tobin's (1990) analysis of the regulatory failure associated with biological diversity is seminal in this regard. Also, Wood and Waterman have produced two pioneering pieces of research that seek to identify some of the political determinants of longitudinal changes in environmental regulatory practices (Wood 1988; Wood and Waterman 1991).

A difficulty in studying the interaction between politics and the environment is the current lack of data with regard to long-term environmental degradation. However, we are not at a total loss in this regard. Environmental modelers routinely develop models in the absence of data to explore the consequences of environmental changes to a broad range of interests, including ecosystem stability (R. May 1974), global warming (Energy Modeling Forum 1992), and other aspects of planetary transformation (see *New York Times*, Nat. Ed., 5 May 1992, pp. B5–7). In such cases, models are developed in anticipation of the data that are sure to arrive eventually.

In the current absence of data, model simulations are used to understand the environmental consequences of human action. Indeed, the current separate worries regarding ozone holes and global warming are a response to just such model simulations. In part, simulations are useful because a society may want to *avoid* a particular consequence of human activity. Thus, data for catastrophic scenarios can, hopefully, never be collected if the simulations lead to policy changes that avoid the catastrophes. In fact,

simulations of plausible models are the *only* path available to us to understand the consequences of a given course of action without actually performing the action and waiting to see if disaster strikes in, say, a hundred years.

Yet models and simulations are built upon an understanding of the basic inputs of a process. Fortunately, we know a great deal about democratic electoral processes in the United States. Relevant to the current investigation, we know how constituent pressures are aligned differentially with regard to the political parties and how governmental policies reflect these constituent pressures. Moreover, in the social sciences more broadly, there exists a long and rich tradition of exploiting models similar to the one developed here (with or without data) using simulations. This includes areas of research as diverse as political economy (Hibbs 1977), contextual theories of voter activity (Brown 1991; Huckfeldt 1983; Przeworski and Soares 1971; Przeworski and Sprague 1986), arms races (Richardson 1960; Ward 1984), and racial segregation (Schelling 1978). Thus we have a methodological tradition as well as sufficient substantive knowledge to begin investigating an area of research that we might call "political ecology."

THE MODEL

In constructing a model of longitudinal change in environmental degradation that will be heuristically useful for analysis with simulations, it is wise to follow two basic guidelines.² First, the model should be general. Thus, model complexities should be held to a minimum so that it is relatively easy to identify change in the model behavior caused by varying each input. Second, it should be easy to identify the type of input that each of the components of the model contributes. In this case, the types of inputs will be limited in a classical fashion to gains and losses.

We begin by constructing a model describing change in environmental damage. Beginning with the gains (i.e., increases in degradation), a straightforward approach is to assume that environmental damage will increase logistically. At first, pollution will increase exponentially as industries grow and populations consume more products that harm the environment. However, this cannot continue forever, even in the complete absence of environmental legislation. In a worst-case scenario, people would eventually die, perhaps of starvation if the polar ozone holes spread to temperate zones, leading to diminished agricultural production due to the higher levels of ultraviolet radiation. But in a less draconian fashion, one would expect that environmental damage has some upper limit beyond which a society will not willingly go. To ease the discussion of the model, the level of environmental damage, X, will be scaled to have an upper limit of unity. Thus, increases in environmental damage can be expressed as

$$dX/dt = rX(1-X), (1)$$

where r is a constant parameter of the model and represents what we can label as the "pollution growth rate" parameter.

Some readers may wonder whether it is correct to model environmental degradation as a smooth growth process, since it may seem as if occasional spurts of activity would make the "ride" more bumpy. As with all models, the model developed here contains some simplifications, and a degree of "smoothing" is a desired trait of all attempts at segregating systematic and stochastic components of longitudinal change. However, many spurts in environmental degradation may be accounted for by the complicated oscillatory components of the model in its more fully developed form.

It is necessary to include two separate loss terms in equation 1. First, governmental environmental policy can act to limit or repair environmental degradation. This occurs, for example, when governments clean up toxic waste dumps, prohibit lead in gasoline, or (perhaps in the future) orbit large mylar balloons to reflect some amount of solar radiation in the event of catastrophic global warming (see Schelling 1992). The amount of governmentally inspired reduction in environmental damage is likely to depend on the interaction between the amount of damage that exists, and the current level of public concern for that damage. Such connection of policy to longitudinal cycling in the public mood can be deduced from empirical work by Stimson (1991), MacKuen (1981), and others and has played a notable role in Rowland, Lee, and Goetze's (1990) theoretical specifications involving cycling catastrophe models of environmental change. The theoretical justification for a specification for this loss is explained more thoroughly below. The second loss term to include with equation 1 addresses the ability of the environment to repair itself over time. Pollutants tend to decay, be they chlorofluorocarbons or toxic wastes. Some have short half-lives, while others stay around considerably longer, but virtually all eventually decay.

Thus, we can now write the complete expression of change in environmental damage as

$$dX/dt = rX(1-X) - pXY - kX.$$
 (2)

Here, the variable Y is the current level of public concern for environmental damage, and the parameter p identifies the effectiveness of governmental policies in reducing current levels of damage as an interactive function of the level of damage and public concern for the environment. The parameter k is a decay parameter that reduces environmental damage based on some proportion of current levels of that damage. In this analysis, k is set equal to the expression, -ln(.5)/halflife, where halflife is the number of years before one-half of the damage decays by itself.3 This allows us to examine the model based on how long a particular pollutant is expected to stay around once it is released into the environment. For immediate purposes, the term halflife can be thought of as an average half-life for current and total environmental pollutants. Note that the entire expression in equation 2 limits the level of environmental damage to the range of zero to unity. This convenience further generalizes the model to an acceptable range suitable for numerical investigation.

The second major input into the environmental relationship identified is public concern for the environment. Again, we will have gains and losses in public concern. As with environmental damage, it is reasonable to place a logistically defined upper limit on public concern. In this case, public concern for the environment would increase as damage to the environment increases up to some limit (call it the "panic" limit).

However, the public does not typically react to current levels of environmental damage in an instantaneous fashion. There is usually a lag in public concern as people wait until the environmental damage begins to affect them directly. Since the direct effects of environmental damage are usually due to pollutants introduced into the environment some time previously, the public is actually responding to an earlier level of environmental damage. Indeed, this lag can be as short as a few years or as long as many decades, depending on the particular type of environmental damage being considered. For example, it took a few decades for environmental pressure to build substantially with regard to the poisoning of the Great Lakes, yet concern for the ozone holes and global warming increased more quickly once the connections between these phenomena and human behavior were identified. For modeling purposes, we can say that there will be an average lag in public concern. Thus, public concern will increase as a function of previous levels of environmental damage up to some limit. As with environmental change in equation 2, it is convenient to set this limit at unity.

Decreases in public concern for the environment are most likely to be due to the costs of cleanup. If people are going to have to pay substantially higher gasoline taxes or taxes of another type, concern to clean up the environment is likely to diminish, on average. Thus the total expression for change in public concern for the environment can be written as

$$dY/dt = X_{\text{old}}(1 - Y) - Z. \tag{3}$$

Here $X_{\rm old}$ is the lagged value of the level of environmental damage, and Z is the cost associated with cleaning up the environment. Growth in public concern for the environment continues as long as concern is not yet near its limit and costs are relatively low. Once concern is near its limit or costs are high, costs will tend to dominate the dynamics in equation 3 and concern for the environment will begin to diminish.

The costs of cleaning up the environment will vary as well. There will be gain and loss characteristics of the change in costs. With regard to the gains, costs will tend to increase as *both* concern for the environment and actual environmental damage rise. Concern for the environment will spur politicians to address

environmental issues, but politicians will allocate money for the environment only when there are clearly definable problems that can be addressed. Yet there are limits to the funds available for all governmental projects. For numerical purposes, we can scale the variable for costs such that the upper limit for governmental spending on the environment is unity. Thus, spending for the environment will increase in the direction of this limit as long as there is increasing public concern for the environment and sufficient current environmental damage.

Longitudinal decreases in governmental spending on the environment are likely to be due to the magnitude of the current burden of environmental spending. Environmental spending will tend to increase as long as costs are low. As costs increase (and especially when costs approach their limit), public concern for the environment will tend to diminish (perhaps some environmental problems may appear resolved), and overall spending will tend to decline. Thus, the equation describing longitudinal change in spending for the environment can be expressed as

$$dZ/dt = XY(1-Z) - Z. (4)$$

In combination, equations 2–4 constitute a nonlinear system of three interconnected differential equations. The three state variables—environmental damage, public concern for the environment, and spending for the environment—interact longitudinally in a continuous fashion. As the system is currently specified, interactive cycling among all of the state variables is possible, as I shall demonstrate. However, to capture a more complete characterization of the cycling between public concern for the environment and electoral activity (a generalized empirical reality described more fully in Stimson 1991), this system requires a modification.

The modification is to allow the system to vary according to the partisan politics originating from the White House. This modification can be accomplished using parameter p in equation 2. The analysis has two levels of sophistication. As a first approximation to modeling the interaction between politics and the environment, parameter p oscillates between two different values, depending on the ideological perspective of the current president. When a conservative president occupies the White House, that president's supporters are not likely to include those environmentalists who want large increases in governmental spending on the environment, and thus parameter p is likely to have a relatively low value. However, a more liberal president will be more closely tied to the desires of environmentally active constituents. In such a situation, the value of parameter *p* is likely to be relatively high. Thus we will need to vary the value of parameter p in a systematic fashion in order to see how the change in presidential perspective affects overall environmental damage, given the other elements in the model.4

Oscillating the value of the parameter p between two discrete values is considered a first approxima-

tion to modeling the interaction between politics and the environment because many environmental policy changes that occur across administrations may happen more gradually. Discrete changes are examined first in order to identify causally the behavioral characteristics associated with specific components of the model in its simplest form. Later in this analysis, the model is extended to include gradual changes in the parameter p that would reflect less sudden policy alterations that may be more typical of many situations.

But let us be clear at the outset about what is being captured by the parameter p. There are two ways of thinking about the parameter. The first is that the government actually cleans up the environmental mess that it and others made. However, a second motivation underlying parameter p reveals a more subtle reasoning.⁵ Governments sometimes do clean up toxic disasters. But, perhaps more commonly, governments actively reduce environmental degradation by phasing out dirty technology in favor of newer and cleaner technology. Examples of this can be found in the construction of power plants, the use of catalytic converters on cars, the removal of lead from paint, and many other areas. Industry initially resists such conversions to cleaner technology, but once the conversion is made, the situation is relatively permanent. The reason for the permanence is cost. Industry does not want to invest in new infrastructure twice—once to clean things up and a second time (if allowed) to get dirty again.

The desire of industry to stay clean once it is forced to invest in cleaner technology does not nullify the reasoning behind the specification using parameter p. The reason is that society is always changing, and industry is always growing. Thus, there will constantly be new products and new industries that will be producing new sources of environmental degradation. The parameter p captures the government's overall ability to keep on top of this ever-expanding and constantly changing problem. It is not just that government sometimes cleans up environmental damage. Government also inhibits, through a variety of mechanisms, the development of new environmental problems. Thus, a high value for the parameter p reflects a government that is actively engaged in developing and maintaining a cleaner environment through a variety of means. The phasing out of old, dirty technology and the gradual introduction of cleaner technology is just one such means. The parameter p captures the summary effect of the total efforts.

There is very substantial empirical evidence suggesting that varying the value of the parameter p (both discretely and gradually) is a useful approach to modeling the interaction between politics and the environment. Since public concern for the environment began to heighten in the 1970s, there have been marked contrasts across administrations with regard to environmental concerns. Wood and Waterman have demonstrated that agency leadership is the critical factor in determining the direction of administrative environmental activities (Wood 1988; Wood and Waterman 1991). This affects dramatic and sud-

den shifts in agency funding and contributes to the long-term effectiveness of regulatory activity. Indeed, political appointments have greater influence over environmental activity than legislation, budget variations, or even actions by Congress. In sum, administratively orchestrated politics determines whether or not an environmental agency will be an effective advocate of environmental concerns.

Two heuristic examples are useful here. In the early Reagan years, a determined (and generally successful) effort was made to curtail quickly the scope, enforcement, budget, and effectiveness of environmental regulation. (For the detailed story of these efforts, which included the very effective appointment of an antienvironmentalist attorney, Ann Burford [née Gorsuch], see Harris and Milkis 1989; Vig and Kraft 1984; Waterman 1989; Wood 1988; and Wood and Waterman 1991.) However, such visible efforts are not needed in order to subdue an agency. Late in the Bush administration, it was reported that the Justice Department systematically but quietly blocked the prosecution of environmental crimes, thereby fatally derailing the efforts of the Environmental Protection Agency to enforce its own regulatory activity (New York Times, Nat. Ed., 30 October 1992, p. A13).

Thus, there is no one way to measure an administration's concern or lack of concern for the environment. If an administration wants to undermine environmental regulatory activity, there are many ways to do it. Moreover, the same is true if an administration seeks to be broadly supportive of environmental concerns. It is for this reason that parameter *p* is specified as a summary estimate of all current environmentally related activity that reverses total levels of degradation, assuming that each administration will use whatever means are currently at its disposal to achieve its desired goals.

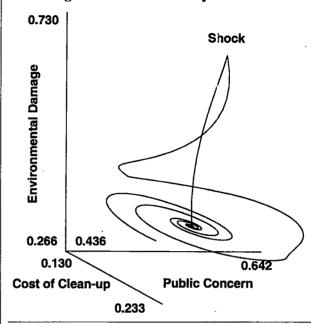
THE NUMERICAL EXPERIMENTS

As is typical of nearly all such nonlinear and interconnected systems of equations, it is not possible to uncouple the separate state variables in order to evaluate the system's behavior using techniques of indefinite integration. Moreover, linearizing techniques are both cumbersome and not very useful in the current situation. Rather, to learn from the system outlined, it is best to approach the system numerically with Hamming's maxim constantly in mind: "The purpose of computing is insight, not numbers" (Hamming 1973, 3).

The strategy of the current approach to analyzing the system is similar to the way one would approach any empirical problem. First, a baseline description of the system is established. From this baseline description, the general behavior of the system in its simplest form is identified. Subsequently, experiments are performed in which one change is made to the system at a time, thereby gaining an understanding of the effect of that one influence on the overall model.⁶ In this way, the model in its baseline char-

FIGURE 1

Phase-Portrait Representation of Environmental Damage, Public Concern, and Environmental Cleanup Costs: Sample Trajectory with Six-Year Public Lag and No Partisan Policy Differences



Note: All trajectories spiral to a single global attractor, thus moderating environmental damage, despite short-term catastrophes.

acterization acts as the controlling specification with regard to the component that is being varied.

Figure 1 presents a phase portrait representation of a sample trajectory for the model.⁷ This figure is used here as the baseline characterization of the system's behavior. The three axes represent the three state variables in the system: environmental damage, public concern for the environment, and environmental cleanup costs. In Figure 1, the parameter *p* is held constant with a value of 1 and is therefore not varied between two values.⁸ This represents a situation in which there are no policy differences between the parties and all presidents behave similarly with regard to favorably promoting environmental policies. Moreover, the lag in public awareness with regard to concern for the environment is held at a moderate value of 6 years for this simulation.

Note that the trajectory spirals into a stable equilibrium point from its initial condition. Moreover, this equilibrium point reflects a relatively low level of overall environmental damage, as would be expected in a situation in which both parties favorably support environmental policies. After the trajectory's arrival in the proximity of the equilibrium point, a large shock is added to the system to see how quickly the trajectory would return to the neighborhood of its equilibrium. This shock represents what would happen to the system following a short-term ecological disaster. Note that the return trip is relatively rapid.

System Phase Portrait: Sample Trajectory with Six-Year Public Lag and Partisan Policy Differences 0.884 0.192 0.192 0.192 0.136 Cost of Clean-up 0.296 Note: With a public lag of six years and political party differences, a stable orbit develops that leads to large—and potentially disastrous—changes in environmental damage, as well as cleanup costs.

Thus, the system as represented in Figure 1 (i.e., in the absence of partisan policy differences and with a moderate lag in the public's perception of environmental damage), supports (at equilibrium) a stable low level of environmental damage with moderate levels of public concern and relatively low financial costs. The swings encountered while arriving at that point may seem a bit rough, particularly following a major shock to the environment. But the eventual arrival at a pleasant and sustainable equilibrium does not take long. All this makes sense and would be expected from a properly functioning representation of this process under the given conditions.

Partisan differences in environmental policies are added to the system in Figure 2, that being the only difference. Thus, in Figure 2, the parameter p was allowed to oscillate between 0 and 1 every eight years. This represents a situation in which each party would hold substantial differences in environmental policies and the two parties would rotate control of the White House every eight years. A value of 0 for parameter p would reflect an administration that, say, strongly supports economic growth over environmental protection. Thus, governmental actions to decrease further damage to the environment would be minimal at best. A value of 1 for parameter p would reflect an administration that actively promoted proenvironment policies.

In the situation given in Figure 2, note that a large stable orbit develops in the system. Moreover, even the ecological shock to the system disturbs the trajectory from this stable orbit only briefly. Thus, in the long run, the model is quite sensitive to environmental policy variations between the parties that rotate control of the White House.

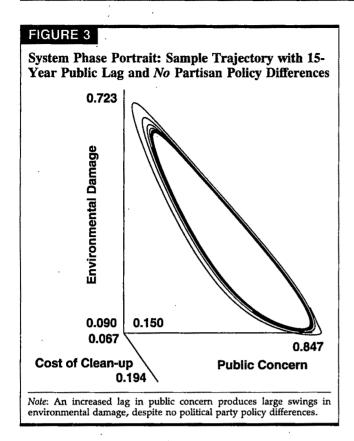
From an environmental and cost point of view, the model's behavior as represented in Figure 2 is not good news. The problem is the large magnitude of the cycling and the consequent high level of governmental financial costs. When, say, a pro-jobs president is elected, the environmental policies of his or her predecessor may have reduced the level of environmental damage. Moreover, with environmental damage more or less under control, the public's concern for the environment can begin to diminish. This leads to a relaxation of proenvironmental legislation and regulatory activity with its consequent eventual increase in environmental damage. At this point, the public desires change, a new party occupies the White House, and the process of reducing damage to the environment repeats.

One major problem identifiable in Figure 2 is that the overall costs of this cycle appear to be much greater than the relatively moderate and stable costs represented in Figure 1. From an environmental point of view, the repeated and high levels of damage are the problem. But from a financial point of view, the cycle itself, with its large and costly swings into the expensive programs needed for environmental recovery, is the most damaging. Indeed, from both points of view, it might seem a better choice to reduce such long-term costs as well as environmental dam-

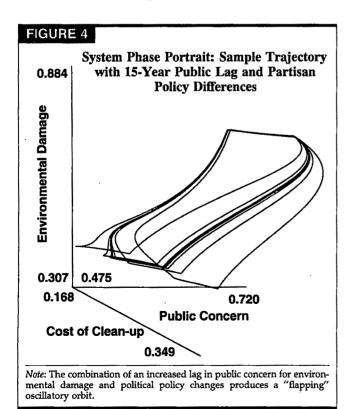
age by avoiding the cycling.

But differing party policies with regard to the environment is not the only thing that can cause large magnitude and costly swings with this system. Indeed, even in the complete absence of political differences, the system is sensitive to the length of the lag in the public's perception of environmental damage. This sensitivity is demonstrated in Figure 3. The only thing changed is the length of the lag in public concern regarding environmental damage. Policy differences between the political parties are not included in the computation of the trajectory in this figure. Here, the lag between environmental damage (e.g., the release of pollutants) and the public becoming concerned about the damage is 15 years. The result of this increased lag is that the system develops a large stable orbit. This is a substantial characteristic change from the stable equilibrium that dominated the trajectory in Figure 1. It is clear in Figure 3 that the trajectory slowly converges to an orbit in which there are large changes in environmental damage, public concern, and economic costs. This is despite a political situation in which both political parties pursue similar environmental policies!10

When political policy changes occur together with an increased lag in the public's concern for environmental damage, the oscillations continue, but with more abrupt changes in direction and a much more complicated orbital form. Figure 4 combines both the longer 15-year lag as well as 8-year policy changes in the White House. From the phase-plane perspective, the orbit in Figure 4 seems to "flap" at lower levels of environmental damage, somewhat mimicking the



swings of a pendulum. From this figure, it is clear that directional changes in environmental damage can be much more radical than those associated with Figure 3. Of course, these changes are associated with substantial changes in costs. But the costs often



vary in this figure out of phase with changes in the other variables.

This temporal disconnectedness between costs and actual damage is directly tied to the interactive influence of the long lag in the public's awareness and the discontinuities inherent with the political policy differences. Moreover, this type of lagged cycling may be common. Notably, such cycling has been observed between the public mood and partisanship by Stimson, using a large body of survey data (1991, 93–94).

In terms of a goal of maintaining a healthy environment, this latter situation could be quite dangerous. The overall impact on the environment is one of rapid departures from previous states and thus an increase in the longitudinal volatility of environmental quality. In some respects, this situation may partially reflect the general characteristics of the contemporary state of much environmental management in the United States today.

GRADUAL POLICY CHANGES

In its present form, the model presented here assumes that governmental policy changes with regard to the environment are sudden. In some—perhaps many—situations, this may not be an unreasonable assumption. Wood and Waterman have offered empirical evidence suggesting that national environmental policy changes are primarily influenced by the nature of top political appointments rather than by legislation, changing budgets, or Congress (Wood 1988; Wood and Waterman 1991). Moreover, these policy leaders can display a remarkable ability to produce a wide range of change very quickly.¹¹

However, not all policies—environmental or other -will change suddenly. Indeed, more gradual changes across a broad range of policies may likely be closer to the norm of regulatory politics. At first, this may seem like a good thing. Gradual changes enable governments to come to grips with the consequences of previous policies and thereby to fine-tune subsequent policy developments. Thus we would expect increased predictability and a reduction in environmental instability when policy changes are more gradual. This, in turn, promises a reduction in the overall level of environmental damage. Surprisingly, these intuitions may be largely false. Indeed, gradual changes in policy need not eliminate dangerous and large-scale oscillations in environmental damage, and they can also end up hindering our overall ability to manage the environment.

To show why we are not necessarily better off with gradual policy changes with regard to the environment, it is necessary to reformulate the model to include gradual, rather than sudden, changes. Thus, we need a new equation in the system. This equation will structure change in the parameter p, the government policy response to environmental damage. A high value for p suggests that the party in power is strongly engaged in reducing environmental damage through regulatory and other activities, whereas a

low value for p indicates the opposite. Previously, the model has allowed p to flip from one value to another, thereby reflecting discrete changes in policy preferences across administrations.

We now want to vary p continuously, rather than discretely. That is, the value for parameter p must change continuously in the direction of a desired partisan goal for that parameter. For example, a Democratic administration may desire p to have a high value, whereas a Republican administration may prefer it at a lower value. These desired values are partisan goals, to which the value of the parameter p should asymptotically approach, depending on which party is in power at a given point in time. Algebraically, this is accomplished by introducing into the system the equation

$$dp/dt = ep(g_{\text{Dem}} + g_{\text{Rep}} - p).$$
 (5)

In equation 5, $g_{\rm Dem}$ and $g_{\rm Rep}$ are the partisan goals for the parameter p. Only one such goal can operate at a time, since only one party at a time can control the presidency. Thus, when a Democratic administration is in power, g_{Dem} is set to its ideal value and g_{Rep} is set to zero, the reverse being true when a Republican administration is in power. Thus, all of the terms in parentheses in equation 5 act in combination as a logistic directional control for the evolving value of parameter p. For example, when a Democratic administration is in power, the value of pchanges in the direction of $g_{\rm Dem}$. Change slows down when p approaches $g_{\rm Dem}$ as a limit. The parameter eidentifies the rate (proportional to the current value of p) at which this movement in the value of p takes place. All of this changes *p* from a constant parameter to a variable parameter. The entire model is now a four-equation system, which is collected and summarized as follows:

$$dX/dt = rX(1 - X) - pXY - kX$$

$$dY/dt = X_{old}(1 - Y) - Z$$

$$dZ/dt = XY(1 - Z) - Z$$

$$dp/dt = ep(g_{Dem} + g_{Rep} - p)$$

Again, the variables and parameters are

environmental damage

X Y Z public concern for the environment

economic costs of environmental clean-up governmental policy response to environmenp

tal damage (a variable parameter)

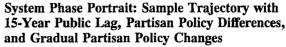
ideal Democratic policy response goal &Dem ideal Republican policy response goal gRep pollution growth rate parameter

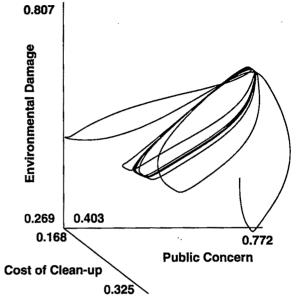
k natural pollution decay rate

parameter determining speed of government policy changes toward partisan goals

Our first test to see the effect of this change in determining the value of parameter p on the overall behavior of the model will be to duplicate all of the other conditions used to create Figure 4, which in-







Note: The combination of an increased lag in public concern for environmental damage and gradual political party policy changes can produce smoothed versions of the flapping oscillation observed in Figure 4. Here, r=1 and e=1. Democratic and Republican ideal policy response goals are 1 and 0, respectively. This corresponds to position A in Figure 6.

cluded partisan policy differences as well as a 15-year lag in public concern for environmental damage. This new simulation is presented in Figure 5. The values of parameters r and e are set equal to 1, and the Democratic and Republican ideal policy response goals (g_{Dem} and g_{Rep}) are set equal to 1 and 0, respectively. Thus the situation is very similar to that of Figure 4, except that the value of parameter p is allowed to move continuously and gradually between two oscillating limits. From Figure 5, the primary effect of this change on the overall model is to round off the sharp edges of the trajectory that were previously the result of sudden policy changes. Largemagnitude oscillations in environmental damage still occur, and the flapping oscillation pattern of Figure 4 reappears, albeit in the form of rounded loops rather than more sharply angular movements.

A natural observation relating to Figure 5 is that we are looking at only one value for the parameter e. Recall that the value of parameter *e* controls the rate at which government policy changes toward the partisan goals emanating from the White House. High values for this parameter corresponds to rapid movement toward these goals. Very high values can produce movement that mimics that associated with sudden shifts in policies, as portrayed in Figure 4. Very low values make movement between the two goals sluggish at best.

At this point in the analysis, we need a way to portray the overall behavior of the model through a continuous and realistically suitable range of values for the parameters e and r without paging through a countless series of trajectory phase portraits. This is important because we do not yet have a hundred or so years of data from which to estimate these parameters. One counterargument to the current analysis would be that the particular values used for these two parameters are not likely to be the exact values that the system will actually have once it is estimated, given an appropriately long collection of time-series data. Other parameter values may produce less dangerous systemic behavior. Moreover, it is likely that these parameter values will migrate over time (i.e., not remain constant) to keep pace with evolutionary changes in our social and political cultures. Thus, it is important to describe the general topology of the environmental playing field on which the values of these parameters are placed so that one can generalize about the overall characteristics of the system. As will become obvious, one can find little solace in the idea that it may be better to wait to collect the data before worrying about the intricacies of the politicalenvironmental connection. Indeed, in terms of environmental risk and a desire to engineer an effective collection of environmental policies, it may be difficult to imagine a less hospitable general setting.

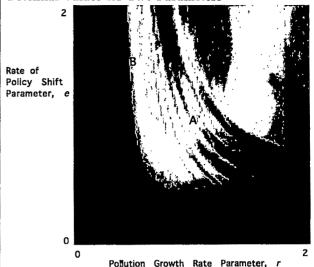
To do this, a new measure is needed that will capture two aspects of the overall behavior of the model. The first is the sum of the absolute value of total change in environmental degradation. Small values of this aspect indicate that change is slight and thus probably more manageable from a regulatory perspective. On the other hand, large values of total change indicate dangerous levels of environmental volatility. The second aspect of our new measure must indicate how much directional change characterizes the model's behavior. This is captured by counting the total number of changes in sign in the model's four derivatives for a fixed period of time.

The new measure is calculated by weighting the total magnitude of change in the environment by the total number of sign changes in the system's derivatives. This measure is an estimate of overall environmental turbulence. High levels of environmental turbulence indicate large magnitudes of overall environmental damage combined with frequent oscillations in the direction of change. Low levels of this measure indicate relative stability in environmental damage, both in terms of the total magnitude of change and the direction of movement. The measure is comparable across trajectories as long as the length of time for all trajectories is held constant.

Figure 6 is a portrait of the measure of environmental turbulence across a continuous range of values from 0 to 2 for the parameters *e* and *r* (the pollution-growth-rate parameter).¹² The environmental turbulence measure is being shown on the third dimension of the figure by the shading. Brighter shades in the portrait indicate high levels of environmental turbulence whereas darker shades indicate lower levels of turbulence. The letter "A" on the figure indicates the

FIGURE 6

Portrait of Environmental Turbulence for the Entire System Based on Continuous Ranges of Potential Values for Two Parameters



Note: This is a portrait of the total magnitude of change in the environment weighted by the number of sign changes in the system's derivatives. Brighter shades indicate large-magnitude changes in the quality of the environment combined with many shifts in the direction of change for the variables (i.e., high levels of environmental turbulence).

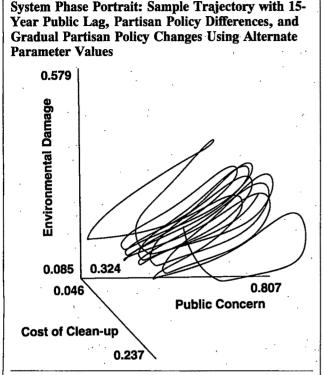
location of the parameters used to construct the trajectory presented in Figure 5.

In Figure 6, note that continuous change in the values of the parameters does not produce gradual changes in the measure of environmental turbulence. Indeed, the portrait in Figure 6 is filled with closely interwoven variations in shading, indicating rapidly changing levels of turbulence. That is, turbulence does not increase smoothly as, say, industries increase their level of pollutant discharge. Nor is the change in turbulence gradual if the government pollution reduction rate changes evenly. Moreover, the only areas in Figure 6 in which darker shades dominate in a significantly continuous fashion are for situations with very sluggish rates of partisan policy change (i.e., very low levels for the parameter e) or under conditions of very low levels of environmental damage, neither of which seem realistically to describe current regulatory or environmental conditions.

In order to manage the environment, one must be able to predict what will happen when changes are made. The results of Figure 6 suggest that this may not be easy or even possible, given current measurement inadequacies. Attempts to manage the environment within a setting of significant differences in partisan approaches can lead to large-scale and highly volatile environmental changes, many of which may not be easily reversible from an ecological point of view.

As bad as this sounds, the situation is worse. There are large areas in the parameter spaces of parameters e and r for which the determination of what will happen next in the environmental trajectory is nearly

FIGURE 7



Note: Modest parametric variation in the four-equation model can produce very complex oscillations in environmental damage. Here, r=.5 and e=1.7. Democratic and Republican ideal policy response goals are 1 and 0, respectively. This corresponds to position B in Figure 6.

impossible to predict. This is due to complexities in the oscillatory components imbedded in the trajectory movements. The oscillations portrayed in Figure 5 seem easy to follow, but for other values of the parameters *e* and *r*, the situation changes dramatically and can easily become much more complicated.

To show these conditions, an additional phase portrait is constructed, using the parameter values as identified in Figure 6 by the letter B. Figure 7 presents such a complex trajectory using the parameter values e = 1.7 and r = .5. From this figure, it is clear that policy managers would have a difficult time predicting the future of environmental change given this political and pollution context. The oscillations are evident, but they do not settle down into a clearly discernable pattern within any reasonable length of time. On a technical level, such complexity in trajectory structure may not be certifiably chaotic (classically defined), since the characteristic Lyapunov exponents calculated using these parameter values do not indicate a strong sensitivity to initial conditions (analysis not shown). However, a Fourier analysis of the system's periodicity (not shown) indicates a complexity very typical of chaotic systems. From a macro substantive perspective, there seems little difference between these complex oscillations and pseudorandomness, since the level of longitudinal complexity is sufficiently high to make a practical ability to predict the future virtually impossible.

Substantively, this implies that a great deal can happen quickly with regard to the environment when small changes occur in pollution and policy rates and when the political parties differ in their environmental policies. This is precisely the fear raised by Schelling with respect to global warming (1992, 8). It is this potential characteristic of the environmental system that holds the greatest long-term danger for planetary ecological management.

CONCLUSION

It would be absurd for an astronaut in a space vehicle to start a fire in the cabin to keep warm. No one would question this because we understand that the regulatory mechanisms within the vehicle would be overloaded from the consequences of the fire. The problem with making a similar statement with regard to damaging the environment of our planet is that we do not entirely understand the automatic processes of environmental management. These processes include political structural components as well as ecological and physical components, and these political structural components have been largely ignored in the environmentally related literature.

I have endeavored to describe some of the political components that may strongly interact with the rest of the overall biological system. The normative hope is that the discussion will engender a more balanced approach to the discussions of our environmental problems. The social-scientific angle to the environment is not just economic. Since politics is at the root of all attempts at environmental management, political scientists should play a major participatory role in the evaluation of current problems and in the prescription of current and future remedies.

This analysis presents a model of environmental change in which the political, social, and economic inputs into the system interact nonlinearly to produce highly varied global patterns of ecological damage. The model is developed with respect to presidential-electoral and regulatory practices in the United States. The results of the analysis suggest that relatively minor parametric changes in the system can lead to major alterations in the longitudinal patterns of environmental change.

One of the major implications of these experiments is to suggest that the regulation of our environment may be a much more challenging task than we have presently envisioned. Indeed, our current electoral structures may directly assist the long-term degradation of our environment. The complexities that exist within the simplified system investigated here suggest a greater level of complexity in the actual physical system. The political components of that complexity certainly play an important role in the determination of the general structure.

In terms of recommendations, it is likely that substantial benefit for environmental management from a political point of view would be to reduce the impact of party policy differences on ecological cycling. A complete plan on how to do this is beyond my present scope, since I am here concerned more with demonstrating the dilemma we are in than formulating a way out. However, some suggestions to initiate the discussion of solutions may be helpful.

First, in the Federal Reserve Board we have an example of an institution developed in order to deal with potential cycling problems (in this case, in the economic arena). Thus, we have experience with related problems in other areas and the effectiveness of some of the solutions that have been attempted. It is not likely that a simple political compromise between the political parties would last sufficiently long to be meaningful to the long-term global properties of the system. The temptations to exploit short-term political gain would eventually destroy any temporary effects that would result from the compromise. Something more permanent and substantial is needed, and the creation of some type of independent Environmental Preserve Board may be one answer.

It may also be possible—perhaps more so in countries other than the United States—to create, through constitutional means, an elected position independent of the influence of the office of president or prime minister. This elected official would serve as the head of the environmental regulatory agency of a country. This could have major implications on the management of the environment, since the official would gain reelection solely on the basis of whether voters are happy with how the environment is being regulated. Thus, it would not be possible to avoid the issue of the degradation of the environment by talking about other issues in which the voters might also be interested. The basic problem is to isolate the environmental concerns from other political matters.

Yet another suggestion would be to institutionalize the role of the United Nations in monitoring—and to some extent regulating—the planetary environment. Rules could be constructed that would establish planetary norms with which each country would have to abide. Some fair system of penalties would have to be established in order to obtain cooperation across nations. Such penalties would likely involve trade and economic issues, but technology transfers and political cooperation in other areas could also be factors. Interestingly, the United Nations already seems to be becoming much more involved in new global matters since the end of the Cold War. Thus, the stage may already be set for the emergence of a new environmental role for this organization.

But it is important to understand that helpful environmental management cannot function only on the level of bureaucratic organization: individuals must understand the need for this management. In particular, the regulation of the environment is influenced by the lag in the public's perceptions of our environmental problems. To some extent, this can be addressed through an enhanced role for environmental education in our society. On the level of formal education, it is most likely that a comprehensive approach to curriculum reform across all age categories will be required in order to achieve any lasting

benefit to the overall problem, since generational biases often persist for lifetimes and it may be too late in the game for college courses to make a meaningful impact.

This last recommendation cuts to the heart of the matter. If nonlinearities dominate the political-environmental system, it may be futile to try to fine-tune current environmental policies in a rational-decisionmaking sort of way in an attempt to fix the ecological problems. If political leadership, regardless of party, tends (at least on average) to reflect in practice the policy preferences of large numbers of a nation's citizens, then it is necessary to upgrade the preferences of the citizens through education before environmental disaster does it for us. It is not that evil political and corporate leaders continually dupe the ignorant political masses. Rather, in a democracy, it is the masses that let their leaders do what they do. The bottom line is that unless large majorities of the citizens of democratically governed countries become strongly environmentally sensitive, there can be no long-term solution to this problem. The electoral and ecological cycling will continue as parties with differing and large constituencies oscillate in office, and the costs of environmental cleanup will remain both high and volatile. Possible governmental-structural fixes may help in the long run, but only if the educational component is also present.

Fundamentally, any successful approach to environmental management will require an understanding of the political processes that influence that management. At the outset, it is important to understand how these processes extend to the basic electoral practices of our democracy. The current discussion has attempted to demonstrate that the political–environmental connection in the context of contemporary democratic governance is complex. This nonlinear complexity is associated with questions relating to human quality of life, and indeed, survivability. We address these matters not as mathematical curiosities but as issues with a direct bearing on the physical characteristics of our planetary habitat.

Notes

2. See Mesterton-Gibbons 1989 for a more complete overview of such guidelines.

In all of the numerical results presented herein, the half-life for the system's environmental damage was fixed at six years.

4. Recent anecdotal evidence nicely illustrates how sitting presidents can and do change regulatory environmental policy to achieve electoral benefit with regard to current constituents. The following list of articles and editorials, all drawn from the National Edition of the New York Times during the 1992 presidential primary season, describe how President Bush made an effort to reduce, wave, or eliminate environmental regulations that adversely affected potential or current supporters of his campaign. This partial listing is not meant to

^{1.} Indeed, the collapse of the former communist governments of Eastern Europe and the former Soviet Union led to near-immediate revelations of how deep and widespread such damage can be (New York Times, Nat. Ed., 13 May 1992, pp. Al, A4.)

imply that President Bush is unique in any way in this regard. The argument made here is that this is a natural occurrence of partisan politics in the United States in which environmental concerns fall victim to the same constituency pressures as any other issue or set of issues.

"Icy Words on Global Warming," 30 March, p. A14

"Administration Tries To Limit Rule Used To Halt Logging of National Forests," 28 April, p. A7

"The Environmental Pollution President," 29 April, p. A15 "Freezing Government," 4 May, p. A14

"2 Admit E.P.A. Violated Hazardous Waste Law," 8 May,

p. A10 "E.P.A. Head Allows Project on a Lake Michigan Marsh," 9 May, p. A7

"Showdown on Endangered Species," 11 May, p. A15

"White House and Congress Face Showdown on National Forests," 12 May, p. A8

"Bush to Relax 1990 Rule on Air Pollution Notices," 18 May,

"Environment Laws Are Eased by Bush as Election Nears," 20 May, pp. A1, A10

- 5. I am indebted to G. Robert Boynton for suggesting this second justification for parameter p.
- 6. This approach to the numerical evaluation of such systems is described more fully in Kocak 1989, as well as Mesterton-Gibbons 1989.
- 7. Throughout this analysis, all numerical solutions to the differential equations were obtained using a fourth-order Runge-Kutta algorithm. Moreover, all of the results presented here were obtained using software written by the author in Think Pascal and run on a Macintosh Quadra 900. The computer was made available to the author through a grant administered by James Johnson, vice provost of Emory University's Information Technology Division.
 - 8. In Figures 1–5, the parameter r is given a value of 1.
- 9. Of course, actual partisan changes would not be as evenly spaced. Holding the spacing constant here, however, acts to control the structure of this new input, thereby clearly identifying its effect on the overall system.
- 10. By way of casual observation, this scenario indeed seems a partial contributor to what may have happened to the Great Lakes of North America. The public's response to the increasing levels of pollution was very slow, probably due to the indirect ways that lake pollution affected peoples' lives.
- 11. For example, the Bush administration passed a flurry of regulations regarding the environment in the final days of its tenure (New York Times, Nat. Ed., 16 January 1993, p. A1). Similarly, the Reagan administration engineered a dramatic decline in funding for the Environmental Protection Agency during President Reagan's first term in office (Wood 1988).
- 12. Figure 6 was constructed using imaging software provided by the National Center for Supercomputing Applications at the University of Illinois, Urbana-Champaign.

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DIVIDED GOVERNMENT IN THE AMERICAN STATES: A BYPRODUCT OF LEGISLATIVE PROFESSIONALISM?

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Since World War II, divided government has become increasingly common in the American states. A significant component of the increase is the deterioration of Republican fortunes in state legislatures: after the 1990 and 1992 elections, for example, only five state legislatures were controlled by the Republicans. I shall examine the hypothesis that the professionalization of state legislatures makes legislative service more attractive to Democratic candidates and less attractive to Republican candidates, because full-time legislative service is incompatible with another career, and Democrats, on average, have less lucrative career opportunities than Republicans. Statistical analysis of post–World War II legislative elections outside the South is consistent with the argument: other things being equal, every ten-thousand-dollar increase in real biennial legislative compensation is associated with approximately a 1% increase in Democratic legislators.

Bill Clinton's election brought an end to an unprecedented 12 consecutive years of divided national government. But few observers are confident that Clinton's plurality victory has banished divided government from the scene, especially with the Democrats defending two-thirds of the Senate seats to be contested in 1994. Given the frequency and duration of split control of national institutions in recent decades, some have labeled the contemporary period a "new era of coalition government" (Sundquist 1988), while others, continuing to adhere to the electoral realignment–party system interpretation of American electoral history, view the contemporary period as a "sixth party system" (Aldrich and Niemi n.d.; see also Shafer 1991).

Students of elections have increasingly begun to focus on the electoral causes of divided government (Campbell n.d. [a]; Ginsberg and Shefter 1990; Jacobson 1990). The research question typically has taken one of two forms: (1) Given their continuing advantage in party identification, why have the Democrats had so much difficulty constructing presidential majorities? and (2) Given their sweeping presidential victories, why have the Republicans been unable to convert their presidential support into congressional-especially House-majorities? Commentaries adopting the first question as a starting point typically focus on a combination of divisive social issues and disappointing economic and foreign policy performance, perhaps exacerbated by an open nominating process that highlights the divisions within the Democratic party. Commentaries starting with the second question typically focus on Democratic gerrymandering and incumbency, though academic research casts doubt on such explanations (Jacobson 1990; Mann 1987). These two starting points obviously are not mutually exclusive: the Democrats may fall short of their potential in the presidential voting while the Republicans simultaneously fall short in the congressional voting. There is reason to believe that divided national government is a complex rather than

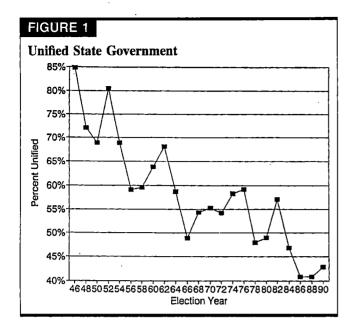
simple phenomenon, with multiple causes that may be logically and empirically independent.

Recent research further complicates the discussion. I have noted elsewhere that American state governments have also become increasingly divided (Fiorina 1992; see Figure 1). Moreover, the features identified in state patterns of divided control resemble those in the national case. In particular, the Republicans do somewhat better in executive races than state partisanship would predict, but they have steadily lost ground in state legislatures. These developments suggest the operation of factors broader than those typically used to explain the components of divided national government, for if developments in Ohio, California, and other states resemble those in the nation at large, the existence of common or related causes should be considered, at least as a starting point.

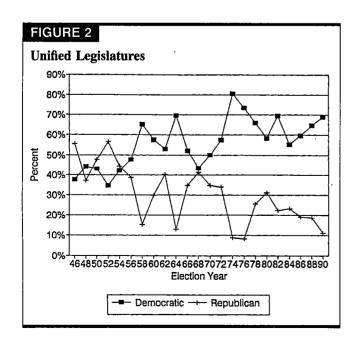
I shall focus on one aspect of the growth of divided government in the American states—the decline of Republican fortunes in legislative elections—and one contributing factor to this decline, namely, the development of legislative professionalism, or (as some might prefer to call it) careerism. Briefly, the analyses that follow are consistent with the argument that all other things being equal, professionalized legislatures are relatively more attractive to Democrats than to Republicans. Thus, postwar good government reforms that encouraged the professionalization of state legislatures had the unintended consequence of undercutting the Republican party in that arena. Moreover, given that state legislatures are the traditional "farm teams" for Congress, the decline in Republican state legislative fortunes should naturally lead to some decline in their congressional fortunes as well.

PARTY FORTUNES IN STATE LEGISLATIVE ELECTIONS

The decline in unified state government largely reflects a decline in unified Republican state govern-



ment, which in turn largely reflects a decline in Republican legislatures (Fiorina 1992, chap. 3). Since World War II, there has been considerable variability but no partisan trend in gubernatorial elections. Legislative elections are another matter, however. Even while spotting the Democrats a 15-state lead in the southern and border states, in 1946 and 1952, the Republicans captured an absolute majority of state legislatures (Figure 2). Their fortunes have slipped dramatically, however, and following the 1990 and 1992 elections, only five states had Republican legislatures. The impact of adverse national circumstances is clear (the 1958 and 1974 recessions, the Goldwater debacle), but it is also clear that such adverse shocks are embedded within a long-term secular decline. Note that the Republican collapse in state legislatures had taken place by the mid-1970s (Figure 2). Over the



more recent period spanned by the State Legislative Elections Project (1968–86), there is little variation in the number of Democratic state legislators.

Interestingly, although there are several excellent studies of state legislative elections, none of them have addressed the Republican decline. Analyzing elections in 41 states between 1944 and 1984 Campbell (1986) finds that popular presidential candidates help state legislative candidates of their parties and that the longer the presidential coattails, the larger the subsequent off-year loss. Because Campbell's dependent variable is change in legislative seats, however, the steady decline in Republican strength does not stand out. Chubb (1988) examines Democratic lower-house success in nonsouthern legislatures over the period 1940-82, finding that state election results are significantly influenced by the coattails of candidates for higher offices, by national economic conditions, and by turnout surge and decline. He does not comment on the declining percentage of Republican legislators, though the decline is statistically incorporated in a lagged variable measuring the percentage of lower-house Democrats in the preceding legislature. Similarly, Simon, Ostrom, and Marra (1991) find that control of legislative chambers between 1950 and 1988 depends on such short-term factors as the president's standing in the polls and dramatic political events and such longer-term factors as the national party balance and the departure of the state outcome from its long-run equilibrium. Again, these authors do not comment on the Republican decline, though it is incorporated in the long-run equilibrium. Thus comprehensive statistical analyses establish that legislative outcomes in the states reflect both long-term party competition and short-term political events and conditions, but these analyses incorporate the Republican legislative decline statistically without calling attention to it or providing any substantive explanation for it. The analyses reported herein build on these earlier efforts but keep the Republican decline front and center.

As a starting point, let us take a closer look at party control in the state legislatures. As usual, the South is special. The Republicans have *never* won control of a southern or border legislative chamber in the postwar period: all of the action in Figure 2 occurs outside these regions. Thus, I omit the 15 southern and border states from consideration, along with Alaska, Hawaii, Minnesota, and Nebraska.² Controlling for short-term variation in the national political climate (measured by interelection change in Gallup approval ratings appropriately signed for the party of the president), the time trend of Republican control in the remaining 31 nonsouthern states is given in Table 1, column 1.

Since World War II, the Republicans have lost legislatures in the non-South at a rate of almost 2% per election. The passage of time is not an explanation, of course, but a context in which true explanatory variables operate. Elsewhere I have suggested that legislative professionalization works in favor of Democrats (Fiorina 1992). Substituting an extremely

 $1946 = 1, 1948 = 2, \ldots, 1990 = 23.$

TABLE 1

Correlates of Republican Control of State
Legislatures: Non-South, 1946-90

INDEPENDENT	MODELS		
VARIABLES	1	. 2	3
Constant	60.53 (15.25)	55.93 (15.13)	60.55 (15.71)
Presidential approval change	−.59 (.21)	58 (.22)	59 (.22)
Time trend	-1.74 (.57)	*********	-1.69 (1.61)
Annual sessions		36 (.13)	01 .36
Republican control (t - 1)	.08 (.19)	.17 (.18)	.08 (.20)
Corrected R ²	.55	.52	.52

crude indicator of professionalization (the simple percentage of states whose legislatures meet annually) for the time trend in Table 1, we get column 2. For every 3% increase in states holding annual legislative sessions, the Republicans lost control of approximately 1% of the state legislatures. The trend in annual sessions tracks the time trend very closely (the simple correlation between the two variables is .95), and when both are included in the equation (column 3), multicollinearity deprives both of significance.

Of course, there are numerous other time series that trend upward (or downward) over this 23-election period. Any of these might perform just as well statistically as the trend in annual legislative sessions. I shall develop the argument that legislative professionalization enhances the electoral prospects of Democratic candidates, then present the evidence.

OPPORTUNITY COSTS AND LEGISLATIVE CAREERS

In the early postwar years, most state legislatures were part-time, minimally supported institutions whose members were poorly compensated. Most were constitutionally limited to sitting every other year. Legislative scholars commonly label such bodies amateur, because under these conditions turnover is high, and service is an avocation, not a profession. Some state legislatures still fit this description. The Wyoming legislature, for example, is permitted to meet for 20 days during even years and 40 days during odd years. Members receive \$75-per-day salary with a \$60 per diem, for a total biennial compensation of less than \$7,000. Other perks are minimal. Upper- and lower-chamber turnover rates are similar—about 30% per election in the early 1980s (Niemi and Winsky 1987, tbl. 1).

New York offers a striking contrast. It is a good

example of what legislative scholars refer to as a professional legislature. In session annually from January through June and again for a period in the Fall, with almost 20 staff per legislator, each member receives a \$57,500 annual salary and \$75 per diem, as well as travel allowances. Moreover, two-thirds of the assembly and the entire senate receive so-called "lulus," that is, "in lieu of" payments for holding various party and committee leadership posts (Fowler and McClure 1989, 84–89, 96). Assembly turnover in the early 1980s was about 20% per election, with senate turnover under 15%. Voluntary turnover was less than 10% (p. 90).

In the early postwar years, most state legislatures fell on the amateur end of the continuum. But over the years, especially during the 1960s, many legislatures have become more professionalized (Kurtz 1991). In particular, legislative service has increasingly become a full-time occupation. Bazar reports that as of 1986, between 11% and 20% of legislators nationwide were full-time and that in some areas (e.g., the middle Atlantic states), over half were professional legislators (1987, 2). My hypothesis is that this development advantages Democratic candidates relative to Republicans. The hypothesis is a logical implication of several propositions previously noted by students of state politics.

First, legislative service does not make equivalent demands on people from different walks of life. Discussing Arkansas (an amateur legislature), Blair comments: "A realtor or banker or lawyer, the owner of a furniture store or car dealership, may have the time flexibility and financial security to adjust his or her schedule to a regular session of two months or more, increasingly frequent special sessions, and countless committee meetings and constituent obligations in between. The realtor's or lawyer's secretary, a bank teller, the furniture and car salespersons who must be at work if they are to receive an income, simply cannot serve" (1988, 164).

Second, if legislative service has an asymmetric impact on people from different occupational and income strata, we should expect *changes* in the conditions of legislative service to change the relative attractiveness of service for different categories of people. That is what Rosenthal reports:

Minnesota is an example of a state in which marked change in the composition of the legislature has taken place. Lawyers, independent business owners, and farmers have left. Their places have been taken by young people, many of whom see the legislature as an entry level position for a career in politics. But as noticeable as anything else, the contemporary Minnesota legislature has more full-time members. . . . The neighboring state of Wisconsin has moved in the same direction, with younger members and fewer who work part time at the job. There is only a single farmer left in the senate. . . . Legislators themselves are not united on the benefits of professionalization: . . . in California the entire fabric of the process has changed. "The way it used to be, we only had men of means, of experience," a senior senator remarked. "Now we have full-time legislators." (1981, 58)

Finally, we have the common observation that the two parties are based in different socioeconomic strata. Arguing that the Republicans had an advantage in an era of part-time politicians, Key observed:

Outside the South, the Republican group has . . . a far larger reservoir of persons with leadership skills on which to draw. Well-connected lawyers, businessmen with time and money to devote to politics, and perhaps to a lesser extent, persons with skill in professional politics gravitate in greater degree to the Republican party than to the Democratic. By the secondary network of economic relations within their own group-legal retainers, insurance commissions, real estate transactions, and the like-the business community within the Republican party can sustain a class whose time and energies may be dedicated principally to the practice of politics. . . . The Democratic Party, on the other hand, enjoys the handicaps in recruiting leadership created by its position as a party devoted in principle to mass causes. (1956, 256–57)

Now, as then, the parties have discernibly different socioeconomic bases. As mass surveys monotonously demonstrate, the Republicans have a higher-income, higher-status popular base than the Democrats, especially when the profit sector is distinguished. And elite surveys find that Republican elites come relatively more from the profit sector, Democrats from the public and other nonprofit sectors.³

Consider a simplified model based on the preceding propositions. Within a state, two parties compete for public office. Within each party is a pool consisting of all potential candidates, that is, those possessing the minimal qualifications deemed appropriate for service. The parties' candidate pools differ in their occupational and income characteristics. The Republicans are a higher-income, more private-sector party; their potential candidates are independent professionals, proprietors and farmers, and the independently wealthy (as well as their spouses, given that such families are better able to afford domestic substitutes). The Democrats are a lower-income, more public-sector party. Their potential candidates are union officials, teachers, and other public- and nonprofit-sector employees. Given such differences, variations in the degree of legislative professionalism are likely to affect the decisions of those in the respective candidate pools.

Those in the Republican pool are advantaged where the legislature is on the amateur end of the continuum. They are more likely to have the career and financial flexibility to absent themselves from their principal occupations for one to three months a year. Some of their responsibilities can be moved to evenings and weekends or delegated to associates, relatives, or employees. Indeed, for lawyers in private practice, the contacts made in political life raise the value of their outside hours.⁴ In contrast, salary and wage earners in the Democratic pool forgo their incomes when they take leave for legislative service—if leave is even feasible. Even if ambition levels are comparable, Republican candidates are more able to pursue their ambitions.

With a professional legislature, the logic reverses. If legislative service is a full-time occupation, legislators must sacrifice outside careers for legislative office. Those who have lucrative private-sector careers will tend to exit legislative service—or never enter—rather than abandon those careers. In contrast, for potential Democratic candidates, legislative service now becomes an attractive alternative career, probably better compensated and more highly regarded than their present careers. Again, even if ambition levels are equal between the respective candidate pools, Democratic candidates are now in a better position to act on their ambitions.

In sum, in amateur legislative settings the critical question is, "Who has the flexibility to combine legislative service and an outside career?" In professional legislative settings, the critical question is, "Who is willing to sacrifice an outside career for legislative service?" If the career patterns of Democrats and Republicans are as just posited, then, ceteris paribus, the answer to the first question is, "More likely, Republicans," and the answer to the second is, "More likely, Democrats."

Several aspects of the argument deserve additional comment. First, the argument says nothing about net turnover in legislatures. There is a substantial literature on that subject, of course. Reformers have supported professionalization partly as a remedy for excessively high turnover, and some research finds that correlates of professionalism (e.g., longer session length and higher compensation) do reduce turnover (Calvert 1979; Rosenthal 1974). The argument made here, however, is that increasing session length and compensation have a differential partisan impact, advantaging Democrats relative to Republicans.

Second, the argument is a selection argument. Institutional change (professionalization) has altered the mix of incentives facing legislative candidates so that prospective Republican candidates find legislative service relatively less attractive than in the past, while prospective Democrats find it relatively more attractive.⁵ Increasingly, the former select out, and the latter select in. This logic resembles that of Ehrenhalt (1991), though the two arguments have different principal emphases. Noting that Democrats (including those of the most liberal persuasion) sometimes dominate local councils and state legislatures in seemingly conservative locales, Ehrenhalt hypothesizes a difference in party attitudes toward government. Democrats generally believe in government, like to use government to improve society, and consequently enjoy government service; Republicans, in contrast, are skeptical of government capacity, dislike government, and consequently find service much less rewarding. This too is a selection argument. The two arguments are not at all inconsistent. Indeed, they are quite complementary, and Ehrenhalt takes note of the greater career commitment evidenced by Democrats. But I have said nothing about psychological or ideological benefits and costs, only the more tangible costs and benefits of legislative service. Full-time service imposes higher

opportunity costs on Republicans, and generous compensation raises the direct benefits of service for Democrats. If the logic of my argument is operative, party fortunes will respond to measures of *tangible* benefits and costs. Ehrenhalt's argument, in contrast, could be valid even in the absence of such relationships.⁶

A third point is that my argument is easy to caricature. I am not claiming that candidates for office look at nothing but the prospective wages and hours, with Republicans being discouraged by longer hours, and Democrats encouraged by higher wages. I am suggesting merely that such considerations affect career choices. On the margins, some qualified Democrats—however public-spirited—will find the sacrifices of service in an amateur legislature too great to bear. Similarly, some qualified Republicans—however public-spirited—will find the sacrifices of service in a professional legislature too great to bear. Such marginal decisions affect the respective candidate pools and, indirectly, the composition of the legislature.

One objection to the foregoing argument is that correlations between occupation, income, and partisanship, while significant, are far from perfect. The objection is well taken, but the empirical question is not the absolute strength of the correlations but whether they are strong enough to contribute to the explanation for the Republican legislative decline. That question can only be answered by looking at the evidence. From past research, we know that candidates' decisions vary systematically with measureable characteristics of the offices they hold, the offices they seek, their attitudes toward risk, and such features of the electoral context as party competitiveness and national forces (Black 1972; Jacobson and Kernell 1981; Rohde 1979; Schlesinger 1966, esp. chaps. 2, 3). This research simply expands the list of factors known to affect candidate decisions.

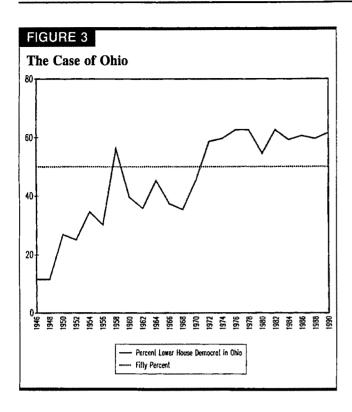
ANALYSIS

There is a wide-ranging literature on legislative professionalism.8 Much of it addresses legislative organization and resources (the power of the leadership and the autonomy of committees, the size of legislative staffs, etc.) For purposes of the present research, however, I limit my attention to the two aspects of professionalization that should most immediately affect the individual career decisions of candidates: the length of time that the legislature sits and the compensation for legislative service. Other aspects of professionalism (e.g., size of legislative staffs and autonomy of legislative standing committees) are important for the operation of legislatures and apparently affect incumbent reelection chances (Holbrook and Tidmarch 1991; King 1991; Weber, Tucker, and Brace 1991). My principal attention, however, is on the consequences of candidates opting in and out of the arena, and for that decision, time commitment and compensation would seem to be of greatest

relevance. Price (1975) and Ehrenhalt (1991) refer to these aspects of legislative professionalization as simply "professionalism."

The analysis that follows examines party fortunes in the lower houses of 31 nonsouthern state legislatures over the 23 elections between 1946 and 1990. Three variables were selected to represent the career attractiveness of a legislature. First, analogous to the aggregate analysis already reported, I coded a dummy variable (ANNUAL) as 0 for the years in which a state legislature was restricted to biennial sessions and 1 after the change to annual sessions. In the immediate postwar period, only 16% of the legislatures in the analysis had annual sessions. That number more than doubled in the 1950s, more than doubled again by 1972, and stands at 87% today (see Appendix). Despite this sharp trend, I did not expect this gross measure to produce significant relationships in the disaggregated analysis. In the first place, a few states already had annual sessions in 1946—so for them, the variable is a constant for the period of this study. And in the second place, some states that adopted annual sessions experienced only small increases in actual legislative service. For example, in 1974, Wyoming began meeting for 20-day sessions in the even years-hardly the kind of increase that would lead many potential candidates to recalculate the benefits and costs of legislative service. Thus, I expected any effects of annual sessions to be incorporated in a second, more sensitive measure, DAYS.9 Calculated from successive editions of Book of the States, this is simply the total days that the legislature was in session during the election year and the year preceding (see Appendix). While exact session dates are available, this measure nonetheless understates the demands of legislative service, since it does not include committee meetings and constituency service conducted when the legislature is not in session. Given the argument I developed, I expected Republican electoral fortunes to wane as states moved to annual sessions and days in session rose.

Third, again using The Book of the States, I calculated the total biennial compensation for legislators, expressed in 1982 dollars. This measure includes legislator base salaries, plus their per diems and other daily allowances multiplied by days in session. Again, the measure is only approximate. It does not capture payments for legislative service (such as committee hearings) while the legislature is not in session. Additionally, "lulus" are not included, although they have come to be increasingly important. Such "in lieu of" payments are hard to factor in since only some legislators receive them. At any rate, since more experienced legislators are usually the beneficiaries, "lulus" probably do not dominate the decisions of new candidates. 10 As explained earlier, we expected Democratic fortunes to wax as real compensation rose. In the early postwar years, average compensation was less than per capita state income, but in the 1950s and 1960s, legislative compensation rose at a much faster rate (see Appendix). State



legislators suffered in the inflationary 1970s and prospered in the 1980s.

To illustrate the variation in the three indicators of professionalism, consider the experiences of two states. Figure 3 shows Ohio's interesting postwar history. Prior to 1972, the Republicans won the lower house in every year except 1958. But in the 1972 elections, with George McGovern heading the ticket, the Democratic percentage jumped 13 points, and they have been continuously in the majority since then. Did anything obvious happen around the time of this sharp change in partisan fortunes? Yes: at least two developments may be relevant. First, the 1970s reapportionment was carried out by a reapportionment board that had a three-to-two Democratic majority. Perhaps they were extraordinarily clever and produced a massive, lasting Democratic gerrymander.1 Alternatively, Ohio voters approved a constitutional amendment providing for annual sessions. From a total of 315 days in the 1967 biennial session, the Ohio legislature moved to 717 in the 1971-72 annual sessions. 12 Coincidentally, between the 1968 and 1972 elections, the Democrats scored a 23% seat gain. 13 Simple regressions analogous to those reported earlier in Table 1 indicate that the increased session lengths account for more than half of the Democratic gain.

California (Figure 4) provides a different illustration. Republican hegemony came to an end earlier than in Ohio—in the election of 1958. Since then, Republicans have attained majority status only in 1968. Since California has had annual sessions throughout the postwar period, that variable has no explanatory relevance. While days-in-session has been trending steadily upward, a simple regression

analysis analogous to those in Table 1 shows little indication that length of session is related to the improvement in Democratic fortunes. But the third professionalism variable—real compensation—bears a significant relationship to Democratic success. Each \$10,000 (biennial) increase in real legislative compensation is associated with a 1% increase in Democratic legislators. Thus, the observed \$80,000 increase in real compensation in postwar California directly contributes to a 9% improvement in Democratic fortunes.¹⁴

While suggestive, individual state analyses are untrustworthy, given the paucity of observations underlying them. By pooling the data, we can carry out a more efficient analysis. The pooled time-series cross section is a rectangular array of 713 observations, somewhat denser in space (31 states) than in time (23 elections). Analysis of such pooled data often runs afoul of ordinary least squares assumptions, namely the homoskedasticity and independence of the residuals. Various correctives have been proposed, but all have their weaknesses. Fortunately, in practice, they seem to produce estimates that are very similar (Stimson 1985). Inclusion of a dummy variable for each state and a lagged variable for the percentage of Democratic representatives in the preceding legislative session should take account of the most obvious sources of difficulty. 15 Based on previous research, we also need to include variables representing national economic conditions, the coattails of candidates for higher offices, and the on-year-offyear cycle.

Table 2 reports the estimates for several related specifications of a dummy variable least squares model. In these specifications, each state has its own intercept (the constant is omitted). In effect, each state is allowed to have a different average *level* of

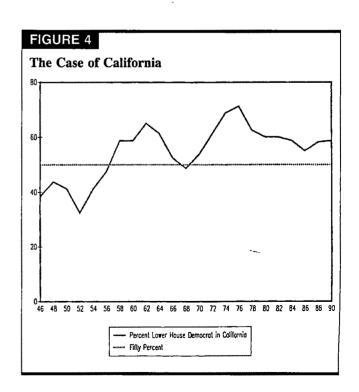


TABLE 2					
Democratic Seat Share in Non-southern Lower Houses, 1946-90					
INDEPENDENT		MOI	DELS		
VARIABLES	1	2	3	4	

INDEPENDENT	MODELS			
VARIABLES	1	2	3	4
Year	.004 (.047)			
Democratic Reps. (t - 1)	.505 (.029)	.505 (.028)	.512 (.027)	.515 (.02 7)
Annuai	1.05 (1.19)	1.11 (1.04)	_	
Days-in-session	.002 .004	.002 (.0038)	_	
Compensation				
Real	.0071 (.0042)	.0073 (.0039)	.0099 .0030	*********
Relative				1.21 (.363)
Presidential				
Year	-27.70 (2.81)	-27.68 (2.80)	-27.29 (2.78)	-27.14 (2.78)
Vote	.5199 (.0610)	.5191 (.0604)	.5086 (.0598)	.5026 (.0597)
Gubernatorial	, ,		•	, ,
Year	-20.13 (2.78)	-20.18 (2.74)	-20.14 (2.74)	-20.08 (2.74)
Vote	.3986 (.0529)	.3993 (.0523)	.3981 (.0522)	.3968 (.0522)
Off-year	9.76 (1.06)	-9.78 (1.03)	-9.98 (1.02)	-10.11 (1.01)
GNP growth	.1731 (.0628)	.1732 (.0628)	.1670 (.0625)	.1501 (.0622)
SEE	9.02	9.01	9.01	9.01
Note: Standard errors are in parentheses. $N = 712$. Corrected $R^2 = .735$.				

Democratic representation, although a common dynamic is presumed. (However, the 31 state intercepts are not reported.) Column 1 includes all the variables discussed so far. 16 The time trend (YEAR) is effectively zero, suggesting that the substantive variables in the analysis capture all the temporal variation in the data. 17 Democratic strength in the preceding session (DEMOCRATS (t-1)) is of major importance. The coefficient of .5 might suggest less dependence on past history than one might have anticipated, but coefficients of such lagged terms are generally biased downward in this sort of analysis. National economic conditions, measured here as the rate of growth of gross national product in the election year are significant, consistent with the findings of Čhubb (1988).18 On average, every 1% gain in gross national product during a Democratic (Republican) administration adds (subtracts) about a sixth of a percentage point to (from) Democratic state legislative strength. The coattail specifications follow Chubb: presidential and gubernatorial coattails are each represented by (1) a dummy variable equal to 1 for the election year and

(2) a variable for the actual vote in the state. This specification differentiates between years in which there is no election (vote = 0) from years in which the Democratic presidential or gubernatorial candidates perform poorly. Thus, presidential coattails are estimated as 0 in nonelection years, and -27.70 + .520(PRESIDENTIAL VOTE) in election years. Solving, Democratic state legislators are collectively better off running in a presidential election year than in an offyear, so long as their presidential candidate wins more than 53.3% of the statewide vote. For gubernatorial candidates, the figure is 50.5% of the vote. Given the performance of Democratic candidates other than Lyndon Johnson in the postwar period, it appears that they have not been of much help to their state legislative compatriots. Republican presidents, however, have been quite helpful: the coefficient of OFFYEAR (-9.75) indicates that on average, Democrats win almost 10% percent more state legislative seats in midterms when a Republican is president than when Democrats hold the White House. This result is consistent with either a "presidential penalty" model (Alesina and Rosenthal 1989; Erikson 1988) or a surge-and-decline" model (Campbell n.d. [6]).

Of most interest are the three indicators of professionalism. Annual sessions and days-in-session have the expected sign, but their coefficients are smaller than their standard errors. Real compensation, however, is just significant at the .05 level (one-tailed test). The coefficient indicates that each \$10,000 increase in real biennial compensation results in .7% more Democrats.

Column 2 omits the insignificant time trend. The coefficients change little but their standard errors become a bit more precise as multicollinearity lessens. Annual sessions and days in session continue to fall far short of significance, but the case for real compensation becomes slightly stronger. Column 3 omits "DAYS" and "ANNUAL." Absent their interference, the coefficient of real compensation rises to almost 1% more Democrats for every \$10,000 and the estimate is more than three times its standard error. Finally, if potential candidates define their career alternatives within state boundaries, the more appropriate measure might be legislative compensation relative to some state norm. Column 4 reports estimates of an equation that includes real legislative compensation relative to real per capita state income. The coefficient is again three times as large as its standard error. Thus, whether we measure it in absolute or relative terms, legislative compensation clearly matters. To illustrate, real biennial compensation in California has increased from approximately \$10,000 to more than \$90,000 in the postwar period. Applying the national coefficients from column 3, this gain in compensation should have led directly to a Democratic legislative gain of 8 + %. Alternatively, real legislative compensation relative to real per capita disposable income in California was five times higher in 1990 than in the early postwar years. According to column 4, the predicted Democratic gain is about 11%.

OBJECTIONS

Increasing legislative compensation and increasing Democratic representation are apparently related. Could the relationship be spurious? The most obvious possibility is that both variables could be correlated with some omitted third variable. For example, some commentators have hypothesized a pro-Democratic trend in political sentiments in nonsouthern state elections. It is difficult to test such a possibility directly: registration or other measures of partisanship are incomplete both by state and by year. At any rate, once substantive variables are taken into account, there is no trend in Democratic state legislative fortunes (column 1). Thus, proponents of a "trend" hypothesis would have to argue that some unobserved drift in partisan sentiments happens to bear a close relationship with the observed trend in real legislative compensation. The trend in compensation is a more parsimonious explanation for the trend in Democratic fortunes than is some hypothetical unobserved trend in partisan sentiments.

Alternatively, one might object that legislative compensation is just a reflection of state population, state income, and size of state government. Bigger, richer, more active states pay their legislators more, and such states—the populous, urban, industrial states—tend to be more Democratic. These two propositions are certainly true, but that does not make the relationships reported in Table 2 spurious. In the first place, these propositions were as true in 1952 when the Republicans won 55% of the presidential vote and 26 of 31 nonsouthern legislatures as they were in 1988 when the Republicans won 54% of the presidential vote but only 9 of 31 nonsouthern legislatures. If there were no temporal correlation between rising compensation and increasing Democratic strength, we would only find larger intercepts for the urban, industrial states. In the second place, if we add state sociodemographic variables to the analysis, we find that they do not eliminate or even weaken the estimated effect of legislative compensation: in fact, the coefficient of legislative compensation marginally increases when state population, personal income, and state government expenditures are added to the model (b = 1.06, t = 2.38). Moreover, the coefficients of the additional control variables are smaller than their standard errors, and expenditure has the "wrong" sign. Thus, the state intercepts and the variables previously included in the analysis fully take into account interstate differences in socioeconomic development.

Finally, what of the fact that the upsurge in Democratic legislators ceases in the early 1970s? Part of the answer may be that real legislative compensation also stopped rising after 1972 (see Appendix), although it resumed its increase after 1982. Another possibility is that the effect of compensation is nonlinear, being stronger up to the level where legislative service becomes competitive with alternative Democratic career opportunities, but weakening thereafter. Taking the log of real compensation, however, resulted in a

TABLE 3	•	;			
Democratic Vote Share in Nonsouthern Gubernatorial Races, 1946-90					
INDEPENDENT	NDEPENDENT MODELS				
VARIABLES	1	2	3		
Year	.023 (.032)	anti-gram	_		
Democratic vote (t - 1)	.324 (.047)	.334 (.045)	.340 (.045)		
Compensation					
Real	.0022 (.0027)	.0026 (.0026)	.—		
Relative	.		.1028 (.1717)		
Presidential					
Year	-8.43 (3.15)	-8.46 (.3.15)	-8.49 (3.15)		
Vote	.144 (.067)	.138 (.067)	.135 (.067)		
Off-Year	-4.83 (1.02)	-5.13 (.929)	-5.29 (.942)		
GNP Growth	.0786 (.0635)	.0078 (.0635)	.0742 (.0648)		
Corrected R ²	.228	.229	.228		
Note: Standard errors are in pare	ntheses; n = 4	165; SEE = 7.6	i3.		

poorer specification than the linear one. And several interactive specifications yielded no significant results. Why the Democratic gains ceased after the early 1970s must remain a question for future research.

CONTRAST WITH GUBERNATORIAL ELECTIONS

Why have Republican fortunes plunged in legislative elections but fluctuated without trend in executive elections? The office of governor is and always has been full-time, prestigious, and respectably compensated. Thus, unless party competition is so unbalanced (e.g. as in the South) that every aspiring politician must seek a career within the dominant party, we would expect that the gubernatorial candidate pools are and have been more evenly balanced than the legislative pools. In particular, compensation levels were not so low early on as to disadvantage Democrats nor later on so low relative to the private sector as to discourage Republicans. Thus, real compensation should *not* have any impact on partisan fortunes in gubernatorial elections.

Gubernatorial elections have been much studied and are known to reflect state party strength, the national economy, and national party fortunes (Campbell 1986; Chubb 1988; Simon 1989; Simon, Ostrom, and Marra 1991). To variables of this sort I add real gubernatorial compensation (Table 3). The resulting

estimates are consistent with earlier, more elaborate studies in finding that presidential coattails, the off-year penalty, and previous support levels are important. As anticipated, however, the effect of real compensation, whether measured absolutely or relative to state income, has only a weak and insignificant effect.

IMPLICATIONS FOR CONGRESS

I do not think that a simple extension of the present argument can help to explain the persistence of Democratic majorities in the modern U.S. House. The New Deal realignment and resulting distribution of party identification would seem to be a sufficient explanation for Democratic House majorities between 1952 and the mid-1960s. The more interesting question is why Republican House candidates have not fared better during the more recent elections when their presidential candidates were faring so well. Possibly, falling congressional salaries (in real terms) and rising executive and professional compensation discouraged some potential Republican candidacies, but given the prestige of the office and the absolute level of compensation, the argument seems somewhat strained.22

Still, professionalism may have contributed indirectly to the stability of the Democratic House majorities. State legislatures have long been considered "farm teams" for Congress. State legislatures provide the opportunity for inexperienced politicians to learn how to nurture a constituency and solidify an electoral base. The more talented and ambitious of these eventually have opportunities to move up to the national legislature. Despite suggestions that service in some state legislatures may now be preferable to service in Congress (Fowler and McClure 1989), state legislatures remain the principal springboard to Congress. Indeed, Canon (1990) reports that state legislatures have become an even more important source of congressional recruits in recent years. Thus, the decline in Republican fortunes at the state level has probably contributed to their chronic difficulties at the congressional level. Jacobson attributes the lack of Republican congressional success in the 1980s to their failure to run "good" candidates, where "good" is defined as previous electoral success (1990, chap. 4). By the 1970s, the Republicans had fewer state legislators in general and fewer in positions of legislative leadership in particular. Their lack of good candidates in the 1980s may well have been a by-product of their declining fortunes in state legislatures during the preceding two decades.

CONCLUSION

While the work reported herein is preliminary, the relationships uncovered seem robust enough to merit further investigation and the argument plausible

enough to merit further development. Several lines of research are prime candidates for further work.

First, the empirical research reported here is an indirect test of the general argument. The outcomes of state elections are consistent with the implications of the argument, but focused research in the states could shed additional light on the hypothesized mechanisms. If professionalism makes legislative seats more attractive to Democrats and less attractive to Republicans, then, ceteris paribus, as professionalism increases, does the number of contested Democratic primaries increase, and Republican, decrease, and does the number of uncontested Democratic races increase, and Republican, decrease? Do retirement rates of Republican incumbents increase relative to Democrats as professionalism increases? Do Democratic candidates look more impressive and Republican candidates less impressive as professionalism increases? Such patterns would constitute more direct evidence that candidate career calculations are indeed the mechanism underlying the patterns reported herein.2

Second, was there any partisan political impetus for professionalization of state legislatures? I have taken professionalization as exogeneous in this study—a result of socioeconomic development and a nonpartisan reform impulse. But partisan politicians can be expected to see the partisan implications of nonpartisan reforms: Did they, in the case of professionalism? Were Democrats more likely to support and Republicans to oppose the move toward fulltime, reasonably compensated legislative service? Analysis of the data reported herein finds that both session lengths (significantly) and real compensation (not significantly) are positively related to the proportion of Democrats in the previous legislature (controlling for the previous values of session lengths and compensation as well as the socioeconomic characteristics of the state). If further research supports this finding, we might conclude that Democrats tend to encourage professionalism, but whether either party consciously anticipated a partisan impact is a different question. Democrats may well have done what they could to make public office a full-time position without suspecting that it would give them an advantage relative to Republicans. Again, however, this is an indirect way to look at the question. There are records of legislative deliberations and constitutional amendment campaigns: Who supported and who opposed what? Were partisan divisions present in roll calls and referenda? Or were the decisions to extend sessions and raise compensation more often regarded as nonpolitical, procedural matters?

Third, I reiterate the neutrality of my findings. Higher levels of legislative compensation appear to have made service in state legislatures relatively more attractive to Democrats and relatively less attractive to Republicans. This dynamic has contributed significantly to the great turnaround in party fortunes in state legislative elections since World War II. Such a general finding would appear to be fraught with political and partisan implications. In particular, frus-

trated Republicans may jump to the conclusion that state legislatures have been captured by greedy Democrats who consider the legislature a good job. No such conclusion follows. Judgments of over- or underrepresentation presume some objective baseline. Perhaps Democrats are overrepresented now, while Republicans were overrepresented earlier. But Democrats might be properly represented now because Republicans were so greatly overrepresented earlier. We cannot tell without some independent measure of the "proper" level of representation. It is tempting to use the vote for some statewide office like auditor or attorney general as a measure of partisan sentiment, but districting arrangements would still muddy the representational picture. The residential concentration of one party's supporters may make it inevitable that it "wastes" more of its votes and wins fewer legislative seats than its level of popular support indicates. Judgments about appropriate levels of representation are exceedingly complex and require more information than anything reported here.

Fourth, to the extent that terms-limitation initiatives also significantly limit days-in-session, legislative compensation, and legislator perks, such initiatives should make the legislatures more amateur and, quite likely, more Republican. But if service limits are imposed on legislatures without changing much else, little partisan impact should be expected: in amateur legislatures term limits would bind few members (Moncrief et al. 1992), and in professionalized legislatures, 8 to 12 years in office should be a sufficiently long lead time that even the most risk-averse Democrat will not be discouraged.

Finally, one of the perennial themes in the literature of political science is that political reforms have unintended consequences. In the professionalization of state legislatures we appear to have another example of a seemingly nonpartisan, "nonpolitical" reform, some of whose consequences may well be highly partisan and highly political.

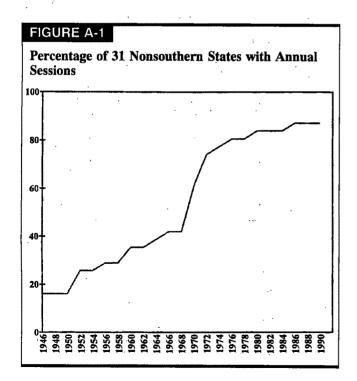
APPENDIX

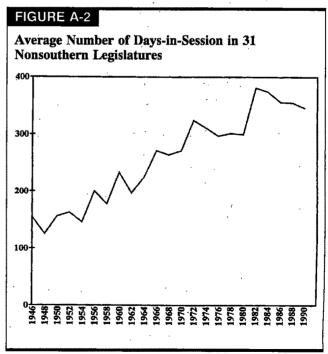
Figure A-1 shows the percentage of non-southern states holding annual legislative sessions doubling between World War II and the mid-1960s and doubling again by 1980.

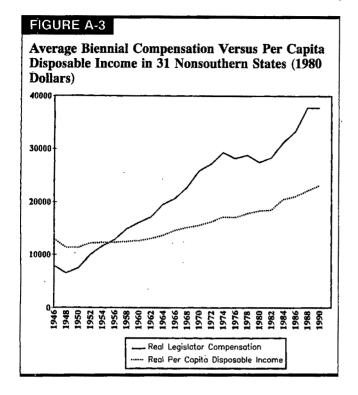
Figure A-2 shows session lengths in non-southern legislatures increasing steadily between World War II and 1980. Currently such legislatures sit for an aver-

age of 350 days during each two-year inter-election period.

Figure A-3 shows that legislative compensation was lower than per capita income in the early postwar period, rose steadily in the 1950s and 1960s, plateaued in the 1970s, and then resumed its increase in the 1980s.







Notes

This research has been supported by grants from the National Science Foundation (SES-91-12648) and the Joseph B. Grossman Fund. I wish to thank James Alt, Neal Beck, Derek Bok, Alan Ehrenhalt, Jay Greene, Ronald Hedlund, Gary King, Keith Krehbiel, Karl Kurtz, Roger Noll, Theda Skocpol, James Q. Wilson, and Mark Zupan for helpful comments.

1. Karl Kurtz (personal communication) has suggested that I differentiate professionalization from a Professionalization reform movement. The former refers to ongoing developments (however caused) that make state legislatures more like the Congress, whereas the latter refers specifically to good-government reform efforts, especially those of the 1960s.

2. Nebraska has nonpartisan legislative elections, Minnesota was nonpartisan before 1972, and Alaska and Hawaii had their first elections as states (1958 and 1959, respectively) after a significant part of the action in Figure 1 had already occurred.

3. On the well-known socioeconomic differences in the mass bases of the parties, see Miller and Traugott (1989, tbls. 2.4, 2.8, 2.18). On differences in party elites (convention delegates) see, e.g., Stanley and Niemi 1990, tbl. 4-10). More recently, Black and Black report that 45% of the delegates to the 1992 Democratic convention were employed by government in some capacity (1992, 5). (Unfortunately, they do not report a figure for Republicans, but it is undoubtedly lower.) I have been somewhat surprised by the scarcity of comprehensive data on occupational characteristics of party elites. In one case, for example, excellent data exist but are not reported separately by party (Bazar 1987).

4. Keefe and Ogul observe that for lawyers, "political involvement may well bring an unearned increment by attracting, through publicity and social visibility, new clients and higher fees" (1993, 140). Interestingly, Bazar reports a sharp drop in the number of lawyer legislators between 1976 and 1986, citing "the time requirements of legislative office," stringent disclosure laws, and the legalization of advertising as contributing factors (1987, 3).

5. There is nothing novel about this argument. Classic references include Aristotle, who took note of the claim that the Athenian law courts deteriorated after pay for service attracted a more "common" kind of men (Fritz 1964, 97–98),

and J. S. Mill, who opposed parliamentary compensation on grounds that it would discourage successful professionals and attract "adventurers of a low class" (Acton 1972, 311). Indeed, the standard argument in favor of decent legislative compensation is that in its absence, legislative service is the privilege of the rich, who presumably are not representative of the entire polity.

6. There is an obvious synthesis of the two arguments: desiring full-time careers in government, Democrats work to professionalize state and local offices. Although that argument is beyond my scope here, I shall offer some brief

remarks.

7. Francis (1993) has shown that representatives in highpaying state legislatures are significantly more likely than those in low-paying legislatures to wait for open seats rather than challenge incumbent senators. This finding suggests too that career decisions do depend on mundane wages and hours considerations.

8. The Citizens Council on State Legislatures has published numerous reports advocating the professionalization of state legislatures. The seminal articles relating professionalization to public policy are by Grumm (1970, 1971). Hedlund (1984) provides an exhaustive review of studies that relate professionalization to legislative organization and policymaking.

ing.

9. Obviously, legislatures that meet annually spend more days in session, but the correlation is far from perfect.

10. A third possible omission is allowances for travel, postage, and so forth which are usually expressed as formulas rather than payments actually made. If such allowances are reasonable and members are honest in claiming them, no understatement of compensation will arise from this source. If not, an additional income source will be overlooked.

11. The aggregate statewide vote for lower House Democrats jumped from 46% in 1968 to 54% in 1970 and fell slightly to 53% in 1972. The 1972 gain in seats with no gain in votes over 1970 is consistent with the gerrymandering hypothesis. The 7% jump in votes between 1968 and 1972 (even while McGovern was drawing little more than a third of the statewide vote) is consistent with an improvement (decline) in the quality of Democratic (Republican) candidates.

12. As far as I can determine, the legislature had been "fudging" somewhat by recessing at the end of the odd-year session and meeting for a time in the even years. In the intervening 1969–70 legislature, they sat for 414 days by

extending the session into the even year.

13. Interestingly, a contemporary observer (Asher 1978) speculated that these developments would have unintended consequences. Asher cautioned that professionalization in Ohio would lead to insulation of incumbents, as was apparently occurring in the U.S. House of Representatives.

14. Lest some prejudiced readers dismiss this finding as a reflection of the stereotypical materialism of Californians, I point out that the analogous regression estimated for Wisconsin, purported to be one of our more altruistic states, yields a coefficient (.022) precisely *twice* as strong as California's.

coefficient (.022) precisely twice as strong as California's.

15. According to Stimson, "Where unit effects are present and concern for estimator efficiency is not paramount (e.g. when estimating from very large samples) Least Squares Dummy Variables is a reasonable choice" (1985, 945). Neal Beck has examined the variables used in this data set for classroom purposes and reports that they are suitable for "garden variety analysis" (personal communication).

16. There are five intercorrelated right hand side variables: time, lagged Democrats, days-in-session, annual sessions,

and real income.

17. In addition to a linear time trend, I also checked for a pro-Democratic shift coincident with the "reapportionment revolution." No such shift was found. Previous studies reported only small Democratic seat gains in the wake of *Baker* v. *Carr* (e.g., Erikson 1971). My analysis suggests that even such small gains may have been attributable not to reapportionment but to the rapid professionalization that took place in the late 1960s (see Appendix). That development was not apparent to scholars at the time.

- 18. Simon, Ostrom and Marra, (1991) found no direct effect of economic variables with presidential approval included in their equations. Before 1950, Gallup did not ask the approval item during the campaign, but if we reestimate the equation for column 1 for 1950 onwards and include Gallup approval, gross national product growth is not significant-consistent with the findings of Simon, Ostrom, and Marra.
- 19. Neither DAYS nor ANNUAL approaches significance, with real compensation in the equation. With real income omitted their coefficients are greater than twice their standard errors. Numerous estimations leave little doubt that compensation is the most important of the three (though compensation, of course, is partly a function of days in session).

20. Comparing the three postwar elections with the three most recent elections, average real legislative compensation has increased 500%, whereas average real gubernatorial com-

pensation has increased only 25%.

21. National economic conditions are r.ot, however, in contrast to Chubb's findings. Apparently, the OFFYEAR variable included in this analysis captures the decline in economic prosperity that often occurs in midterm years.

22. By the mid-1970s, congressional salaries fell to a level of six or seven times per capita income, lower than at any time since 1920 (Congressional Quarterly Weekly Report 1989). In real terms, congressional salaries declined between 1972 and 1984.

23. A confounding factor in such tests is the increasing importance of incumbency in state legislative elections (Jewell and Breaux 1988). For example, rising compensation might generate more "potential" Democratic candidates, but if in-cumbency effects grow stronger at the same time, the number of primary challengers for Democratic incumbents and general election challengers of Republican incumbents might actually decrease overall. Research clearly would have to control for incumbency as well as other obvious factors like majority status.

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RACIAL THREAT AND PARTISAN IDENTIFICATION

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ver the past three decades, as the Democratic party in the South has come to depend more heavily on black voters for its success, it has experienced a decline among white adherents. Power theory views relationships between groups as a function of their competitive positions in political, economic, and social arenas. In contexts where the threat posed by a minority group is high, the dominant group's response is predicted to be more hostile than in contexts where that threat is low. A pooled time series analysis of voter registration data for Louisiana parishes for 1975–90 provides support for the operation of the threat mechanism. Higher black concentrations are associated with declines in the percentage of white registered voters who are Democrats and an increase in the percentage who are Republicans. Consistent with the expectations of power theory, this relationship is conditioned by the social status of the parish.

The past three decades have witnessed two dramatic trends in southern politics. First, beginning with the Voting Rights Act of 1965, there has been an extraordinary increase in political participation among southern blacks. This mobilization has been directed almost entirely to the Democratic party. Second, commencing at about the same time, a steep decline has occurred in the identification of southern whites with the Democratic party. This decline has been accompanied by a limited growth in Republican identification in the region and a large increase in the number of southern whites who identify as Independents. The end product of these trends has been twofold. On the one hand, the once solidly Democratic South no longer can be said to have a majority party (Black and Black 1987, 232). On the other hand, the Democratic party that once championed racial segregation in the region has come increasingly to depend upon black voters for its success and, thus, to reflect a more multiracial set of policy preferences.

Several authors have suggested that there is a relationship between the rise of black influence within the Democratic party and the decline of that party's fortunes among white voters (Black and Black 1987; Carmines and Stimson 1989; Huckfeldt and Kohfeld 1989; Lamis 1988, 299; Parent 1988, 209; Sundquist 1983, 400-403; for an opposing view, see Beck 1977; Stanley 1987; Stanley and Castle 1988).1 One source of theoretical support for such a linkage can be found in the *power theory* of intergroup relations (Giles and Evans 1986).² This approach views relationships between groups as a function of their competitive positions in political, economic, and social arenas. This competition is conceptualized as contextually conditioned. In contexts where the threat posed by a minority group is high, the dominant group's response is predicted to be more hostile than in contexts where that threat is low. The operation of such a dynamic in American race relations has been supported by research linking the level of black concentration in an area to the level of various forms of white hostility; lynching (Corzine, Creech,

and Corzine 1983), resistance to school desegregation (Giles, Cataldo, and Gatlin 1975; Pettigrew 1957), attitudinal support for racial integration (Fossett and Kiecolt 1989), and voting for racist candidates (Black 1973; Wright 1977).

Of the advocates for a linkage between black influence and white decline in the Democratic party, the argument of Huckfeldt and Kohfeld (1989) most closely parallels that of the power approach. They argue that the defection of white Democrats is a direct response to the threat that black voters pose to white hegemony within the party, and they conceptualize this black threat as contextually specific (pp. 46-47, 106). And, "the severity of racial incompatibility within the [Democratic] coalition, and the attendant unraveling of the coalition, are dependent upon the relative size and intersection of racial groups in particular settings" (p. 41). Moreover, they view the process of threat and defection as self-accelerating. As whites defect from the Democratic party, the party becomes more heavily dependent upon black support which in turn leads more whites to defect: "Racially polarized politics is not a simple function of individually based racist impulses. It is also the product of the progressive transformation of party coalitions that is consequent of these antagonisms" (p. 106). Huckfeldt and Kohfeld find support for their thesis in the 1984 presidential election where the percentage of whites voting for the Democratic candidate decreased as the percentage of blacks in the state's coalition increased (1989, 52).

The power approach leads us to expect that members of the dominant group situated in higher-threat contexts will respond more negatively toward the minority than will those situated in lower-threat contexts. However, since the extent to which members of the dominant group compete with the minority varies, the effect of minority threat may not be constant across all members of the dominant group even under similar contextual circumstances (Giles and Evans 1986). Those dominant group members most directly in competition with the minority are expected to be most sensitive to the level of threat

that it presents to dominant group power in a particular context. Since blacks are more likely to be employed in lower-status unskilled and semiskilled occupations, whites in similar occupations are more directly threatened by black advances. Moreover, lower-status whites also are more likely to compete with blacks for housing and for control of public facilities, such as schools and parks (Giles and Evans 1985; Labovitz and Hagedorn 1975). Thus, the power approach suggests that white reactions to the growth in influence of blacks within the Democratic coalition is conditioned by social status. Lower-status whites are expected to be more sensitive than higher-status whites to this threat and thus more likely to leave the party. Consistent with this expectation, Huckfeldt and Kohfeld found that the negative relationship between the percentage of whites voting for the Democratic candidate in 1984 and the percentage of a state's Democratic coalition that was black was stronger among low-income whites than among highincome whites (1989, 49).

The relationship between social class and Democratic support, however, may not be as straightforward as suggested by power theory. Black threat constitutes a force pushing lower-class whites toward exiting the Democratic party but there has been little to pull this group toward the Republicans (Huckfeldt and Kohfeld 1989). With the exception of their appeals to fundamentalists, the Republicans have not actively recruited support from among white lower classes. They have instead built their electoral coalition from the apex of the the social structure downward (Black and Black 1987, 249, 275; Sundquist 1983, 364-67). This may be particularly true for Louisiana in the 1980s, where Parent sees the Republican party as a coalition of white-collar, affluent suburban whites (1988, 216). Thus even if lower-status whites are more sensitive to black threat, this may be counteracted by a perception of the Republican party as not responsive to their interests. The final result could be that lower-status whites remain in the Democratic party despite their concerns or that they exit to become Independents rather than Republicans.

Racial sensitivity within a dominant group also may not be constant across time. Events occurring either within or outside of the local context may cause the salience of race-and hence the response of whites to racial threat-to vary. One such event in recent political history was the presidential candidacy of Jesse Jackson. Huckfeldt and Kohfeld argue that the candidacy of Jesse Jackson "could not help but increase the salience of race as a dividing line between Democrats and Republicans" (1989, 108). This theme was reiterated by Tyrone Brooks, a black Georgia state legislator, "I believe Jackson's candidacy may in fact contribute to the further alienation of conservative whites and Democrats who will go and vote Republican in the General Election" (quoted in Lamis 1988, 299). Thus, we would expect a surge of white defections from the Democratic party during the 1984 and 1988 presidential campaigns.

The present study reexamines the relationship be-

tween black political threat and the decline in white support for the Democratic party. Adopting the power approach, we hypothesize that as blacks constitute a larger component of a given political context, defection of white voters from the Democratic party will increase. Moreover, we hypothesize that this relationship will be conditioned by white social class and will vary temporally with the salience of race in the political arena.

MODELING DEFECTION

Official voter registration data for all 64 Louisiana parishes (counties) in the time period from 1975 through 1990 are employed to examine these hypotheses. Given its contextual nature, testing the power hypothesis necessitates information on the partisan identification of whites within areal units. Voter registration data in Louisiana are particularly well suited for this purpose. Since 1975, the state of Louisiana has provided data on the number of registered voters in each parish broken down by race and party. Louisiana is one of only two states to provide such information and the only state for which such data are available on an annual basis. Louisiana

Louisiana is also an interesting case because of its adoption of an "open" primary system in 1975 (Lamis 1988, 112–13; Parent 1988, 212–13). Under this system candidates from all parties run in the same primary. The candidate who receives a majority of the votes cast is elected to office. If no candidate receives a majority, then a general election is held between the top two candidates. Unlike a "closed" primary, the Louisiana system does not penalize registration as an Independent nor registration as a member of a perpetual minority party. For example, a voter so disposed can register as a Republican and still participate in the choice between two Democratic candidates for the governorship.

The adoption of the open primary in 1975 makes that year the reasonable starting point for examining the effect of black threat on growth in Republican registration. Black registration increased dramatically in Louisiana after the 1965 Voting Rights Act, growing from approximately 240,000 in 1966 to approximately 400,000 in 1975. This growth constituted an increased threat to white hegemony within the Democratic party but, given the monopoly that the Democratic party maintained over office holding, the closed primary meant that registering as an Independent or a Republican essentially removed the voter from effective participation in electoral politics. As a result, Republican registration increased prior to 1975, but the growth in absolute numbers was fairly small from approximately 20,000 in 1966 to slightly more than 50,000 in 1975. The principle response of whites to increasing black threat during this period was not to desert the Democratic party but to countermobilize. Thus, while the percentage growth in the registration of black voters exceeded that of whites between 1966 and 1975 (42% and 25%, respectively), the growth of white registrants in absolute terms was twice that of black registrants (361,000 and 177,000). It is noteworthy that the tendency of whites to mobilize in response to black threat continued in the period under study. Between 1975 and 1990, parishes with higher black populations were characterized by a higher percentage of registration among the white voting-age population. The higher the black concentration in the population, the more likely eligible whites were to register to vote.⁵

Most studies focusing on changes in partisan identification have relied either on survey responses or electoral results. Both of these are susceptible to the short-term factors that surround elections and so may indicate more volatility among the electorate than actually exists (Brodsky 1988). Voter registration, which requires an expenditure of time and effort by the individual, would appear to be less susceptible to such short-term factors. On the other hand, the requirement of effort, as well as strategic considerations, suggests that some voters will maintain official registration with one party while voting for, and attitudinally identifying with, another. Absent major strategic considerations, like the closed primary, the pressure for cognitive consistency should motivate such individuals eventually to alter their registration to match their voting behavior and attitudes. This movement toward consistency, however, need not be rapid or complete. Thus, official voter registration statistics probably present a conservative estimate of voter change and thus a conservative test of the hypotheses. This conservative bias is to some extent offset by the richness of the voter registration data for Louisiana, which provide more detailed longitudinal information than is available through the use of either surveys or election results.

White defection from the Democratic party can take two forms: movement to Independent identification and movement to Republican identification. These are measured straightforwardly as the percentage of white voters registered as Independent and Republican in a parish for a given year. Since both the relationship between these two forms of defection and the relation of each to black threat is uncertain, they will be treated as separate dependent variables in the analysis.

One possible measure of racial threat to white political hegemony is the percentage of the total Democratic registration that is black. This provides a precise measure of the dependence of the Democratic party on black voters and thus of the racial threat to white control of the party. There are two potential problems with employing this operationalization in the present study. The first is that an arithmetic relationship exists between racial threat operationalized in this manner and the dependent variable of interest.6 Whites may be leaving the Democratic party for a variety of nonracial reasons (e.g., more conservative positions regarding defense policy), rather than because of higher black concentrations in the Democratic party, and these departures, by reducing the Democratic base (i.e., the denominator)

necessarily results in a higher percentage of blacks within the Democratic party. Simply put, the causal flow may be from growth in Independent and Republican identification to increases in the percentage of blacks within the Democratic party, rather than vice versa, as hypothesized here.

The second problem with using this operationalization is that it requires a presumption that whites are aware of and sensitive to the racial balance among registered voters. While the relevant context for assessing a contextual hypothesis is often problematic, it would seem more likely that whites would be aware of and sensitive to the racial balance in the population at large than among voter registrants.

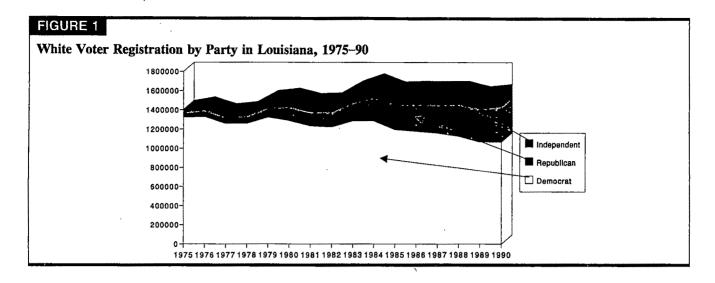
Given these concerns, racial threat is operationalized in the present study as the percentage of the voting-age population in each parish that is black. Conceptually, this is an indicator of the potential threat that blacks pose to white political hegemony in each parish generally and within the Democratic party specifically. Empirically, this measure of potential threat is strongly linked (r = .82) to the actual percentage of registered Democrats in each parish who are black. The lack of a perfect relationship between these two measures probably reflects the operation of the arithmetic process noted, as well as social and political processes relevant to mobilization (e.g., parish differences in socioeconomics, the presence of voter registration organizations).⁷

Since population data are only available through the census, interpolation was employed to estimate the size of the total and black voting eligible populations in the years between 1970, 1980 and 1990. In the analysis that follows, racial threat is modeled as having a one-year lagged effect. This allows for a reasonable delay between recognition of the level of racial threat and the response of registering as an Independent or Republican.

As noted, considerable research has found racial concentration to be perceived as a threat by southern whites. Much of this research, however, focused on earlier time periods. It might be argued that after prolonged experience with blacks as voters, whites no longer perceive higher concentrations of potential black voters as a threat. The evidence for Louisiana indicates that this is not the case. White electoral support for David Duke, an avowed racist, in his candidacy for the U.S. Senate in 1990 and the governorship of Louisiana in 1991 was positively linked to the percentage of registered voters who were black (Giles and Buckner 1993). The greater the concentration of blacks among the registered voters in a parish, the greater the percentage of registered whites who voted for Duke. Thus, the evidence supports the operationalization of racial threat in terms of the level of black concentration.

The influence of the Jackson candidacy is captured by two dummy variables. The first is scored 1 for 1983 and 1984 and 0 for the remaining years, and the second is scored 1 for 1987 and 1988 and 0 for the remaining years.

The social status of whites within the parishes is



measured using white median household income as reported in the 1980 census. The decision was made to use "household" rather than "family" income since the former more closely taps the social structure present in the living quarters of individuals. Since the principal focus is on lower-class whites, information was also collected on the percentage of whites below the poverty level in the parishes. As might be expected, this indicator is strongly related to median income (r = -.85). Accordingly, only median income is employed in the following analysis.

Quite apart from the racial threat in a particular parish, growth in Independent and Republican identification may reflect statewide and national trends. This factor is taken into account by the inclusion of a simple "counter" variable set to 0 in 1975 and incremented by 1 in each year to 1990. Likewise, while voter registration is expected to be more stable than responses to party identification items on surveys, it cannot be assumed that registration is completely immune to the short-term factors associated with elections and, in particular, may reflect differentials in voter registration efforts by parties. To allow for such effects, a dummy variable is constructed that is 1 in presidential election years and 0 otherwise.

ANALYSIS

Figure 1 presents the changes between 1975 and 1990 in Democratic, Republican, and Independent registration among white voters in Louisiana. In 1975, less than 4% of Louisiana voters were registered as Republicans. By 1990, that number had grown to approximately 23%. Likewise, Independent identification increased from approximately 2% to approximately 9% among white registrants. Coincident to the increase in Republican and Independent registrants, the number of Democratic registrants declined not only as a percentage of white registrants but in terms of absolute numbers, as well. In 1975, roughly 1.3 million whites registered as Democrats in Louisiana. By 1990, that number had declined to less that

1.1 million. However, overall white registration increased during this 15-year period, from 1.4 million to slightly less than 1.6 million. These data suggest not only that registrants defected from the Democratic party but also that new white registration disproportionately favored Independents and Republicans. Thus, voter registration among whites in Louisiana tracks reasonably well the declining fortunes of the Democratic party and the rise of Republicans and Independents chronicled by various observers of southern politics.

Is this pattern linked to the level of racial threat in the context? This question is examined at the parish level in Table 1. Since these are pooled time series data (64 parishes over 16 years), a cross-sectionally correlated and timewise autoregressive approach was used to estimate the effect parameters of our model. The ordinary least squares approach to pooled time series employing dummy variables assumes that the cross-sectional units are independent (i.e., homoscedastic) (Stimson 1985). This assumption is unlikely to be true when the pooled units are geographic areas, such as parishes (Kmenta 1986). The approach adopted here not only assumes heteroscedasticity but also assumes mutual correlation of error terms and autoregression.

Table 1, equation 1, presents the effects of the independent variables on the percentage of whites registering as Independent. Consistent with expectations, the year counter is statistically significant, indicating that the level of Independent registration among whites has generally increased over the time period under examination. The model predicts an increase of roughly 6% over the 16 years of the study (16 * .0038 = .0608). Likewise, higher median incomes in parishes are associated with higher Independent identification. An increase of \$3,500 in median income (the standard deviation for income) yields an estimated increase of roughly 1% in Independent identification. Contrary to expectations, increases in white registration as Independent do not seem to have been influenced by the presence of a presidential election. However, the presence of Jack-

TABLE 1 Effect of the Independent Variables on the Percentage of Whites Registered as Independent, Republican, and Democrat (%)

	INDEPI	ENDENT	REPUI	BLICAN	DEMOCRATIC	
INDEP. VARIABLES	EQUATION 1	EQUATION 2	EQUATION 3	EQUATION 4	EQUATION 5	EQUATION 6
% black (lagged) ^a Year count Election year Jackson 83/84 Jackson 87/88 Median income (in thousands) Med. Income × % black (lagged)	.0126 .0038*** .0002 .0049*** .0033***	.1161* .0038*** .0002 .0050*** .0034*** .0047***	.0907*** .0107*** .0069*** 0033*** 0060***	.4066*** .0109*** .0069*** 0033*** 0060*** .0083***	0932*** 0148*** 0072*** 0022** .0027***	3433*** 0149*** 0072*** 0021** .0027*** 0112***
Constant Buse R ²	0330** .597 ^b	0573** .593	0676*** .854	1445*** .861	1.1043 .853	1.1795*** .864

Note: Unstandardized regression coefficients are estimated using Shazam, Version 6.2. See n. 10 for details.

son in a presidential campaign does appear to increase racial sensitivities and result in slightly increased white registration as Independent. Most importantly, however, these data provide no support for the white backlash hypotheses. There appears to be no simple relationship between the black concentration in the parish population and the percentage of whites that register as Independent.

Table 1, equation 3, presents the effects of the independent variables on the percentage of whites registered as Republicans. Consistent with expectations, the level of black concentration within the voting-age population is positively linked to the percentage of white voters in a parish registered as Republicans. An increase of 10% in the black concentration is predicted to result in an increase of approximately 1% in white Republican registration (.10 * .0907 = .009). While this effect is not substantively large, it is consistent with the idea that Republican growth reflects, in part, a response to racial threat within the local context.

Also consistent with expectations, the year counter indicates an increase in Republican registration across the time period: An approximate 1% increment in Republican registration is estimated to occur with the passage of each year. Unlike the case for Independent registration, white registration as Republican increases on average about one-half of 1% in presidential election years. Apparently, the stimulus toward registration in presidential years carries with it a partisan valence. As expected, parishes with higher median incomes tend to have higher percentages of white voters registered as Republicans. It is interesting to note that the effect of this variable on the level

of Republican identification is essentially the same as its effect on Independent identification.

The major departure from expectations is the effect of the Jackson campaigns. While Jackson's candidacy is significantly associated with the percentage of whites registering as Republicans, the direction of the effect is opposite to that predicted. The percentage of whites registering as Republicans was expected to be higher during Jackson's campaigns in response to heightened racial sensitivity. However, with the effects of the other variables in the model taken into account, the period of the Jackson campaigns is associated with somewhat lower predicted white Republican registration. It should be emphasized that this does not mean that the level of Republican white registration would be lower in absolute terms but rather that the upward trajectory flattened out somewhat during these periods. Alternatively, one could say that the spike in white Republican registration associated with election years is somewhat attenuated during the elections in which Jackson was a candidate.

Table 1, equation 5 presents the effects of the independent variables on the percentage of whites registered as Democrats. Within rounding, these results are a summation of equations 1 and 3. Equation 5 basically shows the negative effect that nearly all of the independent variables had on the percentage of whites registering as Democrats, while equations 1 and 3 show where those defections went. For Democrats, every passing year yields a further decline in the share of registered whites. This decline is higher in election years and increases with the median income of the white population in the parish. Jack-

 a_{ϕ}^{a} black = (the black voting-age population)/(the total voting-age population) lagged one year buse R^{2} is analogous to the normal R^{2} applied to the transformed data.

^{*.05} **.01

^{***.001}

N = 960 (64 parishes/15 years)

son's first presidential bid accentuated this election year decline, but his second campaign had a positive effect on the Democratic share of white registrants. Again, this does not mean that the actual percentage of whites registering as Democrats increased in this time period. The negative effect of the election year variable is more than twice that of the positive effect of the second Jackson candidacy. Thus the decline in Democratic registration among whites was only somewhat lessened during Jackson's second campaign. While these data are inconclusive as to whether the Jackson candidacy "pushed" whites away from the Democratic party, the negative signs of the Jackson variables in equation 3 make it clear that, at least in the short term, there was no "pull" toward the Republican party as a result of his candidacy.

Based on the power model, we hypothesized that the effects of racial threat would be conditioned by social status, with threat having greater effect among lower-status whites than among higher-status whites. This hypothesis is examined by fitting a multiplicative interaction term between white median income and % black in the population. The results of this analysis are reported in equation 2 for Independents, equation 4 for Republicans and equation 6 for Democrats. For both Independents and Republicans, the interaction term is negative and statistically significant. Substantively, this result is consistent with the expectations of the power model. The positive effect of racial threat on the growth of Independent and Republican registration is diminished as the median white income of the parish increases. The positive interaction effect in equation 6 means that the decline in white Democratic registration arising with increases in racial threat is attenuated within higher white income contexts. 10

The model fitted to this point has assumed that the effect of racial threat remained constant during the study period. This assumption is examined in Table 2, where the effects of the percentage of blacks and median income are estimated in biannual cross-sections from 1976 through 1990. Since the remaining variables considered in the original model vary only temporally, their effects cannot be estimated in this analysis. The effects of the percentage of blacks are estimated separately for parishes above and below the mean for white income. This allows the interaction effect between black and white income identified in Table 1 to be presented in a more easily interpretable form.

The results in Table 2 for white registration as Independent do not suggest any patterned shift during the study period in the salience of racial threat. For all the cross-sections, the effect of the percentage of blacks on the percentage of whites registering as Independent is not statistically significant. Using pooled time series in Table 1, the effect of the percentage of blacks on the percentage of whites registering as Independent appeared to be conditioned by white income. This is reflected in the relatively stronger effect that percentage of blacks has on independent registration among lower-income parishes than

among higher-income parishes in Table 2. These differences are simply not of sufficient magnitude to obtain statistical significance when decomposed into cross sections.

In contrast, the results in Table 2 for the effect of the percentage of blacks on the percentage of whites registered as Republicans illustrate the conditioning influence of white income and also reveal a patterned shift in the salience of racial threat. First, consistent with the findings in Table 1 and with the expectations of the power hypothesis for every cross section, the effect of the percentage of blacks is higher in lowwhite-income parishes than in high-white-income parishes. Indeed, in not one cross section does the effect of racial concentration in the voting-eligible population achieve statistical significance in the highincome parishes. Second, while the effects of the percentage of blacks within lower-income parishes are only statistically significant beginning with the 1982 cross section, from 1976 through 1990, the magnitude of the effects of the percentage of blacks on the percentage of Republicans increases. Indeed, between 1980 and 1990, the effect of the percentage of blacks on the percentage of Republicans almost triples. In 1980, a 10-point increase in the percentage of the voting-eligible population who were black is predicted to increase the percentage of whites registered as Republicans by approximately .85 percentage points. In 1990, that same increase is linked to a growth of 2.26 points in the percentage of white Republicans. As required, the results for Democratic registration are the mirror image of those for Republican registration. Thus, during the study period, racial threat to white political control increasingly became an important consideration in the growth of Republican registration and the decline of Democratic registration among whites in Louisiana.

DISCUSSION

The issue of a linkage between the rise of black influence within the Democratic party and the decline of Democratic identification among whites has been debated in both the popular media and the scholarly press. The presence of such a linkage is consistent with power theory, which views relations among groups as a function of their competitive positions in political, economic, and social arenas. Generally, this approach predicts that the willingness of a dominant group to remain in a particular context declines as its hegemony is threatened by increases in the power of the nondominant group. Thus, as blacks constitute a larger element of the Democratic coalition in a particular context, the power approach predicts that whites will increasingly exit the party. Moreover, this approach suggests that the response among whites will be conditioned by their vulnerability to competition from blacks. Thus, lower-status whites are predicted to be more sensitive than are higher-status whites to the threat posed by higher black concentrations.

The results of the present study provide support

TABLE 2

Cross-sectional Examination of the Interactive Effects of the Percentage of Blacks and Median Income on Partisan Registration

	COEFFICIEN	TS FOR INDEPENDENT	VARIABLES	
DEPENDENT VARIABLES YEAR AND PARTY	PERCENT BLACK IN LOWER INCOME PARISHES	PERCENT BLACK IN HIGHER INCOME PARISHES	MEDIAN INCOME	R-SQUARE
1976		,		· ·
Republican	.0086	0258	.0034**	.13
Independent	.0101	0178	.0023**	.13
Democrat	–.0189	.0437	−.0057**	14
1978	,			
Republican	.0115 `	0335	.0040**	.14
Independent	.0134	0125	.0028**	.14
Democrat	0249	.0460	0068**	.15
1980		* -	-	
Republican	.0850#	0085	.0057**	.12
Independent	.0437	.0079	.0047***	.23
Democrat	12 87#	.0007	0103**	.17
1982				
Republican	.1109*	.0064	.0063**	.14
Independent	.0328	.0108	.0049***	.26
Democrat	- 1436	0172	. - .0111**	.18
1984		,		
Republican	.1546**	.0065	.0091**	.18
Independent	.0298	.0088	.0058***	.29
Democrat	1844 *	0153	0149***	22
1986		· · ·		
Republican	.1810**	.0018	.0106**	.19
Independent	.0280	.0021	.0056***	19
Democrat	−.2090*	0039	0162***	.22
1988				
Republican	.2086**	.0111	.0115**	.19
Independent	.0263	.0016	.0049***	.24
Democrat	−.2349 *	0127	0165***	.21
1990	ž			
Republican	.2264**	.0144	.0119**	.19
Independent	.0294	.0048	.0046**	.23
Democrat	2558**	0192	0165***	.20

Note: The ordinary least squares regression coefficients are derived from fitting the following model to the dependent variables:

%REP or %IND or %DEM = $a + b_1$ (median income) + b_2 (L × %black) + b_3 (H × %black)

where L = 1 where the parish median income is below average median income, 0 otherwise, and H = 1 where the parish median income is above average median income, 0 otherwise. This allows the effect of the percentage of blacks to be estimated separately for parishes above and below the average median income. There are 29 parishes in the Higher Income group and 35 in the lower.

for the operation of a linkage between increases in black threat and growth in Republican registration. Between 1976 and 1990, Republican registration grew dramatically in Louisiana. At the parish level this increase was found to be related to the black concentration within the voting-age population. The larger the percentage that blacks constituted of the votingage population in a parish, the larger the increase in the percentage of whites registering as Republican. Moreover, this linkage increased steadily in magnitude during the study period. In sum, these data suggest that the increasing influence of blacks within the Democratic coalition was a factor in the growth of Republican registration in Louisiana and that its importance increased through the 1980s.

The impact of racial threat was also found to be conditioned by white social status as predicted by power theory. Indeed, the relationship identified between black threat and growth in Republican registration was confined almost entirely among parishes with lower white median incomes. Among parishes with higher white median incomes, the level of black concentration appears to be unrelated to growth in Republican registration. These results, combined with the positive relationship between median white income and Republican registration,

 $^{\#}v \leq .10.$

 $p \le .05$. $p \le .05$. $p \le .01$.

^{***} $p \le .001$

N = 64 Parishes per year

suggest that Republican growth is benefiting from a complex relationship between race and class. Republicans are gaining registration among both higher- and lower-status whites. Among higher-status whites, this shift toward Republican registration probably reflects traditional conservative issue appeals. Among lower-status whites, the shift toward Republican registration appears to reflect concerns about black influence within the Democratic party. Consistent with the argument of Huckfeldt and Kohfeld (1989), the end result of the operation of racial threat among lower-status whites is that the class cleavage between the Democratic and Republican parties is diminished.

These concerns of the lower-status whites should not be interpreted simply as racism, although the willingness of a large number of white Louisiana voters to support David Duke in his campaign for governor suggests that the operation of racism cannot be dismissed. Available evidence indicates that, in fact, blacks have become a larger component of the Democratic coalition, Democratic politicians at both the federal (Whitby, 1987; Whitby and Gilliam, 1991)¹² and state (Herring, 1990) levels have become more sensitive to black political demands. Thus, whites who exit the Democratic party in response to its increasingly black makeup may be seen as exiting an organization that in reality is less sensitive to their interests.

While providing clear support for the operation of racial threat in the decline of white support for the Democratic party, the results of this analysis also indicate that other factors played a role in this decline. Across the time period examined, the growth of Independent registration was found to be unrelated to the level of black concentration within the Democratic party. Thus, the movement of Democratic registrants into the Independent category does not appear to have been linked to the threat of black influence within the Democratic party. Moreover, the significant effects of the trend and the election-year variables indicate that general forces outside of the parish context were at work in stimulating growth in Republican and Independent registration. The magnitude of the effects for these variables indicates that at least on average, their effects were more important than those of racial threat. However, it should be remembered that registration data provide a conser-

TABLE A-				
Descriptive (N = 64)	Statistics	for	Principal	Variables

VARIABLE	MEAN	S.D.	MINI- MUM	MAXI- MUM
1975				
% black	.28	.13	.04	.62
% white Republicans	.02	.02	.00	.11
% white Independents	.01	.01	.00	.05
% white Democrats	.96	.03	.86	1.00
1980				
% black	.27	.13	.03	.59
% white Republicans	.06	.04	.02	.19
% white Independents	.05	.03	.01	.11
% white Democrats	.88	.07	.72	.98
1985				
% black	.28	.13	.04	.58
% white Republicans	.12	.06	.03	.28
% white Independents	.07	.03	.02	.14
% white Democrats	.81	.09	.59	.95
1990				
% black	.28	.13	.04	.59
% white Republicans	.18	.08	.07	.36
% white Independents	.08	.03	.02	.14
% white Democrats	.74	.10	.50	.90

vative test of the threat hypothesis and may well underestimate the effects of racial concentration.

APPENDIX

Table A-1 shows descriptive statistics for selected years for the dependent variables and the percentage of the population that is black.

Table A-2 shows the correlations among the principal dependent and independent variables employed in the analysis.

Notes

We would like to acknowledge the assistance of our colleagues Alan Abramowitz, Merle Black, and Courtney Brown, who commented on earlier versions of this manuscript.

1. These authors vary in the importance which they would attach to racial antagonism in producing defection. For

TABLE A-2		•				
Correlation Matrix for Pooled Data (N = 960)						
	WHITE INC.	% BLACK	% WHITE REP.	% WHITE IND.	% WHITE DEM.	YEARCOUNT
White income	1.0000					******
% black	02650	1.0000	. —			
% white Republicans	.17462	.11889	1.0000			******
% white Independents	.36857	.04975	.78275	1.0000		***************************************
% white Democrats	24895	10163	97772	89596	1.0000	********
Yearcounter	.00000	00110	.67003	.54735	66281	1.0000

example, Black and Black see white defection as a complex process of which racial antagonism is only one component, a view with which we agree (1987, 271).

- 2. The power approach has also been referred to as the "conflict approach" or "competitive ethnicity" (see Blalock 1967; Blumer 1958; Schermerhorn 1956; Wilson 1973).
- 3. Louisiana provided total registration by race and by party for each parish prior to 1975, but party registration was not provided by race. Georgia, North Carolina, and South Carolina provide voter registration data by race, but it is not broken down by party.
- 4. Registration reports are actually available on a quarterly basis, although only the final quarter reports issued in December of each year are employed in this analysis. Florida provides voter registration reports by race and party every two years during primary and general elections.
- 5. The effects of the percentage of blacks in the population on the percentage of eligible whites who registered was estimated controlling for (1) white median income, (2) whether Jesse Jackson was campaigning, (3) whether it was an election year, and (4) a trend counter for time. The coefficient for the percentage of blacks was .1315, which was statistically significant at .05.
- 6. The percentage of blacks within the Democratic party is BD/(BD + WD), where BD equals the number of black Democrats and WD equals the number of white Democrats. The dependent variable is the percentage of whites who are Republican, or WR/(WD + WR + WI), where WR equals the number of white Republicans and WI equals the number of white independents. As WD declines, ceteris paribus, the percentage of blacks in the Democratic party must increase.
- 7. The analysis was replicated using the percentage of registered Democrats in each parish who are black as the measure of racial threat. While the magnitudes of coefficients varied, the overall pattern of results was not substantially different from that reported here using the percentage of blacks in the population.
- 8. The trend counter as a surrogate for a general pattern over time has been used by the Bureau of Labor Statistics and econometricians alike (see Maddala 1977, 344-46).
- 9. The estimation techniques used assume that we have a cross-sectionally correlated and timewise autoregressive model. We therefore assume that the cross sections (the parishes) are not independent over time. The solution to this problem has been suggested by Kmenta (1971, 512-14). This model assumes that there is heteroscedasticity, mutual correlation, and autoregression. The procedure to attain efficient estimates involves transforming the variance-covariance matrix with respect to parishes, over time. This has a similar effect to removing the mean over-time value of the variables from each parish.

This is done as a multistep procedure that first involves transforming the residuals with respect to time. This removes the problem of autoregressive behavior. These residuals are then used in an ordinary least squares regression to calculate the regression residuals. The regression residuals are then transformed to remove cross-sectional correlation and create consistent estimates for variance and covariance. These estimates are then used in calculating the parameter estimates.

- 10. The significant interaction between median income and % black might be serving simply as a proxy for a nonlinear relationship between median income and the dependent variables. However, as seen in the Appendix, white median income and the percentage of blacks are not statistically related in these data, making this unlikely. Moreover, when median income squared (a nonlinear term) was added to equations 2, 4, 6, its coefficient was not statistically significant, while the interaction term between median income and % black remained statistically significant. Similar results were obtained when % black squared was added to these equations.
- 11. In addition to the analysis in Table 2, a multiplicative interaction term between the year counter and % black was added to equations 1, 3, and 5 (Table 1). This term was statistically significant for Republican and Democratic registration but not for Independent registration. While patterns

appearing in a pooled cross section may not appear in an analysis of the component cross sections or may only be approximated (Stimson 1985), the results in Table 2 appear to capture the patterns of the pool cross section reasonably well.

12. Some studies have found no relationship between the racial concentration in the district and liberal voting. Whitby and Gilliam (1991) provide evidence that racial composition did have such an effect in a transitional period but that by the 1980s the contribution of black voters to the party extended their influence beyond specific contexts, with the result being a generally greater liberalness among southern Democrats not correlated to racial concentration.

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THE STRATEGIC ROLE OF PARTY IDEOLOGY WHEN VOTERS ARE UNCERTAIN ABOUT HOW THE ECONOMY WORKS

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continuum of voters, indexed by income, have preferences over economic outcomes. Two political parties each represent the interests of given constituencies of voters: the rich and the poor. Parties/candidates put forth policies—for instance, tax policy, where taxes finance a public good. Voters are uncertain about the theory of the economy, the function that maps policies into economic outcomes. Parties argue, as well, for theories of the economy. Each voter has a prior probability distribution over possible theories of the economy; after parties announce their theories of the economy, each voter constructs an a posteriori distribution over such theories. Suppose that voters are unsure how efficiently the government converts tax revenues into the public good. Under reasonable assumptions the party representing the rich argues that the government is very inefficient and the party representing the poor argues the opposite. What appear as liberal and conservative ideological views emerge as simply good strategies in the electoral game.

'n the classical voting model, candidates compete for voters by proposing policies; each voter has a preference ordering over policies and votes for the candidate who proposes the policy preferred. I shall suggest that electoral competition between candidates takes the form of trying to influence voters' beliefs concerning the efficacy of different policies. I shall remain classical in assuming that voters' preferences over economic outcomes are fixed.1 Consider this model. Each voter has a preference ordering over economic outcomes—for instance, over the size of his own income and of public goods provided by the government in an outcome. Candidates can announce not outcomes but only policies as part of their platforms. A voter must have a theory of the economy, which associates to any policy an economic outcome in order to decide between policies.2 Voters are uncertain about what the true theory of the economy is. Candidates compete by announcing not only a policy but also a theory of the economy. By announcing (and arguing for) different theories of the economy, candidates attempt to influence the beliefs of voters in such a way that the candidates' goals are met.

For example, suppose there are two political parties, which represent the interests of two income groups in the population: the poor and the rich. The electoral issue is tax policy: incomes will be taxed to finance a public good. The public good is in large part an income transfer from the rich to the poor. The economic parameter about which voters are uncertain is the effort-supply elasticity of workers with respect to the tax rate.³ (Thus, tax rates will affect labor supply, which in turn will affect the amount of tax revenue raised and of public good provided.) The party of the rich wants as little of the public good as possible, and the party of the poor, the opposite. A theory of the economy consists in a function specifying the effort-supply elasticity schedule for the population as a function of the tax rate. In actual politics, we observe that the rich party usually proposes a low

tax rate and a theory saying the effort-supply elasticities are negative and large in absolute value, while the poor party proposes a high tax rate and a theory saying the effort supply is tax-inelastic.

I shall show that these different views of how the economy works (i.e., different "theories of the economy") will be put forth in electoral equilibrium by parties whose sole principle is to maximize the expected utility of a particular income class—that is to say, these apparently ideological views emerge for reasons that are entirely strategic and have nothing to do with principled disagreements concerning how the economy works. (For example, the penultimate model shows that even if the right and left parties agree about what the effort-supply elasticity of citizens is, in an electoral equilibrium with malleable voters, the right party will propose a theory saying that effort-supply elasticities are large and negative, while the left party will say that effort supply is relatively inelastic with respect to the tax rate.)4 This result implies that based on the observation that parties appear to have principled differences about how the economy works we cannot reject the hypothesis that they are purely class parties, in the sense that they have no ideological preconceptions except to pursue particular class interests. The theory of the economy announced by a party may be purely a strategic matter.

Suppose there are two parties, each representing a different segment of the population (e.g., the rich and the poor). These parties have policy preferences, so that the policy outcome of an election (not just whether it wins) matters to the party. Suppose that voters are uncertain about how the economy works and that they are influenced in their beliefs, to varying extents, by announcements that candidates make about how the economy works. Each candidate (or party) announces, as its strategy in the voting game, a policy and a theory of the economy. Can we

characterize the policies and theories of the economy that parties put forth in equilibrium?

The basic elements needed to formalize this model are as follows. I assume there is a continuum of voters, indexed by s in a sample space S, and distributed according to a probability measure F on S. (Thus, for a measurable subset S^* of S, $F(S^*)$ is the fraction of the population that lies in S^* .) In the particular examples that I shall study, s is the real wage that a voter earns, or the voter's "skill level." There is a set B of possible policies, with generic element denoted b: these will be income tax rates. There is a sample space *T* of theories of the economy, with generic element t. A voter s is characterized by a family of preference orderings $v(\cdot, s, t)$ on B and a measure $\Phi(\cdot, s)$ on T, with density $\varphi(t, s)$. Agent s's preference ordering over policies is $v(\cdot, s, t)$ if the true theory of the economy is t. Think of v as an indirect utility function, derived as follows. For any policy b there is an economic outcome that will result if the true theory of the economy is t and v records voter s's utility in that outcome. The simplest case to study is the one where B, T, and S are intervals of real numbers and for each t and s, the preferences $v(\cdot, s)$ t) are single-peaked: this, indeed, will be the case in the examples. The probability measure $\Phi(\cdot, s)$ summarizes voter s's prior beliefs concerning the theory of the economy; that is, for a measurable subset T^* of T, $\Phi(T^*, s)$ is the probability that s assigns to the event that the true theory of the economy is some t in T^* .

There are two political parties, each of which represents diligently the interests of a constituency of voters. (I depart from the more common assumption that parties/candidates are Downsian, that is, are simply interested in winning elections. There is a literature where candidates/parties have preferences over policies; see, e.g. Alesina 1988; Calvert 1985; Hansson and Stuart 1984; Roemer 1992, 1993; Wittman 1977, 1990.) I shall assume that the parties have representative constituents s_1 and s_2 . These two voters might be the median voters in larger coalitions of voters, each of which is a party's constituency. A party's goal is to maximize the (expected) utility of its representative constituent.⁵

 \tilde{I} must assume something about the beliefs of parties concerning the theory of the economy: I think the most interesting case to examine is one in which both parties know (or believe they know) the true theory and agree on what that theory is (call it t_0). I shall assume this to be the case.

Parties can influence the beliefs of voters when they propose theories of the economy. Under the assumption just made, they dissemble when they propose theories other than t_0 . Party i's platform consists of a policy—theory pair, (b^i, t^i) . The essential part of the model is the description of the effects that the proposed party platforms have on the beliefs of voters concerning the true theory of the economy. I shall suppose that when a voter is presented with two theories t^1 and t^2 by the parties, the voter's prior beliefs $\Phi(\cdot, s)$ become altered into posterior beliefs, $\Phi^{\rm ex}(\cdot, s)$. That is to say, there is a mapping Ψ that

associates to any quadruple (t^1, t^2, φ, s) , where $t^1, t^2 \in T$, φ is a probability density on T, and s is a voter, a probability density $\varphi^{\rm ex}$ on T. Ψ is a primitive datum in this model: it describes the effect of the parties' announcements of a pair of theories of the economy on a voter's beliefs concerning the true theory.

In recent work, Gerber and Lupia have made Ψ endogenous, assuming that voters are rational (Gerber and Lupia 1992; Lupia 1992). They study how voters use information about candidate attributes to decide whether to believe announcements that candidates make about the effects of policies. Similarly, Austen-Smith (1990) models a rational inference process of legislators who try to evaluate the information that other legislators put forth in debate about the efficacy of their proposed policies. I take the inference process summarized by Ψ as a datum but restrict Ψ in only a fairly mild way.

As an example, consider the mapping Ψ that acts as follows. Let T be an interval of real numbers. If the parties propose theories t^1 and t^2 , then any voter s with prior Φ revises the probability distribution by assigning probability zero to the event $T \setminus [t^1, t^2]$ and renormalizes the prior on the interval $[t^1, t^2]$. Formally, if $t^1 < t^2$, then for all s,

$$\Psi(t^1, t^2, \varphi, s) = \begin{cases} 0, & \text{if } t \text{ is not in } [t^1, t^2] \\ \varphi(t) / \int_{t^1}^{t^2} \varphi(t) \, dt, & \text{if } t \in [t^1, t^2]. \end{cases}$$
 (1)

If $t^1=t^2$, then the posterior measure is the unit mass at the common value t^1 . Let us denote the posterior probability measure on T of voter s, when faced with the parties' theory proposals t^i , $\Phi^{\rm ex}(\cdot,s;t^1,t^2)$. The associated density function, if there is one, is $\varphi^{\rm ex}(t,s;t^1,t^2)$. In words, under the inference process described by Ψ , every voter concludes that any theory of the economy t not in the interval $[t_1,t_2]$ has probability zero of being true and simply renormalizes the old density function on the new support $[t_1,t_2]$. I do not assert that such an inference process is reasonable; this Ψ is given only as an example to clarify the idea.

I shall make only one assumption about this inference process:

Assumption 1. (Monotonicity). The mean of $\varphi^{ex}(t, s; t^1, t^2)$ is a nondecreasing function of t^i , for i = 1, 2.

Assumption 1 says that if either party *increases* the parameter that it announces characterizing the theory of the economy, then no voter will *decrease* his or her mean belief about the value of that parameter. Assumption 1, in particular, permits voters *not to change their beliefs* about the unknown parameter when parties change their announcements about its value. Thus, a poor voter can ignore the announcement that the rich party makes: this is permitted by assumption 1. The reader can verify that the inference process Ψ of equation 1 satisfies assumption 1.

A party's objective, as I said, is to maximize the expected utility of its representative constituent. To

formulate this objective, we must first describe how to calculate the probability that a party wins the election, given the two platforms (b^i, t^i) , i = 1, 2. Given a posterior distribution $\Phi^{\rm ex}$, a voter s evaluates a policy b according to its expected utility, $\int_T v(b, s, t) \varphi^{\rm ex}(t, s; t^1, t^2) dt$. Thus, we define the set of voters who prefer b^1 and b^2 when facing the platforms (b^i, t^i) , i = 1, 2 as

$$\begin{split} A(b^1,\,t^1,\,b^2,\,t^2) &\equiv \left\{ s | \int_T v(b^1,\,s,\,t) \varphi^{\rm ex}(t,\,s;\,t^1,\,t^2) \;dt \right. \\ \\ & \left. > \int_T v(b^2,\,s,\,t) \varphi^{\rm ex}(t,\,s;\,t^1,\,t^2) \;dt \right\}. \end{split}$$

Policy b^1 defeats b^2 if and only if $F(A(b^1, t^1, b^2, t^2)) > 1/2$ —that is, when the mass of voters who prefer one platform to another constitutes a majority of the population. We may therefore define the function $P(b^1, t^1, b^2, t^2)$ —the probability that b^1 defeats b^2 given the two platforms t^1 and t^2 —as

$$P(b^1, t^1, b^2, t^2) = \begin{cases} 0 & \text{if} \quad F(A(b^1, t^1, b^2, t^2)) < \frac{1}{2} \\ \frac{1}{2} & \text{if} \quad F(A(b^1, t^1, b^2, t^2)) = \frac{1}{2} \\ 1 & \text{if} \quad F(A(b^1, t^1, b^2, t^2)) > \frac{1}{2}. \end{cases}$$

I have assumed that parties attempt to maximize the expected utility of their representative constituent, where parties evaluate expected utility according to what they believe to be the true theory of the economy, t_0 . Thus, the payoff to party i, when the platforms $\{(b^1, t^1), (b^2, t^2)\}$ have been proposed is defined as

$$\Pi((b^1, t^1), (b^2, t^2), i) = P(b^1, t^1, b^2, t^2)v(b^1, s_i, t_0)$$

$$+ (1 - P(b^1, t^1, b^2, t^2))v(b^2, s_i, t_0).$$

This just says that the payoff to the party representing s_i when the parties propose platforms (b^1, t^1) and (b^2, t^2) is

(Probability 1st Party Wins

- \times Actual Utility to Voter s_i under party 1's Policy)
- + (Probability 2d Party Wins
- \times Actual Utility to Voter s_i under Party 2's Policy).

By actual utility, I mean the utility that the voter gets under what parties think is the true theory (t_0) of the economy.

I am now ready to define an electoral equilibrium for this model, as follows:

DEFINITION 1. For an environment (F, S, T, B, v, $\{\Phi(\cdot, s)\}$ s \in S, Ψ , s_1 , s_2 , t_0) an electoral equilibrium with malleable voters is a pair of platforms $(\overline{b}^I, \overline{t}^I)$, $(\overline{b}^2, \overline{t}^I)$ such that each platform is a best response to the other party's platform; that is,

$$(\overline{b^1}, \overline{t^1}) \in argmax \Pi(b^1, t^1), (\overline{b^2}, \overline{t^2}), 1)$$

and

$$(\overline{b^2}, \overline{t^2}) \in argmax \Pi((\overline{b^1}, \overline{t^1}), (b^2, t^2), 2).$$

That is, each party's platform is a best response to the platform of the other party, where each party's payoff function is the expected utility of its representative constituent under the true theory of the economy. This equilibrium notion is a variant of Bayesian Nash equilibrium.

I must emphasize that in this model, the parties know all the data specifying the politicoeconomic environment, so that there is no uncertainty concerning the outcome of the election, except in the case when the probability of victory at equilibrium is just 1/2. (In particular, although voters are uncertain about how the economy works, parties know exactly what voter's beliefs are and hence can calculate what the outcome of the elections will be.) The model is therefore too simple to reflect a reality in which the outcomes of elections are uncertain: that uncertainty would obtain only if parties did not know everything about voters' beliefs and preferences. I have elected to assume that parties know everything about voters in order not to bring in an issue that would complicate the model a great deal, namely, party uncertainty about voters' beliefs or preferences. (Readers who are interested in seeing a model in which parties are uncertain about voter preferences might consult Roemer 1993.)

A MEDIAN VOTER THEOREM

There is a median voter result for this model: at an electoral equilibrium with malleable voters, both parties will propose the same policy, although they will generally propose different theories of the economy. I emphasize that the first fact is due to the assumption that parties have complete information about voters' preferences and beliefs. If parties had incomplete information about voters' characteristics, they would announce different policies, as well, in equilibrium. The general fact is that if parties both (1) have policy preferences and (2) are uncertain about voter preferences or beliefs, then the electoral equilibrium involves the parties putting forth different policies (see Roemer 1993). If one introduces only one of these two assumptions into the classical Downsian model, however, a median voter equilibrium still attains. (For a proof of this claim, see Roemer 1994.) The present model assumes condition 1 but not condition 2, for reasons of simplicity only.

I shall, for the moment, fix the theories (\vec{t}^1, \vec{t}^2) arbitrarily and define what the equilibrium concept would be if parties competed only with policies as strategies in the electoral game. The expected utility of voter s, given the proposals for theories of the economy (\vec{t}^1, \vec{t}^2) is

$$\tilde{v}(b, s) = \int_{T} v(b, s, t) \varphi^{\text{ex}}(t, s; \overline{t^{1}}, \overline{t^{2}}) dt.$$

I now consider the electoral game between two parties (who complete only over policies) for voters whose preferences over policies are given by the functions $\tilde{v}(b, s)$. The probability that a policy b^1 defeats b^2 is given by

$$\bar{P}(b^{1}, b^{2}) = \begin{cases}
0 & \text{if } F(A(b^{1}, \overline{t^{1}}, b^{2}, \overline{t^{2}})) < \frac{1}{2} \\
\frac{1}{2} & \text{if } F(A(b^{1}, \overline{t^{1}}, b^{2}, \overline{t^{2}})) = \frac{1}{2} \\
1 & \text{if } F(A(b^{1}, \overline{t^{1}}, b^{2}, \overline{t^{2}})) > \frac{1}{2}.
\end{cases} (2)$$

The payoff function for party i is given by

$$\tilde{\Pi}(b^1, b^2, i) = \tilde{P}(b^1, b^2)v(b^1, s_i, t_0) + (1 - \tilde{P}(b^1, b^2))v(b^2, s_i, t_0).$$
(3)

We have the ingredients to define an electoral equilibrium of this "nested" model as follows:8

DEFINITION 2. Fix (t^{I}, t^{Z}) . For an environment $(F, \tilde{v}, v, B, S, \Phi^{ex}, s_1, s_2, t_0)$, a partial electoral equilibrium at (t^{I}, t^{Z}) is a pair of platforms (b^{I}, b^{Z}) such that each platform is a best response to the other party's platform; that is,

$$\overline{b^1} \in argmax \tilde{\Pi}(b^1, \overline{b^2}, 1)$$

and

$$\overline{b^2} \in argmax \tilde{\Pi}(\overline{b^1}, b^2, 2).$$

I now make four assumptions:

Assumption 2. $\tilde{\mathbf{v}}(b, s)$ is continuous and strictly concave in b. 9

Assumption 3. $\tilde{\mathbf{v}}(\mathbf{b}, \mathbf{s})$ is continuous in $\tilde{\mathbf{s}}$.

Assumption 4. $\hat{b}(s) = argmax \tilde{v}(b, s)$ is strictly monotone decreasing in s.

Assumption 5. F is absolutely continuous with respect to Lebesgue measure.

We have, under these assumptions, the classical median voter result:

LEMMA 1. (Classical Median Voter Theorem). Let assumptions 1-4 hold. Then $\hat{b}(s^m)$, where s^m is the median of F, defeats all other policies by majority vote.

(For a proof, see Roemer 1994, lemma 2.1. Note that lemma 1 does not assert that the parties put forth $\hat{b}(s^m)$ as their policies in equilibrium.)

I next make a further assumption:

Assumption 6. The function \tilde{v} has a single crossing property on $B \times S$, i.e., for any distinct pair b^1 , $b^2 \in B$, there is at most one s such that $\tilde{v}(b^1, s) = \tilde{v}(b^2, s)$.

Next, define

$$\underline{b} = \underset{b}{\operatorname{argmax}} v(b, s_1, t_0).$$

$$b = \underset{b}{\operatorname{argmax}} v(b, s_2, t_0).$$

I shall abbreviate $\hat{b}(s^m)$ as \hat{b}^m . \bar{b} and \underline{b} are the policies that maximize the "true interests" of constituents s_2 and s_1 , respectively, while \hat{b}^m is the perceived favorite policy of the median voter, given his probability density $\varphi^{\rm ex}$ over theories of the economy. We can now state

Theorem 1. Fix $(\overline{t_1}, \overline{t_2})$ and let assumptions 2-6 hold. Let $s_1 < s^m < s_2$. If $\overline{b} < \hat{b}^m < b$, then (\hat{b}^m, \hat{b}^m) is the unique partial electoral equilibrium at $(\overline{t_1}, \overline{t_2})$. 10

(For proof, see the Appendix.) Theorem 1 asserts that once the parties have decided upon the theories of the economy they shall propound, then both parties announce the median voter's ideal policy in the unique electoral equilibrium. I reiterate that theorem 1 is not the classical Downsian result; for in this article, parties are not trying to maximize the probability of getting elected, as Downsian parties are: they are trying to maximize the expected utility of their constituents. It is this different objective that requires us to employ an assumption like the single crossing property to get the median voter result.

Note that any full electoral equilibrium with malleable voters $\{(\overline{b}^1, \overline{t}^1), (\overline{b}^2, \overline{t}^2)\}$ is a partial electoral equilibrium at $(\overline{t}^1, \overline{t}^2)$. We can therefore use theorem 1 to study the nature of electoral equilibrium with malleable voters in the examples that follow.

Finally, I remark that the statement of theorem 1 remains unchanged if we drop the assumption that the two parties agree on what the true theory of the economy is. If party i believes the true theory is t_i^* , we need only redefine \underline{b} , \overline{b} , and $\Pi(\cdot, \cdot, \cdot, i)$ in the appropriate way, and the proof of theorem 1 as stated remains the same.

PARTY COMPETITION OVER THE EFFICIENCY OF GOVERNMENT

I shall specify economic environments where I can be explicit about the uncertainty that voters have about how the economy works and can therefore study explicitly the nature of the platforms that parties will put forth in the equilibrium of definition 2. The result that will be used to analyze these political economies is theorem 1.

I consider an economy in which voters differ only with respect to their "skill" (or the real wage they earn) and their prior beliefs concerning the efficiency of government. The voters decide the uniform rate at which income is to be taxed. Taxes finance a public good. What voters are uncertain about is the efficiency with which the government converts tax revenue into the public good. A theory of the economy consists of a value of this conversion parameter.

Let there be a continuum of voters, indexed by real-wage levels $s \in [s, \bar{s}] = S$, where s is distributed according to a probability density function f(s) on $[s, \bar{s}]$. Each voter has preferences given by

$$u(y, G, L) = y + G - \frac{d}{2}L^2,$$
 (4)

where y is consumption of the private good (the real-wage good), G is consumption of the public good, and L is labor expended. If B is total tax revenue, then the government's production function is

$$tB = G$$

where t is the *efficiency of government* (the conversion factor of taxes into the public good). Voters of type s have prior beliefs about the value of t specified by a probability density function $\varphi(t; s)$ on the sample space $T = [t, \bar{t}]$, some real interval.

space $T = [t, \bar{t}]$, some real interval. Given proposals t^1 and t^2 for the theory of economy by the two parties and a voter with a prior $\varphi(t)$, the posteriori beliefs of the voter are given by the density function

$$\varphi^{\text{ex}}(t, s; t^1, t^2) = \Psi(t^1, t^2, \varphi, s),$$

where Ψ is the voter's inference process.

Assumption 1 is central to the argument that follows; it can be justified in either of two ways. The first would attempt to deduce that assumption 1 characterizes the behavior of rational voters, who, using the information they have about candidates/parties, update their beliefs about how the economy works after parties' announcements are made. (This is the spirit of Lupia 1992 and Austen-Smith 1990.)¹¹ The second would be to say that voters are not fully sophisticated (i.e., rational) in the sense that they do not take full account of the fact that parties are behaving strategically in announcing theories of the economy. Hence, they are influenced by what parties say when, perhaps, they should not be.

As I have said, I do not here propose a complete theory of the process of voter inference. Nevertheless, I believe that assumption 1 is a plausible restriction on that inference process when voters are confronted with politicians who announce theories of how the economy works. Notice that what assumption 1 does is to compare a voter's beliefs about how the economy works when the pair of announcements of theories by the candidates changes from (t^1, t^2) to, say, t^1, t^{2*}). Assumption 1 says that if $t^{2*} > t^2$, then no voter would revise his mean belief about the true value of t downward (from what he would have believed if the announcements had been (t^1, t^2)). If such a change were to occur in party 2's announcement in real politics, then the "experts" associated with party 2 and the media who support party 2 would argue that t^{2*} is the true theory of the economy. The experts and media supporting party 1 would continue to argue that t^1 is the true theory. Thus, the sources of information to which the voter is exposed would maintain, on average, that the true value of t is higher than if party announcements had been the pair (t^1, t^2) . It is not unreasonable to suppose that this influences voter beliefs about t in an upward direction—or, more precisely, as assumption 1 says,

no voter would revise a belief about t downward in the face of this change in party 2's announcement.

I next describe the concept of *economic equilibrium* at a tax rate b for voters with posteriori beliefs given by $\varphi^{\text{ex}}(t; s)$.

DEFINITION 3. An economic equilibrium at tax rate b for voters with posteriori beliefs $\varphi^{ex}(t; s)$ concerning the theory of the economy and with preferences given by equation 4 is a function $G(\cdot, b) : T \to \Re_+$ and a function $L(\cdot, b) : S \to \Re_+$ such that

1.
$$L(s, b) = argmax \int_{T} ((1 - b)sL + G(t, b) - \frac{d}{2} L^{2}) \varphi^{ex}(t; s) dt$$
 for all $s \in S$;

2.
$$t \int_{S} bsL(s, b)f(s) ds = G(t, b)$$
 for all $t \in T$.

This first expression says that L(s, b) is the optimal labor supply for a type s agent with preferences given by equation 4 who believes that G(t, b) will be the value of the public good if t is the true theory of the economy. The agent in the expression is choosing labor supply to maximize expected utility, with the expectation taken over the probability distribution that expresses the agent's beliefs about what the true theory of the economy is.

The second expression says that if, for all s, agent s supplies labor L(s, b) and the true efficiency of the government is t, then, indeed, G(t, b) will be the value of the public good.

The equilibrium of definition 3 is a rational expectations equilibrium, in the sense that the labor supplied by agents—given (1) their posteriori beliefs about how the economy works, (2) that the tax rate is b, and (3) their projections about the amount of the public good that will be supplied under the various possible values of the efficiency of government that voters believe possible—will indeed generate precisely the levels of government spending that they believe to be forthcoming at possible values of the efficiency of government. Thus, in such an equilibrium, no agent has good reason to revise beliefs. To put it another way, there is no reason for any voter to update beliefs about the efficiency of government in such an equilibrium.

I shall show that for any $b \in [0, 1]$, there is a unique economic equilibrium à la definition 3. Thus a voter evaluates his *utility at tax rate b* as his (expected) utility in that equilibrium, defined as

$$v(b, s; t^{1}, t^{2}) = (1 - b)sL(s, b)$$

$$+ \int_{T} G(t, b)\varphi^{ex}(t; s, t^{1}, t^{2}) dt - \frac{d}{2}L(s, b)^{2}.$$
 (5)

I include (t^1, t^2) as arguments of v to remind us that the economic equilibrium is calculated for given pos-

teriori beliefs φ^{ex} , which are induced by particular party proposals (t^1, t^2) .

I can now apply theorem 1 to characterize political economic equilibrium in the present model. The parties of the left and the right represent given voters $s_1 < s_m$ and $s_2 > s_m$, respectively, where s_m is the median of the distribution f(s).¹² The political—economic equilibrium will consist of platforms (\hat{b}^m, t^1) and (\hat{b}^m, t^2) , where \hat{b}^m is the optimal policy for the median voter, whose preferences are given by equation 5. In what follows, I shall study the values of t^1 and t^2 that the parties will propose in equilibrium. And to do this, I must calculate the median voter's favorite policy as a function of t^1 and t^2 .

To this end, I begin by calculating the functions L(s,b) and G(t, b). Differentiating the expression in statement 1 of definition 2 with respect to L gives L(s, b) =(1 - b)s/d. (It is noteworthy that because of the quasi-linear utility function, L does not depend on $(t^1,$ t^2)—an immense simplification, which allows us to solve for the functions L(s, b) and G(t, b) explicitly.) Plugging the expression for L into statement 2 of definition 3 gives

$$G(t, b) = \frac{tb(1-b)}{d} \mu_2(f),$$

where

$$\mu_2(f) \equiv \int_S s^2 f(s) \ ds$$

is the second moment of f. Plugging the expressions for G(t, b) and L(s, b) into equation 5 gives

$$v(b, s; t^{1}, t^{2}) = \frac{(1-b)^{2}s^{2}}{2d} + \frac{b(1-b)}{d} \mu_{2}(f)\mu_{1}(\varphi^{\text{ex}}(t, s)),$$
(6)

where

$$\mu_1(\varphi^{\mathrm{ex}}(t,s)) \equiv \int_T t \varphi^{\mathrm{ex}}(t;s) dt$$

is the mean of $\varphi^{\text{ex}}(\cdot, s)$.

Next I shall make a new assumption:

Assumption 7.
$$s_m^2 < 2\mu_2(f)\mu_1(\varphi^{ex}(t; s_m))$$
.

If the mean of $\varphi^{\text{ex}}(\cdot; s_m)$ is not too much below unity, then assumption 7 is a reasonable assumption. In any case, it is assumed because it simplifies the analysis by guaranteeing that $v(b, s_m; t^1, t^2)$ is a concave function of b, whose maximum can therefore be calculated by setting the derivative of equation 6 with respect to b equal to zero:

$$-(1-b)s_m^2 + \mu_2(f)\mu_1(\varphi^{\text{ex}}(s_m))(1-2b) = 0.$$

Thus, the optimal policy for the median voter with beliefs φ^{ex} is

$$\hat{b}_m(t^1, t^2) = \max\left(0, \frac{s_m^2 - \mu_2(f)\mu_1(\varphi^{\text{ex}}(s_m))}{s_m^2 - 2\mu_2(f)\mu_1(\varphi^{\text{ex}}(s_m))}\right). \tag{7}$$

We might make a stronger version of assumption 7: Assumption 8. $s_m^2 < \mu_2(f)\mu_1(\varphi^{ex}(t; s_m))$.

Then equation 7 implies

$$\hat{b}_m(t^1, t^2) = \frac{s_m^2 - \mu_2 \mu_1}{s_m^2 - 2\mu_2 \mu_1}.$$
 (8)

Let us review the situation. I have, thus far, fixed the announcements (t^1, t^2) and calculated the median voter's ideal tax policy, b_m , as a function of (t^1, t^2) , given by equation 7 or 8. Now think of what (t^1, t^2) must look like at political-economic equilibrium. First, note that, at the *true* theory of the economy t_0 , which, we suppose, is known by both parties, the utility of a voter s is given by

$$v(b, s; t_0) = \frac{(1-b)^2 s^2}{2d} + \frac{b(1-b)}{d} \mu_2(f) t_0.$$

(This is derived in the same way that I derived equation 6.) The corresponding ideal policy of voter s_i (and therefore party i) is given by

$$\hat{b}_i(t_0) = \max\left(0, \frac{s_i^2 - t_0 \mu_2}{s_i^2 - 2t_0 \mu_2}\right). \tag{9}$$

I shall call the corresponding value $b_s(t_0)$, for a voter s, the true optimal tax rate of voter s—true because t_0 is taken to be the true theory of the economy.

It is easy to verify from equation 9 that the true optimal tax rate for voter s is decreasing in s. Therefore, the left party, which represents a constituent with small s, would like an equilibrium where \hat{b}_m , of equation 7 or 8, is large, and the right party would like \hat{b}_m to be small. In equation 8, the parties can influence only the term $\mu_1(\varphi^{\text{ex}}(t;s_m))$ by their proposals (t^1,t^2) . Differentiating b_m with respect to μ_1 gives

$$\operatorname{sign} \frac{\partial \hat{b}_m}{\partial \mu_1} = \operatorname{sign} \ \mu_2 s_m^2 > 0,$$

and so the left party wishes to render $\mu_1(\varphi^{\text{ex}})$ large and the right party wishes to render $\mu_1(\varphi^{\text{ex}})$ small. Now invoke assumption 1, which implies that the right party will propose a low value for t and the left party will propose a high value for t; for that is how they so influence the value of $\mu_1(\varphi^{ex})$.

More precisely, we can say the following. Recall that t and t are the smallest and largest possible values for the efficiency of government. (Voters, that is, assign probability zero to the event that t lies outside the interval $[t, \bar{t}]$.) The platforms $(\hat{b}_m(t, \bar{t}), \bar{t})$ and $(\hat{b}_m(t,\bar{t}),\underline{t})$ constitute, for this model, the unique political–economic equilibrium, as long as $\hat{b}_m(t,\bar{t})$ is not greater than $\hat{b}_2(t_0)$ (the true interest of the left's constituent) and $\hat{b}_m(t,\bar{t})$ is not smaller than $\hat{b}_1(t_0)$ (the true interest of the right's constituent). I have thus shown that a strategic party of the right will argue

that government is very inefficient and that a strategic party of the left will argue that government is very efficient, in an electoral environment whose voters have malleable beliefs as specified by assumption 1.

PARTY COMPETITION OVER THE LABOR-SUPPLY ELASTICITY WITH RESPECT TO THE TAX RATE

I next consider a model where voter uncertainty concerns a parameter that determines the population's labor-supply elasticity with respect to the tax rate. Traditionally, right parties have argued that this elasticity is large and negative, while left parties have argued that it is small in absolute value. Our task is to study whether these "ideologies" are not simply optimal strategies in the electoral game between class parties.

I now assume that voter preferences are given by

$$u(y, G, L) = y + G - \left(\overline{a}L + \frac{d}{2}L^2\right).$$

Each voter s knows that the coefficient on the L term in his or her own utility function is $-\bar{a}$ but is uncertain as to the value of this coefficient for other voters. The voter believes the coefficient for all other voters is a number a distributed according to a probability density $\varphi(a;s)$ on an interval A including \bar{a} . I assume here that the government converts tax dollars into the public good without any efficiency loss; that is, if B is total tax revenue then G = B.

A voter of type s whose utility function has a coefficient a and who expects G to be the value of the public good will optimize labor supply at tax rate b, by computing

$$\underset{t}{\operatorname{argmax}} \left[(1-b)sL + G - \left(aL + \frac{d}{2}L^2\right) \right],$$

which gives an optimal labor supply of

$$\hat{L}(s, b; a) = \frac{(1-b)s - a}{d}.$$
 (10)

To simplify the argument, I assume that for the relevant values of b and for all $s \in S$ and $a \in A$, (1 - b)s > a so that everyone's labor supply is positive. From this, it follows that the elasticity of labor supply with respect to b is given by

$$\frac{\partial \hat{L}(s, b; a)}{\partial b} \cdot \frac{b}{\hat{L}} = \frac{-bs}{(1 - b)s - a}.$$
 (11)

Thus, labor-supply elasticity is indeed a function of *a*; labor-supply elasticity is high if *a* is large and low if *a* is small.

In this model, the theory of the economy concerns the value of the parameter a. Parties compete by proposing platforms (b^i, a^i) , where b^i is a tax rate and a^i is the proposed value of the parameter a. If a party wishes to argue that labor-supply elasticities are large (in absolute value), it proposes a large value of a. (See equation 11 and recall that I have assumed that the denominator in that expression is always positive for relevant b.)

We can now state the definition of economic equilibrium.

DEFINITION 4. An economic equilibrium at a tax rate b when voters' beliefs are given by densities $\varphi^{ex}(a; s)$ is a function $L(\cdot, b) : S \to \Re_+$ and a function $G(\cdot, b) : A \to \Re_+$ such that

1.
$$L(s, b) = \underset{L}{argmax} \int_{A} ((1 - b)sL + G(a, b))$$

$$-(\overline{aL} + \frac{d}{2}L^2))\varphi^{ex}(a; s) da \text{ for all } s \in S.$$

2.
$$G(a, b) = b \int_{S} s(\frac{(1-b)s-a}{d})f(s) ds$$

for all
$$a \in A$$
.

G(a,b) is what voter s thinks the value of the public good will be if all other voters supply labor according to equation 10 and if a is the true theory of the economy. The first expression here states that the actual labor supply of each voter is computed by maximizing the voter's expected utility. Thus, as I discussed after definition 3, the equilibrium of definition 4 is a rational expectations equilibrium, and when it attains, no voter has any reason to change posteriori beliefs $\varphi^{\rm ex}$ about how the economy works.

We can compute that

$$G(a, b) = \frac{b(1-b)}{d} \mu_2(f) - \frac{ab}{d} \mu_1(f),$$

where $\mu_i(f)$ is the *i*th moment of f, for i = 1, 2. It follows that voter s's expectation as to the value of the public good is

$$\int_A G(a, b)\varphi^{\text{ex}}(a; s) da = \frac{b(1-b)}{d} \mu_2(f)$$
$$-\frac{b}{d} \mu_1(f)\mu_1(\varphi^{\text{exx}}(a; s)).$$

From this, it follows that the *expected utility of voter s* at the equilibrium with tax rate b is

$$v(b, s; a_1, a_2) = (1 - b)s \left(\frac{(1 - b)s - \bar{a}}{d}\right)$$

$$-\left[\bar{a}\left(\frac{(1 - b)s - \bar{a}}{d}\right) + \frac{d}{2}\left(\frac{(1 - b)s - \bar{a}}{d}\right)^2\right]$$

$$+ \frac{b(1 - b)}{d} \mu_2(f) - \frac{b}{d} \mu_1(f)\mu_1(\varphi^{\text{ex}}(a; s)), \quad (12)$$

where, as in equations 5 and 6, I include the parties' proposals (a^1, a^2) as arguments of v, for they determine the posteriori densities $\varphi^{\text{ex}}(a; s)$.

I next make a further assumption:

Assumption 9. $s_m^2 < 2\mu_2(f)$.

This is a reasonable assumption, from which it follows that v is strictly concave in b (the coefficient on b^2 is negative) and hence the optimal policy for the median voter is got by setting $\partial v/\partial b$, which we can calculate from equation 12, equal to zero. In so doing, we get an expression for the median voter's favorite policy of

$$\hat{b}_m(a^1, a^2) = \frac{\mu_1(\varphi^{\text{ex}})\mu_1(f)}{s_m^2 - 2\mu_2(f)}$$

+ another term not involving φ^{ex} . (13)

(Here, $\varphi^{\rm ex}$ means $\varphi^{\rm ex}(a; s_m)$.) To be precise, the median voter's favorite policy is the maximum of $\hat{b}_m(a_1, a_2)$ and zero.

In equation 13, the parties can influence only $\varphi^{\rm ex}$ by their announcements of theories of the economy. This means they can influence only the first term on the right-hand side of equation 13. From assumption 9, the first term on the right-hand side of the expression for $\hat{b}_m(a_1, a_2)$ is negative, and so a low value of $\mu_1(\varphi^{\rm ex})$ is associated with a high value for \hat{b}_m . Recalling the reasoning of the efficiency-of-government section, it follows that the left party, which wants the value of \hat{b}_m to be high, therefore wants $\mu_1(\varphi^{\rm ex})$ to be low, while the right party wants $\mu_1(\varphi^{\rm ex})$ to be high. Accordingly, by assumption 1, the right party announces a high value for a, and left party, a low value for a.

But this is equivalent to saying that if assumption 1 holds, the right party will argue that the elasticity of labor supply with respect to the tax rate is high in absolute value, while the left party will argue that the elasticity is low. I have again derived apparently "ideological" views as the outcome of optimal strategic play by class-oriented parties.

CONCLUSION

I have argued that parties compete for voters not only with policy proposals but by proposing to voters different theories of how the economy works. If the parties themselves are certain about all the parameters of the political–economic environment, then there is a median voter result: in equilibrium, parties propose the same policies but different theories of the economy.

In two examples, I have studied the nature of the theories that parties put forth in equilibrium. We have seen that certain views about how the economy works that are conventionally viewed as part of the conservative and liberal *Weltanschauungen* (e.g., that government is inefficient or efficient) will in fact be put forth by partisan parties (ones entirely dedicated

to pursuing the interests of given constituents) as aspects of their election strategies, when, indeed, the parties have no such *Weltanschauungen*. Thus, we cannot reject the hypothesis that conservative-versus-liberal political competition involves no principled disagreements about how the economy works but is, simply, class politics, in the sense that conservative and liberal parties are tailoring their apparent "views" about how the economy works to maximize the expected utility of their different constituents in the electoral equilibrium.

APPENDIX: PROOF OF THEOREM 1

First, it is clear that (\hat{b}^m, \hat{b}^m) is an equilibrium: this follows from equations 2 and 3 and lemma 1.

Second, suppose (b_*, b^*) were another equilibrium. Suppose b_* and b^* are both less than \hat{b}^m ; then party s_1 does better by proposing \hat{b}^m . In like manner, b_* and b^* cannot both lie above \hat{b}^m . Therefore $b^* \leq \hat{b}^m \leq b_*$.

Third, I shall show that $b^* < \hat{b}^m < b_*$ is impossible. For suppose this inequality holds and b^* wins: this cannot be, for party s_1 can do better by proposing \hat{b}^m . Similarly, b_* cannot win; so b^* and b_* must tie.

Fourth, since some voters prefer b^* to b_* and some, vice versa, there must be an s^* such that $\tilde{v}(b^*, s^*) = \tilde{v}(b_*, s^*)$. By the single crossing property, s^* is unique, and it follows that $s^* = s^m$. For suppose not and that, say, $s^* < s^m$. Then, by the single crossing property, either $\tilde{v}(b^*, s) > \tilde{v}(b_*, s)$ for all $s > s^*$ or $\tilde{v}(b^*, s) < \tilde{v}(b_*, s)$ for all $s > s^*$. In either case, one of the policies is a majority winner, contradicting the fact that b^* and b_* tie.

Fifth, I show the following inequalities hold:

$$v(b^*, s_2, t_0) > v(b_*, s_2, t_0)$$
 (A-1)

$$v(b_*, s_1, t_0) > v(b^*, s_1, t_0).$$
 (A-2)

Suppose, to the contrary, that $v(b^*, s_2, t_0) \leq v(b_*, s_2, t_0)$. Since $b_* > \hat{b}^m > \underline{b}$, we have, by single-peakedness, $v(\hat{b}^m, s_2, t_0) > v(b_*, s_2, t_0)$. It follows from the last two inequalities that $v(b^*, s_2, t_0) < v(\hat{b}^m, s_2, t_0)$. Thus \hat{b}^m is a better reply to b_* than b^* (for party s_2), a contradiction. Thus, inequalities A-1 and A-2 hold.

Sixth, since, by step 4, s^m is indifferent between b^* and b_* , it follows that for small ε , the policy $b^* + \varepsilon$ is preferred to b_* by s^m . By step 5, inequality A-1, for small ε , s_2 also prefers $b^* + \varepsilon$ to b_* . Thus, by the single crossing property, $b^* + \varepsilon$ is preferred to b_* for all $s \ge s^m$. But for small δ , $s = s^m - \delta$ also prefers $b^* + \varepsilon$ to b_* (by assumption 3), and so a majority coalition prefers $b^* + \varepsilon$ to b_* . Thus, $b^* + \varepsilon$ defeats b_* , and it follows that $b^* + \varepsilon$ is a better response to b_* , for the s_2 party, than b^* . This contradiction establishes the impossibility of the double inequality $b^* < \hat{b}^m < b_*$.

Seventh, the boundary cases (e.g., $b^* \le \hat{b}^m < b^*$) are likewise shown to be impossible, and we conclude $b^* = b_* = \hat{b}_m$.

For completeness, I note that the Theorem 1 can be expanded to include the other possible cases, namely,

CASE A. If $b < \hat{b}^m$, then, for any $b^* \in [\underline{b}, \hat{b}^m]$, (b^*, b^*) is a partial electoral equilibrium at $(\overline{t_1}, \overline{t_2})$. These equilibria are Pareto ranked, with $(\underline{b}, \underline{b})$ being the Pareto dominating one. All other equilibria are equivalent to one of these. 14

CASE B. If $\bar{b} > \hat{b}^m$, then, for any $b^* \in [\hat{b}^m, \bar{b}]$, (b^*, b^*) is a partial electoral equilibrium at (\bar{t}_1, \bar{t}_2) . A similar statement to that in part A holds.

The proofs of these statements are available on request.

Notes

1. Austen-Smith (1990) proposes a model of legislative debate with the same basic structure: preferences of legislators are fixed, but debate can affect beliefs about the consequences of policies.

2. See Kinder and Mebane 1983 for a careful discussion of the process by which voters form theories of the economy. This is a complex topic, and I shall here make only some highly stylized assumptions about it.

3. That is, they are uncertain about the rate at which workers will reduce their labor supply as the tax rate on

income increases.

- 4. In this model, no cost is imposed on a party for announcing a theory of the economy that is false. Ordinarily, one could justify imposing such a cost if voters could learn what the true theory of the economy is. Thus, my implicit assumption is that voters never learn much about the true theory of the economy. This may be because voters must depend on expert opinion, and there always exist experts with a wide range of opinions about how the economy works. Indeed, this does not seem to be an unreasonable assumption, given our own experience.
- 5. More realistically, one might wish to say that parties maximize some weighted average of constituent interests and candidate interests. I here consider the polar case of parties as perfect agents of constituents for the sake of simplicity.
- 6. I think this case the most interesting, because even here, we shall see that parties propose very different theories of the economy to voters. If candidates' beliefs about the true theory differ, then it is not surprising that they propose different theories.
- 7. Note that the function Ψ has s has one of its arguments; therefore, each voter can be influenced differently by the party announcements. In particular, some voters can ignore the theory of the economy announced by the Left party, also.
- 8. It is nested, in the sense that I have assumed the announcements of parties about the theory of the economy are fixed, whereas in the full model they are strategies.
- 9. Strictly quasi-concave is sufficient here. This assumption's role is just to guarantee single-peakedness of preferences.
 - 10. The theorem, as stated, studies only one possible case,

in which $\bar{b} \leq \hat{b}^m \leq \underline{b}$ holds. The other possible cases are stated at the end of the Appendix.

11. Actually, Austen-Smith is concerned with the inference process of legislators, not voters generally. It is arguable, I think, that legislators, who are professionals, evaluate information from biased politicians more rationally than does the general voter.

12. I do not here propose an endogenous theory of party

formation.

- 13. I should emphasize that I have proved no general theorem in the government-efficiency and labor-supply-elasticity sections but have established the results for the postulated quasi-linear utility functions. The analysis becomes complex for utility functions without this property, as one ceases to be able to compute the economic equilibrium of the model in closed form. Were I to pursue such a generalization, I would begin with simulation. I can make no general claim, as a result of the present analysis, that parties representing poor voters will always propose that the government is efficient, and so on.
- 14. I say that two equilibria are *equivalent* if they give rise to the same observed policy, or lottery of policies.

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ECONOMIC SECURITY AND VALUE CHANGE

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onfirming Inglehart's prediction (1971) of an intergenerational shift toward postmaterialist values, a time series analysis controlling for the joint effects of inflation and unemployment demonstrates that there is a statistically significant trend toward postmaterialism in all eight West European countries for which data are available over the past two decades. Evidence from the 1981–83 and 1990–91 World Values Surveys indicates that this value shift occurs in any society that has experienced sufficient economic growth in recent decades so that the preadult experiences of younger birth cohorts were significantly more secure than those of older cohorts. Large intergenerational differences tend to be found in societies that have experienced rapid growth in gross national product per capita, and are negligible in societies that have had little or no growth. Accordingly, postmaterialism increased in 18 of the 20 societies on five continents for which we have comparable data over the past decade.

ver two decades ago, one of the authors proposed a theory of value change implying that value priorities in advanced industrial society will tend to shift away from *materialist* concerns about economic and physical security toward greater emphasis on freedom, self-expression, and the quality of life, or *postmaterialist* values (Inglehart 1971). Arguing that differences between the formative socialization of young Europeans and their elders were leading younger birth cohorts to give relatively high priority to freedom and self-expression, he suggested that future intergenerational population replacement would bring about a shift toward new value priorities.

If this shift is taking place, it may have far-reaching implications. The growth of postmaterialist values appears to have contributed to a decline of social class voting (Dalton 1991; Dalton, Flanagan, and Beck 1984; Inglehart 1977, 1990; Lipset 1981) and to the rise of new social movements (Ganzeboom and Flap 1989; Inglehart 1990; Klandermans 1990; Kriesi 1989; Offe 1990; Rohrschneider 1990), particularly environmentalist movements and parties (Bennulf and Holmberg 1990; Betz 1990; Hoffmann-Martinot 1991; Müller-Rommel 1990; Rohrschneider 1993). Changing value priorities may be reshaping the nature of political cleavages and the political meaning of left and right, giving rise to a new politics axis. This new axis cuts across the traditional left-right dimension, characterized by radical reform parties and movements at one pole and right authoritarian parties and movements like the Christian Coalition, the National Front, and the Republikaner at the other (Huber 1989; Inglehart 1977; 1990; Kitschelt 1994; Kitschelt and Hellemans 1990; Lafferty and Knutsen 1985; Minkenberg 1990). Moreover, Dalton, Flanagan, and Beck (1984) and Inglehart (1990) argue that weakening party loyalties and low voter turnout partly reflect the established parties' failure to offer meaningful choices about the new politics issues. Conversely, Crepaz (1990) has demonstrated that the presence of a Green party or

some other postmaterialist type of party enhances electoral turnout. Finally, this process of value change seems conducive to democratization throughout advanced industrial society (Gibson and Duch 1994; Inglehart 1992).

Despite this evidence, Inglehart's most basic prediction—that a trend toward postmaterialist values actually does exist—remains controversial. Earlier critics (Böltken and Jagodzinski 1985; van Deth 1983) argued that differences between the basic values of the old and the young were simply due to life-cycle effects, rather than intergenerational change. Empirical support for this position has eroded in recent years, as it became evident that given birth cohorts did *not* become more materialistic as they aged.

Since the original thesis was published, the values battery has been used in scores of countries, and more than two decades of time series evidence are now available to test it. But even in recent years, several scholars have disputed the existence of a trend toward postmaterialist values. For example, Thomassen and van Deth examine value trends in the Netherlands between 1970 and 1986; they argue that the data demonstrate "fluctuations" in values but maintain that "no dramatic shift of political values has occurred" (1989, 64). "There does seem to be a decline of materialism and an increase in post-materialism since 1982," they argue, "but it is unclear at present whether that is the dawn of the expected revolution or just a temporary fluctuation" (ibid.). Shively acknowledges that there has been value change among the West European public but argues that Inglehart's claim that this change is generationally driven is "fundamentally flawed" (1991, 237). Clarke and Dutt (1991) have advanced the farthest-reaching objections to Inglehart's thesis. They argue that there has been no significant trend toward postmaterialism in half of the countries he studied and that even where there is such a trend, this movement is driven by rising levels of unemployment. If high levels of unemployment contribute to

postmaterialism, they argue, then Inglehart's measure and/or theory is flawed, since it claims that security is conductive to postmaterialism, whereas they find that economic *adversity* promotes postmaterialist values.

We shall demonstrate that there has been a clear trend toward postmaterialism largely resulting from intergenerational population replacement. Moreover, the growth of postmaterialism has occurred in spite of, rather than because of, rising levels of unemployment. In short, the trend toward postmaterialism results from the processes that Inglehart discussed when he first advanced his theory of value change over two decades ago.

SHORT-TERM AND LONG-TERM VALUE CHANGE

Inglehart advances two hypotheses that predict changing values. He postulates a scarcity hypothesis, which states that "an individual's priorities reflect one's socioeconomic environment" and a socialization hypothesis, which postulates that "to a large extent, one's basic values reflect the conditions that prevailed during one's preadult years" (1985, 103). "The scarcity hypothesis" Inglehart writes, "implies shortterm changes, or period effects: periods of prosperity lead to increased postmaterialism, and periods of scarcity lead to materialism. The socialization hypothesis implies that long-term cohort effects also exist" (ibid). During the traumatic recessions of the mid-1970s and early 1980s, period effects were substantial. Inglehart (1985) concluded that between 1970 and 1982 period effects and cohort effects were both important, with period effects actually outweighing cohort effects.

Inglehart's theory of value change predicts that intergenerational population replacement will gradually lead to a long-term shift from materialist to postmaterialist values. It not only helps predict the *direction* of future change but also the *magnitude* of the change that one would expect from generational replacement, which is not the only factor involved but *is* a major component of value change (see Abramson and Inglehart 1986, 1987, 1992; Inglehart 1990).

Over short periods, the magnitude of change predicted on the basis of population turnover is always small. If one uses Inglehart's overall measure of values, namely, the percentage difference index (the percentage of postmaterialists minus the percentage of materialists), the model predicts a change of only about one point on the index per year. Given the small size of these predicted effects, short-term forces (or sampling error) can easily swamp the effects of population turnover, but in the long term, its cumulative effects can be massive.

For this reason, it is important to analyze as long a times series as possible. In their analysis of the Netherlands, Thomassen and van Deth study the years between 1970 and 1986, using Inglehart's fouritem measure of materialist/postmaterialist values.
Clarke and Dutt analyze only the data collected from
1976 to 1986, although they could have also analyzed
the surveys conducted in 1970, 1971, and 1973 and
possibly some of the more recent surveys, as well.³
Like Thomassen and van Deth, Clarke and Dutt
employ Inglehart's four-item value measure. Clarke
and Dutt argue that there was no trend toward
postmaterialism in Belgium, France, and Italy and
that the trend toward postmaterialism was not statistically significant in Ireland. Unlike Thomassen and
van Deth, however, Clarke and Dutt do find a significant movement toward postmaterialism in the Netherlands, as well as in West Germany, Britain, and
Denmark.

We shall analyze all the available data from 1970–71 through 1992 for West Germany,4 Britain, the Netherlands, France, Belgium, and Italy⁵ and all of the surveys conducted in Denmark and Ireland between 1973 (when surveys started in these countries) and 1992. We shall assess whether or not there is a trend toward postmaterialism using the full time series for each country. We shall also analyze the 1990-91 World Values Survey and show that rising economic security contributes to postmaterialist values in societies with widely varying institutions and cultural heritages and at various levels of economic development. Finally, we will compare data from the 1981-83 World Values Surveys and the 1990-91 World Values Surveys to demonstrate that the predicted trend toward postmaterialism took place in 18 of the 20 societies that can be compared during this decade.

LONG-TERM VALUE CHANGE

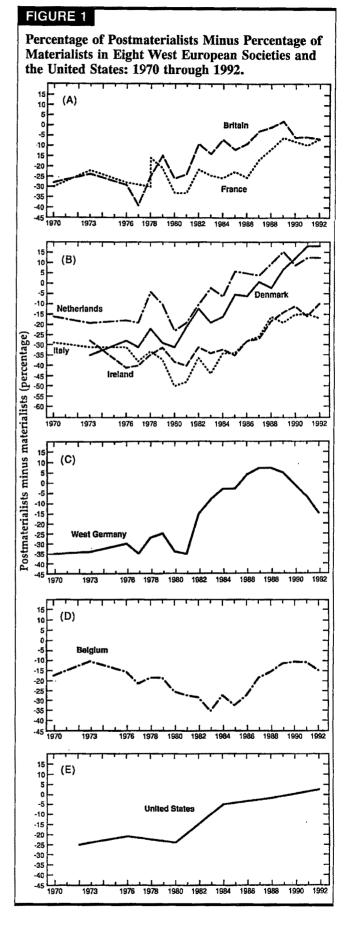
Figure 1 shows the percentage of postmaterialists minus the percentage of materialists based on the four-item index for eight West European countries from the beginning of the time series through the fall of 1992, as well as for the United States.⁶ It is apparent that there has been a trend toward postmaterialism in seven of these eight European societies, as well as the United States. Only the Belgian data fail to reveal a trend toward postmaterialist values.

Figure 1A presents the results for Britain and France. (Presenting the data for all nine countries at once would produce an unreadable graph.) Both countries show an overall trend toward a rising proportion of postmaterialists, but here (as elsewhere), this trend is punctuated by dips toward materialism that reflect the recessions of the mid-1970s and the early 1980s. There is also a downward fluctuation at the end of the series, reflecting the current global recession. This effect is noticeably more pronounced for Britain (where the latest recession has been more severe) than in France. For Britain, the shift toward postmaterialism reached its peak in 1989, when the inflation rate was 7.8% annually and unemployment was 7.0%. By early

1992, these figures had risen to 10.4% and 10.3%, respectively. In France, on the other hand, unemployment rose only slightly (from 9.4% in 1989 to 9.9% in 1992), while inflation remained at 3.7% in both years. Even though both inflation and unemployment were higher at the end of the time series than at the beginning, there was a substantial overall trend toward postmaterialist values. In Britain, for example, at the start of the series in 1970-71, materialists (36%) were five times as numerous as postmaterialists (7%), producing an index of -29 (with the remainder having mixed values). By 1992, 24% were materialists and 16% were postmaterialists, producing an index of -8: postmaterialists were now twothirds as numerous as materialists. In France, the figures changed from 41% materialists and 11% postmaterialists in 1970-71, to 27% materialists and 19% postmaterialists in 1992.

Figure 1B presents the results from four more countries, two of which had even stronger shifts from materialist to postmaterialist values than those found in Britain or France. The Netherlands, Denmark, Ireland, and Italy all show downward fluctuations linked with the global recessions of the mid-1970s and early 1980s, plus a leveling-off or downward movement linked with the latest recession at the end of the time series—but all four show clear trends from materialist toward postmaterialist values.

Although the West German data (see Figure 1C) show the same overall trend toward postmaterialism, they manifest a much larger downward shift between 1988 and 1992. After the wall came down in 1989, West Germany absorbed East Germany, a society with a political system that had collapsed and an economy in free fall. East Germany's problems became West Germany's problems, with huge sums being transferred from the west to help reconstruct Eastern Germany. Inflation rose from 1.3% in 1988 to 4.0% in 1992 and though unemployment remains fairly low in the West, it has risen to appalling levels in the East (with some estimates placing it at 30%). In addition, following the collapse of the socialist regimes in Eastern Europe, hundreds of thousands of political and economic refugees fled to Germany, giving rise to a pervasive sense of insecurity and severe xenophobia, linked with attacks on foreigners. Despite the severity of recent conditions, West Germany nevertheless shows a significant long-term shift from materialist to postmaterialist values over the past two decades. In 1970 (with much lower rates of inflation and unemployment than in 1992), 45% of the West Germans were materialists and only 10% were postmaterialists, producing a percentage difference index of -35. In 1992, the proportion of materialists had fallen to 31%, and that of postmaterialists had risen to 14%, for an index of -16. This overall upward movement of the index, in the face of strong, short-term, downward pressures, resulted largely from intergenerational population replacement processes in which younger, relatively postmaterialist birth-cohorts replaced older, relatively materialist cohorts.



Belgium is the only remaining country for which we have detailed survey data with numerous regular measurements covering a long span of years (see Fig. 1D). The Belgian time series shows no clear trend toward postmaterialist values. At the end of the series in 1992, postmaterialism among the Belgian public was only marginally higher than it was in the 1970–71 baseline surveys.

Finally, though no detailed time series is available for the United States like that produced by the Euro-Barometer's semiannual surveys, the four-item materialist/postmaterialist values battery has been included in each of the National Election Study (NES) presidential election surveys from 1972 through 1992. Figure 1E shows the results: again, we find the predicted long-term upward trend, with the ratio rising from 35% materialists and 10% postmaterialists in 1972, to 16% materialist and 19% postmaterialists in 1992.

Inglehart's thesis predicts only modest gains of about one point per year in postmaterialism, since he argues that the trend is driven by generational replacement. If generational replacement were the only factor influencing value change, the trend toward postmaterialism would be similar in all these societies, since the speed of generational replacement varies only slightly from country to country (see Abramson and Inglehart 1992). As we shall see, economic factors do influence short-term value change. Before examining the dynamics of long-term and short-term value change, it is important to illustrate that the trend toward postmaterialism is robust in the face of alternative values measures.

Inglehart has also developed a broader indicator of values based upon a choice among 12 national goals.7 As Inglehart has demonstrated (1977, 1990), this 12-choice measure is more reliable and valid than the widely employed measure based upon a choice of four goals and less sensitive to the effects of inflation. Unfortunately, the more complex index is also more time-consuming and expensive to administer and, as a consequence, has been used in relatively few national surveys. However, we can examine levels of materialism/postmaterialism using this 12-choice measure with surveys conducted in 1973 and 1990. The results from these eight European societies are presented in Table 1. These data reveal a shift toward postmaterialism in all eight countries, although the shift in Belgium is relatively modest. Across the eight countries for which data covering this 17-year period are available, the percentage scoring "high" on the materialist/postmaterialist index rose from 41% in 1973 to 54% in 1990. As Inglehart (1990) has shown, young Europeans are much more likely to score as postmaterialists on this measure than older Europeans are. Though there are not enough observations for a detailed analysis of the dynamics of value change, the trend toward postmaterialism also emerges when the more reliable 12-item measure is used.

TABLE 1

The Shift toward Postmaterialist Values: Results from the 12-Item Index in 1973 versus 1990

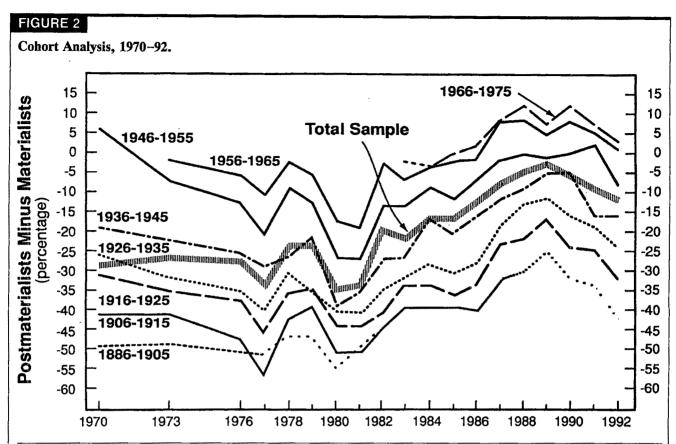
	PERCENTAGE SCORING "HIGH" ON 12-ITEM INDEX				
COUNTRY	1973	1990	SHIFT		
West Germany	25	53	+28		
Italy	34	52	+18		
Denmark	35	51	+16		
Netherlands	53	69	+16		
Britain	38	50	+12		
Ireland	38	46	+8		
France	50	58	+8		
Belgium	51	55	+4		
Mean	41.	54	+13		

Source: 1973 European Community Survey and 1990 World Values Survey. "This index is constructed by summing the scores from choosing the five postmaterialist goals, when a score of +2 is given if the given item is ranked highest in its group of four items, and a score of +1 is given if it is ranked second highest in its group. Scores on the resulting index range from 0 to 8; a "high" score is one of 4 or higher.

WHAT CAUSES LONG-TERM CHANGE?

Inglehart's thesis maintains that the trend toward postmaterialism results mainly from generational replacement. We have earlier demonstrated that replacement is a driving force behind value change (Abramson and Inglehart 1986, 1987, 1992), but we can provide additional evidence. Figure 2 presents a cohort analysis (using the four-item measure) among eight birth cohorts tracked from 1970 to 1992 using a combined weighted sample of Germany, Britain, the Netherlands, France, Belgium, and Italy.8 Vertical comparisons between birth cohorts in each year reveal that younger cohorts consistently have higher levels of postmaterialism than their elders. These relationships are not just a function of the higher educational levels of younger Europeans, although the very low educational levels of older Europeans contributes to their levels of postmaterialism.9 Following given cohorts from left to right across the figure tracks the value fluctuations of each cohort as it ages. For example, when it was first sampled, the cohort born from 1946 to 1955 was between the ages of 15 and 24. By 1992, this cohort was between the ages of 37 and 46.

There are obvious short-term fluctuations for every birth cohort. But by following given cohorts from left to right across the figure, it becomes clear that there is no overall tendency for birth cohorts to become more materialist as they age. That cohorts do not become more materialist as they age undermines a life-cycle interpretation for the relationship between age and materialism. But as Shively (1991) correctly argues, such evidence does not conclusively disprove a life-cycle interpretation. As Glenn (1977) has shown, one



Source: Based on combined weighted sample of European Community Surveys carried out in West Germany, France, Britain, Italy, the Netherlands, and Belgium, in given years, using the four-item materialist/postmaterialist values index (N = 222,699).

Note: % postmaterialists - % materialists in eight birth cohorts and for total sample in six West European societies.

can never definitely choose between a life-cycle and a generational explanation on statistical grounds alone, for in any cohort matrix the age of a cohort is a perfect function of its date of birth and the time that the survey is conducted. Even though Figure 2 demonstrates that these cohorts did not become more materialist as they aged, one could always argue that some unmeasured period effect conducive to postmaterialism suppressed true life-cycle forces that would otherwise make the cohorts more materialist as they aged. On the other hand, as Shively also notes, analysts can distinguish period effects from cohort effects and life-cycle effects if they have side information that enables them to identify any one of the three. We have two important pieces of side information: (1) we know that major economic recessions affected all six of these countries in the mid-1970s, the early 1980s, and again in the early 1990s; and (2) theoretically, these economic conditions would be expected to produce a downward shift toward materialism, since they lead to a diminished sense of economic security. This interpretation was proposed by Inglehart (1981, 1985) to account for the downward swings in postmaterialism in the mid-1970s and

the early 1980s. We now have an additional test of this interpretation: another recession occurred in the early 1990s, and we find another downward swing in the proportion of postmaterialists. If we accept the interpretation that these fluctuations in the values indicator are, indeed, due to these recessions, then we have identified the major period effects. The remaining pattern over time is essentially horizontal for each cohort, which implies that there are no important life-cycle effects and that the enduring intercohort differences reflect generational change. We find this interpretation quite plausible and think most other observers will also: it fits the original theory neatly, with each successive recession producing a shift toward materialism, followed by a resurgence of postmaterialist values when economic recovery takes place.

Even though we cannot definitively rule out the possibility that these cohorts might eventually become more materialist as they age, the overall increase in postmaterialist values that we observe here results largely from generational replacement. This can be seen by comparing the overall trend line in Figure 2 with the trend lines for the eight cohorts.

Despite substantial short-term fluctuation, given cohorts change very little from 1970-71 to 1992. But during these years the overall percentage difference index rises from -29 to -10, a 19-point gain. None of the cohorts tracked over these years registered a gain that is even close to this magnitude (they show virtually no gain at all). The rise in the overall percentage difference index occurs because during these 22 years, the older, more materialist cohorts were gradually dying and being replaced by younger, more postmaterialist cohorts. These results make clear that generational replacement is a major force driving postmaterialism upward. The net rise in postmaterialism clearly is not due to changes in inflation rates, even though the short-term fluctuations within any given cohort do track the inflation rate very closely (as Figure 3 will demonstrate), for the overall inflation rate at the end of this period was higher than at the start (6.5% in 1992, 5.2% in 1970), but the proportion of postmaterialists nevertheless reveals a long-term increase. In 1970-71, the sixnation sample examined in Figure 2 contained 40% materialists and 11% postmaterialists, with materialists outweighing postmaterialists by nearly 4:1. By Spring 1992, the figures had shifted to 27% materialists and 16% postmaterialists, for a ratio of less than 2:1. Overall gains in postmaterialism result mainly from the gradual exit of the older cohorts through death and their replacement by cohorts with higher levels of postmaterialism. Although generational replacement is gradual, over the course of two decades, it becomes substantial. According to census statistics, by the end of 1992, 40% of the adults in these six societies were too young to have been sampled in 1970. In other words, between 1970 and 1992, twofifths of the adult population had been replaced. Obviously, a theory of value change based upon intergenerational population replacement is compatible with substantial short-term variation. But just what causes that short-term variation?

WHAT CAUSES THE PERIOD EFFECTS?

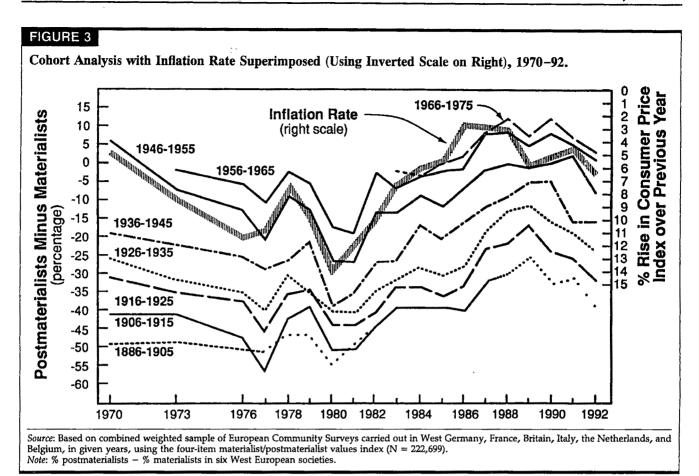
Although there definitively is a trend toward postmaterialism in seven of these eight European societies, none of them shows a simple linear progression toward postmaterialist values (as Figure 1 demonstrates). Clearly, short-term period effects are present. Inglehart's theory predicts that economic and physical insecurity will tend to depress the proportion of postmaterialists and that relatively secure conditions will be conducive to increases in postmaterialism. The four-choice value battery employed to measure materialist/postmaterialist values includes one choice that makes the measure sensitive to changing rates of inflation, since one of the four proposed national goals is "fighting rising prices." Indeed, Inglehart has demonstrated that much of the short-term variation in values results from changing

levels of inflation. Using just two variables—(1) cohort effects and (2) period effects (as measured by change in the consumer price index)—he explained 87% of the cohort-level variation in value priorities in a combined sample of six Western European publics between 1970 and 1982 (Inglehart 1985). We can now examine the relationship between cohort change and inflation rates over a much longer period.

Figure 3 presents the results of a cohort analysis of the pooled results from the six countries that have been surveyed since 1970, but we now superimpose the current rate of inflation. Since Inglehart's theory predicts that postmaterialist values will rise when inflation falls, the inflation index runs from low rates at the top of the graph to high rates toward the bottom. This makes it easy to see that (as predicted) inflation and postmaterialist values move up and down together, bearing in mind that a downward movement of the inflation line indicates rising rates of inflation.

Substantial period effects occur during these years, as even a cursory glance at Figure 3 suggests. Though the long-term tendency is for each cohort to remain about as materialist or postmaterialist as it was when it was first sampled, we find three pronounced dips in the proportion of postmaterialists; they correspond to the recessions of the mid-1970s, the early 1980s, and the early 1990s. As predicted, period effects are closely linked with the inflation level at a given time. At most time points, the percentage difference index for each cohort moves up or down with the current rate of inflation. When we control for the effects of inflation, the period effects largely disappear. But statistical controls are not needed in the present example. As it happens, by 1990, the weighted mean inflation rate for these six countries had returned to almost exactly the same level that it had in 1970. Thus, when we compare the 1970 results with the 1990 results, the main source of short-term period effects (inflation) has been controlled for by nature. With inflation thus controlled, note that the percentage difference index for these six countries rose from -29 to 1970 to -6 in 1990, a net gain of 23 points. In other words, over this 20-year period, the balance between materialists and postmaterialists shifted by 1.15 points per year—the very amount that would take place from the intergenerational population replacement process alone. In any given year, current period effects will add to, or subtract from, this amount, but this is the long-term trend. The finding that short-term change results largely from changes in inflation is clearly consistent with Inglehart's scarcity thesis. Moreover, as we shall see, in seven of the eight countries for which detailed data are available, short-term value change results mainly from changing inflation rates.

Clarke and Dutt propose an alternative source of short-term variation that is not consistent with the scarcity hypothesis. They argue that contrary to what one might expect, rising levels of unemployment are



conducive to postmaterialist values. They argue that this happens because respondents cannot choose fighting unemployment as a goal, so that when unemployment rises, they tend to choose the option of "Giving the people more say in important government decisions" to express their concern about unemployment.

The rationale for expecting such a relationship is much less straightforward than the relatively obvious linkage between high levels of inflation and the goal of fighting rising prices. But if high levels of unemployment do in fact contribute to postmaterialism, this relationship should be found at the individual level. Clarke and Dutt present some evidence from the 1980-81 wave of a panel study conducted in West Germany, the Netherlands, and the United States that they claim demonstrates this linkage. This survey includes a question about jobs, but it is ambiguous. Respondents are asked, "Which of the following problems do you consider most important?" Immediately after the item "Guaranteeing equal rights for men and women," the next choice is "Seeing to it that everyone who wants a job can have one." This item could tap either the respondent's own anxiety about becoming unemployed or concern for fair employment practices for everyone, since it explicitly refers to jobs for "everyone who wants one" and appears immediately after an item about "equal rights for men and women." In any event, this ambiguous item is

weakly related to postmaterialist values. Among those who make it the main concern, 15% are postmaterialists on the 12-choice index; among the entire 1980–81 cross section as a whole, 12% are postmaterialists.

Although this item yields weak and ambiguous results, there is considerable evidence demonstrating the opposite relationship namely, that at the individual level, concern about unemployment is conducive to *materialist* values, rather than to *postmaterialist* values. Let us briefly review this evidence.

In developing a measure of materialist/postmaterialist values in 1970, Inglehart actually *did* experiment with an item that taps concern about unemployment. He employed a four-choice battery that included the following choices:

- I will now propose a number of concrete objectives. Among the following things, which are the two that seem most desirable to you?
- 1. Guarantee greater job security
- 2. Make our society more humane
- 3. Raise salaries
- 4. Guarantee the participation of workers in the management of enterprises.

This measure was employed in five countries, in addition to the now-standard four-item battery. How these four objectives relate to postmaterialism is presented in Table 2. The table demonstrates that

Source: 1970 European Community Survey. Note: Based on four-item values index.

OBJECTIVE CHOSEN AS MOST IMPORTANT	WEST GERMANY	NETHERLANDS	FRANCE	BELGIUM	ITALY	WEIGHTED MEAN
Raise salaries	5	3	2	8	4	4
Guarantee greater job security	6	15	7	12	10	8
Overall sample	11	19	11	14	13	12
More participation by workers	12	22	12	10	26	16
Make our society more humane	26	30	22	21	22	24

respondents who emphasized job security are consistently *less* likely than the overall public to be postmaterialists (as Inglehart's theory implies).

The 1973 European Community survey provides further individual-level evidence that contradicts the thesis that concern for unemployment gives rise to postmaterialism. Respondents in eight countries were asked the following question:

Here are some things people usually take into account in relation to their work. Which one would you personally place first? . . . and which next?

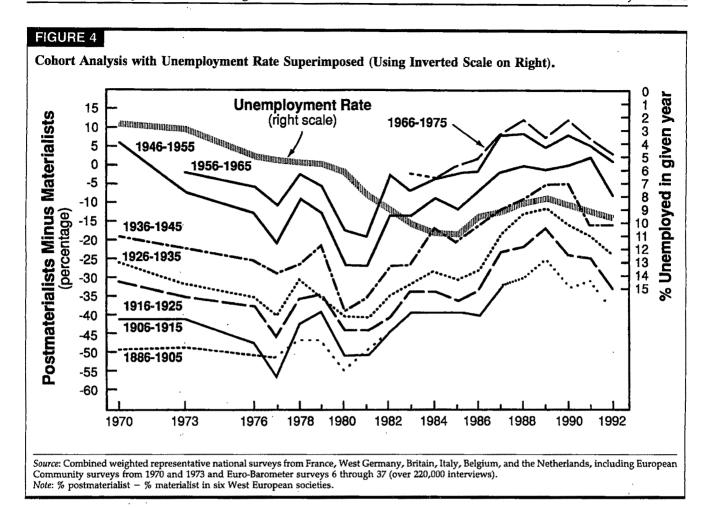
- 1. A good salary so that you do not have any worries about money
- A safe job with no risk of closing down or unemployment
- 3. Working with people you like
- Doing an important job which gives you a feeling of accomplishment.

Unlike both the 1970 question and the 1980–81 question, which could be read as tapping a sociotropic concern to provide greater job security or fair employment practices for everyone, this 1973 item

explicitly refers to concerns that "you personally" would take into account. And here, the relationship between job goals and materialist/postmaterialist values is unambiguous, as we see in Table 3. The pattern is striking. Respondents who emphasize "a safe job with no risk of unemployment" are about half as likely to have postmaterialist values as those who choose "working with people you like" or "a job that gives you a feeling of accomplishment." In all eight countries those who choose "a safe job" are less likely to score as postmaterialists than the overall sample. Moreover, similar results obtain when one examines the relationship between employment goals and a measure of materialism/postmaterialism based upon a choice of twelve national goals. Far from being conducive to postmaterialist responses, a concern with unemployment is linked with *materialist* responses (as the scarcity hypothesis implies).

During certain periods, rising levels of unemployment *did* coincide with aggregate-level increases in postmaterialism. This can be seen in Figure 4, which

GOAL CHOSEN AS	PERCENTAGE CLASSIFIED AS POSTMATERIALIST									
MOST IMPORTANT	WEST GERMANY	GREAT BRITAIN	NETHER- LANDS	FRANCE	BELGIUM	ITALY	DENMARK	IRELAND	MEAN	
A good salary	3	4	4	6	8	6	4	6	5	
A safe job	4	6	8	9	10 .	4.	8	4	7	
Overall sample	9	8	12	13	14	9	12	8	11	
Working with people you like	13	9	17	19	21	13	17	8	15	
A feeling of accomplishme	21 nt	14	18	20	21	16	18	14	18	



(like Figures 2 and 3) shows percentage difference index levels among the eight birth cohorts. For this figure, however, we superimpose unemployment rates. Unemployment is indexed so that a downward movement in the graph indicates higher rates of unemployment. Bear in mind that Clarke and Dutt's analysis is limited to the period from 1976 through 1986.

The most striking feature of the relationship between unemployment and materialist/postmaterialist values is its inconsistency over time. The contrast with the consistent relationship between inflation and values is impressive. From 1970 to 1977 and from 1985 to 1992, the unemployment index moves in the same direction as the value index; that is, higher rates of unemployment are linked with movement toward materialism, not postmaterialism. But the relationship reverses itself during most of the period studied by Clarke and Dutt. Here, rising rates of unemployment were accompanied by value shifts toward postmaterialism, as they claim. This pattern does not hold up even throughout the entire period from 1976 through 1986. During the first year of this period, unemployment is rising, accompanied by a shift toward materialism and during the last year of this period, unemployment is falling, accompanied by a shift toward materialism, and during the last year of this period, unemployment is rising, accompanied by a shift toward materialism, and during the last year of this period, unemployment is falling, accompanied by a shift toward postmaterialist values. But the relationship reported by Clarke and Dutt does prevail from 1981 to 1985. During these years—and only during these years—a resurgence of postmaterialism was accompanied by a steep rise in unemployment. A glance back at Figure 3 reveals that this steep rise in unemployment went along with an even steeper drop in inflation, bringing a sharp rise in postmaterialist values.

A more rigorous test to determine whether there is a significant trend toward postmaterialism can be conducted using a time series regression analysis. Moreover, because statistical analyses are parsimonious, we can examine change in all eight societies, as well as the combined European sample. The results of our time series analysis are presented in Table 4. We present the results of a time series regression analysis (employing ordinary least squares) in which the materialist/postmaterialist values index is the dependent variable and in which the number of years from the baseline survey, the annual inflation rate, and the annual unemployment rate are the independent variables.

The last two columns of Table 4 present diagnostic

					(SIG.			
COUNTRY	В	SE B	BETA	T	2-TAILED)	R ²	DW	ACF (1)
West Germany						,		
Years since baseline	1.31	.28	.50	4.64	.0003	.89	1.36	.263
Inflation	-4.20	.91	45	-4.60	.0003			
Unemployment	1.60	.79	.22	2.02	.0616			
Britain								
Years since baseline	1.21	.31	.66	3.91	.0014	.78	2.01	047
Inflation	99	.36	40	-2.75	.0148			
Unemployment	20	.64	06	32	.7568			
• •								,
Netherlands Years since baseline	1.29	.25	.63	5.18	.0001	.88	2.02	012
	-2.39	.25 .56	.53 50	-4.24	.0007	.00	2.02	012
Inflation	-2.39 58	.36	50 17	-4.24 -1.62	.1261			
Unemployment	56	.30	17	-1.02	.1201			
France								
Years since baseline	1.82	.54	1.34	3.38	.0041	.73	2.19	113
Inflation	60	.39	27	-1.51	.1510	,	•	
Unemployment	-2.54	1.01	~.89	-2.52	.0235			
Belgium								
Years since baseline	.79	.20	.62	4.01	.0011	.85	2.11	136
Inflation	.09	.43	.03	.22	.8278			
Unemployment	-2.41	.29	-1.05	-8.2 9	.0000			
Italy					•			
Years since baseline	1.07	.32	.63	3.29	.0050	.85	1.84	.066
Inflation	-1.46	.23	76	-6.47	.0000			
Unemployment	-2.23	.74	54	-3.01	.0088			
Denmark ^a						•		
Years since baseline	2.65	.59	.82	4.46	.0005	.94	1.63	.145
Inflation	∠.65 −.96	.80	.02 19	1.20	.0005	.54	1.03	.145
Unemployment	96 48	.75	19 06	1.20 64	.5301			
· -	40	./5	06	04	.5301			
Ireland ^a				_				
Years since baseline	2.07	.38	1.09	5.47	.0001	.87	1.47	.116
Inflation	96	.26	57	3.78	.0020			
Unemployment	-2.00	.47	84	-4.25	.0008			
Six European Societies ^b								
Years since baseline	1.03	.25	.61	4.09	.0010	.89	1.84	.003
Inflation	-1.62	.32	53	-4.98	.0002			
Unemployment	40	.53	10	76	.4597			

Source: Data about values are based upon European Community Surveys. Data about inflation and unemployment are from the Statistical Office of the European Communities. For the results employing iterative Prais-Winsten estimators, see Inglehart and Abramson 1993, tbl. A1.

Note: Values are percentage difference indices using the four-item measure (ordinary least squares).

statistics (the Durbin–Watson test and a first-order autocorrelation function coefficient) on the residuals that test for the presence of autocorrelation. Both statistics reveal that there is no problem with autocorrelation, ¹² and we can therefore employ these ordinary least squares results. ¹³

As Table 4 shows, when we simultaneously control for the effects of inflation and unemployment, there is a significant trend toward postmaterialism in all eight countries, *including* Belgium. Of course, we know from Figure 1D that actual levels of postmaterialism increased only negligibly in Belgium. These

analyses suggest that there would have been a substantial increase in postmaterialism in Belgium if unemployment had not contributed to *materialism*. Table 4 also shows that inflation has a statistically significant effect on reducing postmaterialism in every country except France, Belgium, and Denmark. Unemployment has a statistically significant impact on the percentage difference index in Belgium, as well as in France, Italy, and Ireland. But in all four countries, unemployment contributes to materialism. The positive relationship between unemployment and postmaterialism comes close to statistical signif-

[&]quot;Baseline survey is 1973.

^bCombined weighted sample of West Germany, Britain, the Netherlands, France, Belgium, and Italy.

icance in Germany but falls far short of significance when the minimal effects of autocorrelation are removed (see Inglehart and Abramson 1993, table A1).

The crucial finding is that when we take the effects of inflation and unemployment into account, there is a statistically significant trend toward postmaterialism in all eight societies. Moreover, this change is very much in the order of magnitude predicted by Inglehart's thesis—about a one-point gain per annum on the index. For the combined six-nation sample, there is a 1.03-point per year increase in the index; and this result is statistically significant at the .001 level. Inflation also affects values, a result that is statistically significant at the .0002 level. Unemployment is negatively related to postmaterialism, although the relationship is negligible (the significance level is .4597).

These results strongly support Inglehart's (1971) thesis that there would a trend toward postmaterialism. ¹⁴ Moreover, Inglehart was making actual ex ante predictions, not ex post predictions, in which a theory can be designed to fit the observed results. Indeed, the theory predicting the shift toward postmaterialist values was published many years before most of the data used to test it had been collected.

The argument that high rates of unemployment should be conducive to postmaterialism seemed implausible from the start. Although at certain times high rates of unemployment happened to coincide with a rise in postmaterialism, the apparent causal linkage is spurious.

UNEMPLOYMENT AND POSTMATERIALISM IN EX-SOCIALIST COUNTRIES

The evidence we have examined so far is based on only relatively modest changes in unemployment rates. Recent events in the ex-socialist world provide a stronger test of this argument. Within the past few years, these countries have gone from being socialist societies, in which unemployment was quite low, to an abrupt transition to market economies, in which unemployment has suddenly become unprecedentedly widespread. If unemployment is conducive to postmaterialist values, we should observe a sharp rise in postmaterialism in these societies since the collapse of socialism. On the other hand if, as we argue, insecurity is conducive to materialist values, we should find a dramatic shift toward materialism in the last few years.

Among the ex-socialist societies, East Germany provides a particularly good site for testing these two competing interpretations. For one thing, it has experienced a severe rise in unemployment. Since the collapse of the socialist regime in 1990, entire sectors of East German industry have proven to be noncompetitive by Western standards and have closed down completely. Unemployment has risen from an

official rate near zero to traumatic levels that are partly concealed by labeling unemployment payments as retraining salaries but that some observers estimate to include as much as 30% of the work force. Moreover, we can test the proposition readily in this setting because, from reunification to the present time, Euro-Barometer surveys have been carried out, with separate samples in each of the two former regions of Germany each spring and fall.

Let us examine the results. Did the massive rise in unemployment bring a surge in postmaterialist values, as Clarke and Dutt's interpretation implies? Or did it bring a sharp decline in postmaterialism, as the value-change thesis implies? The answer is clear and unequivocal. In Fall 1990, when the German Democratic Republic was abolished, Euro-Barometer Survey 34 found that 21% of the East German public fell into the materialist category, 65% were mixed, and 14% were postmaterialists, yielding a percentage difference index of -7. While this was well below the West German level of postmaterialism, East Germany was probably the most postmaterialist society in the ex-socialist world. But with the subsequent dismantling of large parts of the East Germany economy, this index fell sharply, dropping to -9 in Spring 1991, to -19 in Fall 1991, to -26 in Spring 1992, and to -24 in Fall 1992. Within two years, the proportion of postmaterialists had been cut in half. Here again, the notion that high levels of unemployment are conducive to postmaterialism is contradicted by the empirical evidence.

ECONOMIC SECURITY AND VALUE CHANGE: NEW EVIDENCE FROM 40 SOCIETIES

The World Values Surveys carried out in 1981–83 and 1990–91 provide further evidence that economic security contributes to the rise of postmaterialist values. 15 Though they do not provide the detailed time series that is available from the Euro-Barometer surveys, they do provide a broader coverage of the developmental spectrum that has ever previously been available. The 1981-83 surveys conducted representative national samples in 22 societies; apart from a small sample in Northern Ireland (about 300 cases in both surveys), the sample sizes ranged from 927 to Iceland to 2,303 in Spain. The 1990-91 surveys provide data from representative national samples from 40 societies containing over 70% of the world's population, with sample sizes ranging from 588 in Finland to 4,147 in Spain, and it employed the 12choice value battery. (We give the actual number of respondents in each society that received a score on the 12-choice measure of materialism/postmaterialism in Inglehart and Abramson 1993, tbl. A2). These surveys cover an unprecedentedly broad range of the economic and political spectrum, with data from such low-income nations as China, India, and Nigeria, as well as from ex-socialist countries in Eastern Europe and from advanced industrial democracies. Both World Values Surveys measured materialist/postmaterialist values in every inhabited continent except Australia.

The value change thesis implies that the shift from materialist to postmaterialist priorities is potentially a universal process: it should occur in any country that moves from conditions of economic insecurity to relative security (though during a transitional period, older generations will continue to reflect the conditions that characterized their preadult experiences). This has clear implications concerning the linkage between economic development and value change: though a high level of per capita income does not necessarily produce a sense of security, it seems likely that people living in rich countries will experience a stronger sense of economic security than those in poor nations. A high gross national product per capita is at least a rough indicator of a country's level of economic security. In short, economic development should be conducive to a shift from materialist to postmaterialist values.

Though this implication is straightforward, it has never before been possible to test it adequately because representative national surveys are usually carried out only in relatively developed societies. Using the 1990-91 World Values Surveys, we can now test this hypothesis across the full range of economic development. We elsewhere display the relationship between per capita gross national product and scores on the 12-item measure of materialist/ postmaterialist values for all 40 societies studied in the 1990-91 World Values Survey (Abramson and Inglehart N.d., fig. 1). 16 As we show, societies with relatively high per capita gross national product tend to have higher proportions of postmaterialists than poorer countries have. There is a strong relationship between per capita gross national product and values. The overall correlation is r = .68, significant at the .0001 level. The regression coefficient indicates that every thousand-dollar increase in per capita gross national product contributes to a 1.7 point gain on the materialist/postmaterialist value index. This test suggests that a linear model fits the data relatively well.17

Diez-Nicolas (n.d.) demonstrates that this relationship also holds true at the regional level in Spain. He has included the four-item materialist/postmaterialist values battery in monthly national surveys of the Spanish public since 1988. Cumulating large numbers of interviews for each region enables him to perform a statistically reliable analysis of the relationship between values and economic security at the regional level, in a country with great regional variation. He finds that the relative level of postmaterialism varies considerably from region to region and is stable over time, with the wealthiest regions (the Basque country and Madrid) consistently having the highest propor-

tions of postmaterialists and the poorest regions (Andalusia and Castilla–La Mancha) showing the lowest proportions of postmaterialists in virtually every year. This relationship is strong and shows a particularly good fit with a given region's level of economic development 25 years prior to the survey, during the median respondent's adolescent years (r = .88).

This reinforces the findings from the global array of 40 societies that we have studied (see Abramson and Inglehart n.d.), which also demonstrates that postmaterialist values are closely linked with prosperity. Although the relationship between per capita gross national product and values is cross-sectional, the value-change thesis implies that this configuration reflects a dynamic process in which economic development leads to increasing postmaterialism through a gradual process of intergenerational change. If this theory is correct, we should find a higher proportion of postmaterialists among the younger cohorts than among the old, provided that a society has had sufficient economic growth during the past four or five decades so that the younger cohorts experienced significantly greater economic security during their preadult years than did those who are now in their fifties, sixties, or seventies.

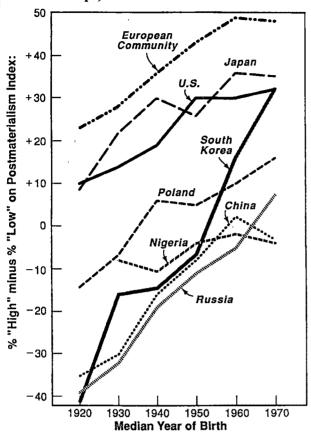
Figure 5 presents the value scores among birth cohorts in several selected societies studied by the 1990-91 World Values Survey (the entire array would be unreadable). As it illustrates, the younger birth cohorts do, indeed, show considerably higher proportions of postmaterialists than the older cohorts. This is not a phenomenon that is limited to Western democracies: it is found across societies having a wide variety of political and economic institutions and a wide variety of cultural traditions. Though the richer countries have much higher absolute proportions of postmaterialists than the poorer ones, we also find a steep slope reflecting intergenerational value differences in poor countries that have experienced a rapid increase in prevailing standards of living during the past several decades.

The results from the European Community countries have already been examined in detail and are combined into a single line on Figure 5. Over all, the European Community shows the highest proportion of postmaterialists on this graph, with the United States and Japan also ranking high. Even the oldest birth cohorts in these advanced industrial societies rank higher than even the youngest cohorts in most other countries. But an upward slope, linked with a rising proportion of postmaterialists to materialists as we move from old to young, is also found in Russia and East Asia.

At first glance, it might seem surprising that the shift from materialist to postmaterialist values has taken place in Russia, which experienced economic stagnation during the decade preceding these surveys and which by 1990 was in a state of economic collapse. If the postmaterialist shift simply reflected

FIGURE 5

Values by Birth Cohort, in Western Democracies, Eastern Europe, and East Asia.



Source: 1990-91 World Values Survey.

Note: Respondents are classified as "high" on the 12-item materialist/
postmaterialist values index if they gave high priority to at least three of
the five postmaterialist goals (i.e., as one of the two most important goals
out of each group of four goals); they are classified as "low" if they chose
none of the five postmaterialist goals among their high priorities.

recent conditions, we would *not* expect to find intergenerational differences. But Inglehart's theory postulates a long-term process of intergenerational change based on the differences experienced during a given cohort's preadult years. From this perspective, we *would* expect to find sizable intergenerational value differences in Russia, for the formative experiences of the cohorts born in the 1950s, and 1960s, and the 1970s were characterized by far more secure circumstances than those which shaped the cohorts born in the 1920s, 1930s, or 1940s.

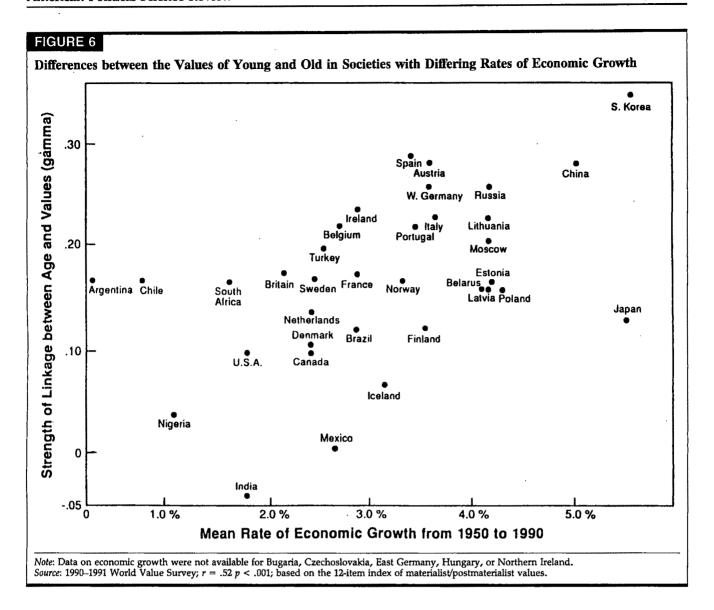
Older Russians, for example, experienced the mass starvation linked with forced collectivization during the 1920s, followed by the terror and the Stalinist purges of the 1930s and the starvation and the loss of 27 million lives in the Soviet Union during World War II. The 1950s and 1960s, by contrast, were an era of recovery and rapid economic growth at rates that exceeded those of most Western countries. This was the era that led Khrushchev to boast, "We will bury

you" (economically)-and at the time, many Western observers thought it a plausible boast. Recent years have been calamitous, creating a period effect that tends to drive all of the Russian cohorts downward toward the materialist pole. But the formative years of the younger cohorts were far more secure than those of the older cohorts, and if the intergenerational differences reflect differences in preadult experience, we would indeed expect to find sizable evidence of intergenerational change in Russia. We do, as Figure 5 demonstrates. Though the Russian cohort line starts and ends at a level far below that of the richer countries, intergenerational differences in Russia are even steeper than those found in Western Europe, the United States, or Japan. Future research will be needed to determine whether the relatively postmaterialist values of young Russians will persist in the face of the economic collapse following the dissolution of the Soviet Union.

China shows an equally steep slope, reflecting sharp intergenerational differences that may have contributed to the Spring 1989 clash between young intellectuals and an octogenarian leadership still in control of the army. There were massive differences between the formative experiences of the older generation in China, who lived through an era of mass starvation and civil war that went on almost continuously from the 1920s to 1949, and those of the younger cohorts, brought up in conditions of relative stability and prosperity, though broken by the severe but relatively brief upheavals of the Great Leap Forward and the Cultural Revolution in the late 1960s.

The most dramatic set of intergenerational value differences are found in South Korea. Since the 1950s, South Korea has shown spectacular economic growth rates: among all the countries for which the World Bank reports data, only one country (Singapore) showed higher economic growth rate during the period from 1965 to 1990. Apart from this ministate, South Korea has led the world in economic growth, rising from starvation-level poverty in the 1950s to become a relatively rich country. In keeping with this history, older Koreans show an immense preponderance of materialist priorities, and Korea's overall level of postmaterialism is still low. But young South Koreans show a clear preponderance of postmaterialist priorities, with the slope rising so steeply that the youngest cohort actually converges with its American counterparts.

By contrast with South Korea, the Nigerian data show negligible intergenerational value differences: the old are almost as likely to emphasize postmaterialist values as are the young. Here again, the size of the intergenerational value differences reflects the rate of economic change experienced by the given country, for Nigeria has had virtually no increase in per capita income since the 1960s. As our theory implies, intergenerational value differences are present if the formative years of the younger cohorts were shaped by significantly higher levels of economic security



than those of the older cohorts. In countries that have not experienced economic development, this does not hold true.

Figure 6 condenses the relationship between age and values for each country into a single coefficient. As this figure demonstrates, the selected examples shown in Figure 5 reflect the overall pattern: intergenerational value differences tend to be largest in those countries which have experienced the greatest amounts of economic growth during the past 40 years. Accordingly, the correlation between age and values is strongest in such countries as South Korea and China. It is weak or negligible in such countries as Nigeria and India, which have not only experienced much lower rates of economic growth than China or South Korea but also have much more unequal distributions of income, with the result that substantial proportions of the population live at the edge of starvation.

Note also that the intergenerational value differences are relatively weak in the United States (the

four-item index shows even weaker differences): though it has a relatively high absolute *level* of postmaterialism, the rate of intergenerational change is relatively small. Although the United States has been one of the world's richest countries throughout the twentieth century, it has not experienced dramatic *changes* between the formative experiences of younger and older cohorts comparable to those which have characterized Europe and East Asia. The United States has been a relatively rich country throughout the lifetime of virtually everyone in the sample and was not devastated by World War II, but it has had relatively slow growth in recent decades.

We find some deviant cases. Argentina and Chile are "overachievers," showing larger intergenerational differences than their economic growth rates would predict; while Japan is an "underachiever," showing smaller intergenerational value differences than its history of economic growth would predict. But overall, the pattern is clear. High rates of economic growth tend to go with large intergenerational value

differences: the relationship is strong (r = .52) and statistically significant at better than .001 level.

Data from a long time series would be needed in order to demonstrate directly that economic development tends to produce an intergenerational shift toward postmaterialist values globally, and such data are not available for most of these countries. But the evidence that is available has a remarkably good fit with the predictions of the postmaterialist value shift thesis, and we can supplement it with a modest amount of relevant time series data: for 20 of these countries, data from the four-item values battery are available from both the 1981–83 and the 1990–91 World Values Survey. 18 As Table 5 shows, among these countries, 18 out of 20 reveal a shift in the predicted direction. Only two countries (Iceland and South Africa) show shifts toward materialism. We have no explanation for why Iceland is a deviant case, but it is not at all surprising that South Africa shows a shift toward materialist goals. A society's values at any given time point reflect a combination of longterm trends and current period effects, and South Africa experienced a period of severe insecurity during the 1980s. Its economy, suffering from international boycott and low commodity prices, experienced economic stagnation throughout the 1980s. Moreover, widespread violence and political instability gave rise to growing concern for physical security among both blacks and whites. Powerful period effects were working to produce a sense of insecurity, rather than the security that contributes to postmaterialist values.

CONCLUSION

The critics who argue that there has been no clear trend toward postmaterialism are mistaken. Despite Thomassen and van Deth's doubts, the evidence since 1986 makes it clear that there has been a strong trend toward postmaterialism in the Netherlands. Although Shively is technically correct in stating that one cannot definitively reject a life-cycle interpretation using cohort data, the data strongly suggest that generational replacement has been a major long-term force pushing postmaterialism upward. As of 1992, the data for all eight countries they examined reveal a statistically significant trend toward postmaterialism.

Clarke and Dutt also assert that short-term change in values responds to economic changes in ways that are inconsistent with Inglehart's thesis. The argument that postmaterialism increases when unemployment rises is not sustainable. At the individual level, the bulk of the data indicate that persons who are concerned about unemployment tend to have materialist values. During the early 1980s, postmaterialism did tend to increase when unemployment was rising, but this relationship reflects a negative correlation between inflation and unemployment. In fact, in those countries in which the effects of unemployment are statistically significant, unemployment is related to the growth of materialism.

TABLE 5

The Shift toward Postmaterialist Values: Results from the 1981–83 versus 1990–91 World Values Surveys

	PERCENTAGE						
		TMATERIAL!					
	PERCEN	TAGE MATE	ERIALIST				
COUNTRY	1981–83	1990–91	NET SHIFT				
Finland	21	23	+2				
Netherlands	-2	26	+28				
Canada	-6	14	+20				
Iceland	-10	-14	-4				
Sweden	-10	9	+19				
W. Germany	-11	14	+25				
Britain	-13	0	+13				
France	-14	4	+18				
Belgium	-16	2	+18				
S. Africa	-16	-33	-17				
Mexico	-19	-14	+5				
Ireland	-20	-4	+16				
Argentina	-20	-6	+14				
Norway	-21	-19	+2				
United States ^a	-24	6	+30				
Japan	-32	-19	+13				
Italy	-39	7	+46				
Spain	-41	6	+35				
N. Ireland	−45	-7	+38				

Source: 1981-83 and 1990-91 World Values Surveys.

Hungary

-50

"The values question was not asked in the United States in the 1981-83 survey; results are from the 1980 NES survey. Results are based on the four-item values index.

-41

+9

In short, the basic four-choice value measure developed by Inglehart behaves very much as he theorized. By the usual tests of validity, the basic fourchoice battery has performed remarkably well. Over the past two decades, investigators in more than 20 countries have tested its validity against scores of theoretically relevant demographic and attitudinal variables; in the vast majority of cases, the predicted relationships have been found. The dimensional structure of the measure has also been analyzed in detail, and the measure has a theoretically coherent structure across a wide range of countries. The stability of this structure has also been demonstrated in surveys conducted in 1973, 1978, 1988, and 1990 that employ a larger number of value choices. To a remarkable degree, the same structure appears in survey after survey.

The four-choice indicator that has been used since 1970 has proven to be a very useful indicator of values, and one that is strongly related to intergenerational differences. This relationship has allowed scholars to predict the direction and even the approximate magnitude of change (the component due to

intergenerational population replacement). That cohort replacement is the major force driving value change seems clear. Figures 2–4 show that given cohorts tended to have about the levels of postmaterialism in 1992 as they had when they were first surveyed 22 years earlier. If there had been no population turnover during these two decades, overall levels of postmaterialism would have increased very little. But in fact, absolute increases in postmaterialism took place in seven out of the eight countries for which detailed data are available over the past 22 years. In every country but Belgium, generational replacement has been a major force pushing absolute levels of postmaterialism upward.

Of course, there is no guarantee that the trend toward postmaterialism will continue. As Duncan reminded us, "There is nothing about a trend, supposing it to have been reliably ascertained for some specific period, that guarantees its own continuation" (1968, 679). It does seem likely, at least for the next several decades, that future generational replacement will tend to push postmaterialism upward. In every society except India, the young were more likely to have postmaterialist values than their elders (although, as Figure 6 shows, this relationship varies greatly from society to society). As Hyman observed: "It is the inevitable fact of life that the young will replace the old and will determine the future course of any trend. Thus if the young differ from the old, and were to continue to do so despite their own aging, some prediction can be ventured" (1972, 243).

Although generational replacement is likely to push postmaterialism upward, our time series analyses demonstrate that economic forces (especially changing inflation rates) can affect value change. Thus, while a trend has been clearly documented and although young Europeans have remained more postmaterialist despite their own aging, economic forces can influence the pace, and even the direction, of future value change. It is possible to make projections about the future magnitude of value change in these societies (see Abramson and Inglehart 1987, 1992; Inglehart 1990), but such projections are only guidelines about what *might* happen.

The consequences of the breakup of the Soviet Union are only beginning to be felt, and it is easy to advance a bleak scenario that renders the entire study of mass values as meaningless. But it is possible to end on a brighter note. We now have data on values from a wide range of societies, representing 70% of the world's population. Conducting surveys, in cooperation with native scholars would have been impossible in many of these countries a decade earlier. The changes that made such research possible have benefited social scientists, but they have the potential of benefiting billions of ordinary citizens as well. This broad range of data from the World Values Surveys helps place the Western European data in global perspective. These new data illustrate two major points.

First, the shift from materialist to postmaterialist values is not a uniquely Western phenomenon.

Rather, it is found in societies with widely different institutions and cultural traditions. The rise of postmaterialist values is closely linked with prosperity and seems to occur wherever a society has experienced enough economic growth in recent decades so that the younger birth cohorts have experienced significantly greater economic security during their formative years than older cohorts experienced. Intergenerational value differences reflect a society's rate of economic growth. Economic growth, of course, is only one factor contributing to security or insecurity, but it happens to be (1) an important part of the story and (2) one for which we have relatively good cross-national and cross-time data. War, domestic upheaval, and ethnic conflict can also have a major impact on feelings of security, but precisely because they tend to be situation-specific (and are less readily quantified), they are more difficult to analyze empirically.

Second, where value change has occurred, intergenerational differences are remarkably robust. In Western Europe, clear and substantial differences between the values of younger and older birth cohorts persisted through the recessions of the mid-1970s and the early 1980s. More remarkable still, in Russia and Eastern Europe sizable intergenerational value differences are found despite the collapse of the economic and political systems in recent years. These values show predictable period effects, in response to current economic conditions. But the postmaterialist value shift does not simply reflect current conditions: it also has a long-term component that seems to reflect the distinctive formative circumstances that given birth cohorts experienced as much as 40 or 50 years ago.

The enduring intergenerational differences found here seriously undermine rational choice explanations that seek to explain behavior as an immediate response to a given situation, unshaped by internal cultural differences, for we observe generational differences that seem to reflect the enduring legacy of the distinctive formative experiences of given generations. At any given point in time, the respective birth cohorts in a given society are in the same situation as their elders, but they respond to it in fundamentally different ways, because they evaluate it by different values. These different responses to the same situation do not seem simply to reflect the fact that the respective cohorts are of different ages, for the distinctive values continue to characterize given cohorts even after they have aged over many years.

Notes

We are grateful to the Commission of the European Communities (and especially to Karlheinz Reif and Anna Melich) for providing us with the Euro-Barometer surveys and to Jim Granato and to Charles W. Ostrom for their extensive advice. Sara McLaughlin assisted with the data analysis. Jan van Deth, Nicolas van de Walle, and Frank P. Zinni, Jr., made helpful comments on earlier versions of this article. The U.S.

National Election Studies surveys were provided by the Inter-University Consortium for Political and Social Research.

- 1. Respondents are asked to select what they believe their country's two top goals should be among the following four choices: (1) maintaining order in the nation, (2) giving the people more say in important government decisions, (3) fighting rising prices, (4) protecting freedom of speech. Respondents who select "maintaining order" and "fighting inflation" are classified as materialists, while those who choose "giving the people more say" and "freedom of speech" are classified as postmaterialists. The four remaining combinations are classified as "mixed."
- 2. As the basic measure of materialism/postmaterialism has three categories (materialist, mixed, and postmaterialist), the percentage difference index is equivalent to a mean score (see Inglehart 1990, 436). More specifically, it is equivalent to a mean score that assigns each postmaterialist a score of 100, each respondent with mixed values a score of 0, and each materialist a score of -100. In the vast majority of analyses, there is a monotonic relationship among the three categories of this measure. For example, in the 1992 U.S. presidential election postmaterialism was positively correlated with voting for Bill Clinton. According to our analyses of the 1992 National Election Study survey, among major-party voters who were postmaterialists (N = 248), 76% voted for Clinton; among those with "mixed" values (N = 876), 57% voted for Clinton; and among major-party voters who were materialists (N = 212), 43% voted for Clinton (based upon weighted Ns.)
- 3. As the time of writing, the Inter-University Consortium for Political and Social Research survey data archive had released the results of all of the Euro-Barometer surveys conducted through Fall 1992.
- 4. Beginning with Fall 1990, the Euro-Barometer surveys include the newly formed states from the territory that used to comprise East Germany. To maintain overtime comparability, we exclude respondents from these states from our analyses.
- 5. Following Inglehart (1977), in our analysis of change over time, we combined the results of European Community Surveys conducted in 1970 and 1971. For 1977, 1978, and 1980–92, we combined the results of two surveys. Clarke and Dutt also employ a single annual result for their analyses (1991, Figs. 1, 2), although Thomassen and van Deth provide results for each semiannual survey (1989, fig. 1).
- 6. We give the full distribution of values for each of these eight European countries for each survey year from 1970–71 or 1973 through Spring 1990 in Abramson and Inglehart 1992.
- 7. In addition to choosing among the four standard national goals (see n. 1), respondents are also asked to choose their two top goals among the following two sets:
- 1. maintain a high rate of economic growth
- 2. make sure that this country has strong defense forces
- give people more say in how things are decided at work and in their community
- try to make our cities and countryside more beautiful and
- 1. maintain a stable economy
- 2. fight against crime
- 3. move toward a friendlier, less impersonal society
- move toward a society where ideas count more than money.

For the scoring procedures used to create the twelve-choice index, see Inglehart 1990.

- 8. We present complete cohort results for each of the eight countries through Spring 1990, as well as the specific values for each cohort for the combined six-nation sample in Abramson and Inglehart 1992. As we point out, most of our cohort estimates for the six-nation sample are based on over a thousand cases, and even the smallest cohorts contain over six hundred cases.
- 9. The impact of education on values is complex, and we discuss its multifaceted effects in Abramson and Inglehart n.d. We present an extensive critique of Duch and Taylor's

(1993) argument that past educational attainment and current economic conditions are the major factors influencing values.

10. See n. 1.

- 11. The cohort results track cohorts through Spring 1992. Data about inflation and unemployment are based upon statistics published by the Statistical Office of the European Communities. German economic statistics for 1990–92 exclude the states that used to comprise East Germany.
- 12. Chatfield points out that the conventional tests of autocorrelation lack power when N is less than 100 (1989, 62). He suggests that the most powerful test is to examine the first-order autocorrelation function coefficient. As there are 18 observations for Denmark and Ireland, a finding of +/-.471 indicates the presence of autocorrelation; as there are 19 observations for the other countries and for the six-nation sample, a finding of +/-.458 indicates autocorrelation. None of the first-order autocorrelation functions is close to these magnitudes. The highest first-order autocorrelation function is in Germany (.263). Germany is the only country in which a result is more than marginally affected by controlling for autocorrelation.
- 13. Even though there was no problem with autocorrelation, we removed the minimal effects of autocorrelation using iterative Paris-Winsten estimators. (See Ostrom 1990 for a discussion.) We present these results in Inglehart and Abramson 1993, tbl. AI. Except for changing a significance level in Germany (the significance of the relationship between unemployment and values falls from .0616 to .1334), there are only marginal differences between the results in that table and those in our Table 4.
- 14. Clarke and Dutt (1991) also argue that Inglehart's measure of materialism/postmaterialism manifests a high level of individual-level instability. This argument is orthogonal to our present analysis, since even relatively high levels of individual-level instability are compatible with the thesis that long-term generational replacement tends to drive postmaterialism upward. However, we have argued elsewhere that Clarke and Dutt's estimates are unreliable: LISREL analyses by Inglehart (1990), De Graaf (1988) and van Deth (1989) all indicate higher levels of stability than Clarke and Dutt found. We offer an extensive discussion of alternative tests of individual-level stability in Inglehart and Abramson 1992.
- 15. The data for the 1981–83 World Values Survey are available through the ICPSR survey data archive; and the 1990–91 World Values Survey data are currently being documented for release through the ICPSR archive and are scheduled to be released in 1994. The documentation will contain information about the sampling procedures used in all 40 societies.
- 16. Factor analyses demonstrate that some minor modifications in Inglehart's 12-choice measure are necessary to construct a measure that is cross-culturally comparable across a wide variety of societies (see Abramson and Inglehart 1993). The "economic growth" option tends to load as a postmaterialist value in the state-socialist countries (including China), and the "beautiful cities" choice tends to load as a materialist value in several state-socialist countries and in Third World societies. However, five of the six items designed to tap postmaterialist values (all except "beautiful cities") clearly tap postmaterialist values. Respondents are initially scored from zero to five, according to the total number of postmaterialist responses they select. Although we have developed a percentage difference to facilitate the visual presentation of these data here, we display the full distribution of scores for each country in Inglehart and Abramson, 1993, tbl. A2.
- 17. Inglehart (1987) does discuss the diminishing marginal utility of increased wealth. However, he does not argue that increasing wealth has a diminishing marginal impact on feelings of security, and the linear relationship between wealth and postmaterialism is consistent with Inglehart's theory.
- 18. Although the first World Values Survey measured materialism/postmaterialism in Denmark, technical problems render this measurement noncomparable with the second World Values Survey. At the request of the Argentine inves-

tigators, the 1983 survey of Argentina was not released through the ICPSR.

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TOWARD A THEORY OF CONSTITUTIONAL AMENDMENT

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Onstitutional design proceeds under the assumption that institutions have predictable consequences, but modern political science has not pursued the empirical verification of these predicted consequences with much vigor. I shall attempt to link the theoretical premises underlying one important aspect of constitutional design, the amendment process, with the empirical patterns revealed by a systematic, comparative study of constitutions. An examination of all amendments in the 50 American states since 1776 reveals patterns that are then confirmed using data from 32 national constitutions. The interaction of the two key variables affecting amendment rate can be described by an equation that generates predicted amendment rates close to those found in the cross-national empirical analysis. A constitution's length measured in number of words, the difficulty of an amendment process, and the rate of amendment turn out to have interlocking consequences that illuminate principles of constitutional design.

constitution may be modified by means of (1) a formal amendment process, (2) periodic replacement of the entire document, (3) judicial interpretation, and (4) legislative revision. What difference does it make whether we use one method rather than another? What is the relationship between these four methods? What do we learn about the constitutional system and its underlying political theory by the pattern of choice among these alternatives? These are some of the questions to be addressed.

Although it is true that a constitution is often used as ideological window dressing and that even in places where constitutions are taken very seriously these documents fail to describe the full reality of an operating political system, it is also true that today hardly any political system, dictatorial or democratic, fails to reflect political change in its constitution. Constitutions may not describe the full reality of a political system, but when carefully read they are windows into that underlying reality.

This essay is an initial attempt to use a critical, if often overlooked, constitutional device—the amendment process—as a window into both the reality of political systems and the political theory or theories of constitutionalism underlying them. A good deal has been written about the logic of constitutional choice using rational-actor models, but little has been written about the empirical patterns that result from constitutional choice. The classical example of the first approach is the work of James Buchanan and Gordon Tulloch (1965). The second approach is exemplified by the work of Douglas W. Rae (Rae 1967; but see also Grofman 1986; Przeworski 1991; Shugart and Carey 1992). I shall use the latter method and attempt to be systematic, comparative, and, to the extent possible, empirical. I shall begin with a brief overview of the theoretical assumptions that underlay the formal amendment process when it was invented, identify a number of theoretical propositions concerning the amendment process, and then look for patterns in the use of the amendment process

that can be used to create empirical standards upon which to erect a theory of constitutional amendment useful to those engaged in constitutional design.

THE ORIGINAL PREMISES UNDERLYING THE AMENDMENT PROCESS

The modern written constitution, first developed in English-speaking North America, was grounded in a doctrine of popular sovereignty (Adams 1980; Morgan 1988). Even though many in Britain were skeptical at best, Americans did not regard popular sovereignty as a experimental idea but, rather, as one that stood at the very heart of their shared political consensus (Lutz 1988, esp. chap. 7). American political writing had used the language of popular sovereignty before Locke's Second Treatise was published, and the early state constitutions of the 1770s contained clear and firm statements that these documents rested upon popular consent (Lutz 1980, 218-25). Although the theory of popular sovereignty was well understood in America by 1776, the institutional implications of this innovative doctrine had to be worked out in constitutions adopted over the next decade. Gradually, it was realized that a doctrine of popular sovereignty required that constitutions be written by a popularly selected convention, rather than the legislature, and then ratified through a process that elicited popular consent-ideally, in a referendum. This double implication was established in the process used to frame and adopt the 1780 Massachusetts and 1784 New Hampshire constitutions, although the referendum portion of the process did not become standard until the nineteenth century.

Americans moved quickly to the conclusion that if a constitution rested on popular consent, then the people could also replace it with a new one. John Locke had argued that the people could replace government but only when those entrusted with the powers of government had first disqualified themselves by endangering the happiness of the community to such a degree that civil society could be said to have reverted to a state of nature. Americans went well beyond Locke by institutionalizing the power to change the constitution amid civil society—that is to say, whenever they wanted. It is of considerable importance that this included not only replacing the constitution but also formally amending it.

The first new state constitution in 1776, that of New Jersey, contained an implicit notion of amendment, but the 1776 Pennsylvania document contained the first explicit amendment process—one that used a convention process and bypassed the legislature. By 1780, almost half the states had an amendment procedure, and the principle that the fundamental law could be altered piecemeal by popular will was firmly in place.

In addition to popular sovereignty, the amendment process was based on three other premises central to the American consensus in the 1770s: an imperfect but educable human nature, the efficacy of a deliberative process, and the distinction between normal legislation and constitutional matters. The first premise, clearly explicated by Vincent Ostrom (1987), held that humans are fallible but capable of learning through experience. Americans had long considered each governmental institution and practice to be in the nature of an experiment. Since fallibility was part of human nature, provision had to be made for altering institutions after experience revealed their flaws and unintended consequences. Originally, therefore, the amendment process was predicated not only on the need to adapt to changing circumstances but also on the need to compensate for the limits of human understanding and virtue. In a sense, the entire idea of a constitution rests on an assumption of human fallibility, since, if humans were angels, there would be no need to erect, direct, and limit government through a constitution.

A belief in the efficacy of a deliberative process was also part of the general American constitutional perspective. A constitution was viewed as a means not merely to make collective decisions in the most efficient way possible but to make the best possible decisions in pursuit of the common good under a condition of popular sovereignty. The common good is a more difficult standard to approximate than the good of one class or part of the population, and the condition of popular sovereignty, even if operationalized as a system of representation, requires the involvement of many more people than forms of government based on other principles. This in turn requires a slow, deliberative process for any political decision, and the more important the decision, the more deliberative the process should be. Constitutional matters were considered more important in 1789 America than normal legislation, which led to a more highly deliberative process distinguishing constitutional from normal legislative matters. The codification of the distinction in constitutional articles of ratification and amendment resulted in America constitutions being viewed as higher law that should limit and direct the content of normal legislation.

Popular sovereignty implies that all constitutional matters should be based upon some form of popular consent, which in turn implies a formal, public process. Human fallibility implies the need for some method of altering or revising the constitution. A distinction between normal and constitutional matters implies that constitutional matters require a distinctive, highly deliberative process and thus implies the need for an amendment procedure more difficult than that used for normal legislation.

Together these premises require that the procedure be neither too easy nor too difficult. A process that is too easy, not providing enough distinction between constitutional matters and normal legislation, thereby violates the assumption of the need for a high level of deliberation and debases popular sovereignty while one that is too difficult, interfering with the needed rectification of mistakes, thereby violates the assumption of human fallibility and prevents the effective utilization of popular sovereignty.

The literature on constitutions at one time made a distinction between major and minor constitutional alterations by calling the former "revisions" and the latter "amendments." As Albert L. Sturm (1970) points out, the distinction turned out in practice to be conceptually slippery, impossible to operationalize, and therefore generally useless. Because revision is used in the literature to mean several different things, I shall use amendment as a description of the formal process developed by the Americans and alteration to describe processes that instead use the legislature or judiciary. Unless we maintain the distinction between formal amendment and other means of constitutional modification, we will lose the ability to distinguish competing constitutional theories.

The innovation of an amendment process, like the innovation of a written constitution, has diffused throughout the world to the point where less than 4% of all national constitutions lack a provision for a formal amending process (Maarseveen and Van der Tang 1978, 80). However, the diffusion of written constitutions and the amendment idea do not necessarily indicate widespread acceptance of the principles that underlie the American innovation. In most countries with a written constitution popular sovereignty and the use of a constitution as a higher law are not operative political principles. Any comparative study of the amendment process must first distinguish true constitutional systems from those that use a constitution as window dressing and then recognize that among the former there are variations in the amendment process that rest on assumptions at odds with those in the American version. Indeed, my chief concern is the efficiency with which study of the amending process reveals such theoretical differ-

At the same time, a comparative study of amendment processes allows us to delve more deeply into the theory of constitutional amendment as a principle of constitutional design. For example, we might ask

the question, what difference does it make if constitutions are formally amended through a political process that does not effectively distinguish constitutional matters from normal legislation? Why might we still want to draw a distinction between formal amendment and alteration by normal politics as carefully and as strongly as possible? One important answer to the question is that the three prominent methods of constitutional modification other than complete replacement—formal amendment, legislative revision, and judicial interpretation—reflect declining degrees of commitment to popular sovereignty, and the level of commitment to popular sovereignty may be a key attitude for defining the nature of the political system.

BASIC ASSUMPTIONS AND PROPOSITIONS

Every theory has to begin with a number of assumptions. We have seen how the original American version rested on the premises of popular sovereignty, an imperfect but educable human nature, the efficacy of a highly deliberative decision-making process, and the distinction between normal and constitutional law. While these help define the working assumptions of one theory of amendment (albeit the original one), they do not provide a complete basis for describing either the American theory or a general theory of amendment. I turn now to developing a theory that includes the American version but also provides the basis for analyzing any version of constitutional amendment. The intent of the analysis is to provide guidelines for constitutional design in any context—guidelines that will allow framers to link the design of a formal amendment process securely to desired outcomes.

My first and second working assumptions have to do with the expected change that is faced by every political system and with the nature of a constitution, respectively.

Assumption 1. Every political system needs to be modified over time as a result of some combination of (1) changes in the environment within which the political system operates (including economics, technology, foreign relations, demographics, etc.); (2) changes in the value system distributed across the population; (3) unwanted or unexpected institutional effects; and (4) the cumulative effect of decisions made by the legislature, executive, and judiciary.

Assumption 2. In political systems that are constitutional, in which constitutions are taken seriously as limiting government and legitimating the decision-making process they describe, important modifications in the operation of the political system need to be reflected in the constitution.

If these two assumptions are used as premises in a deductive process, they imply a conclusion that stands as a further assumption.

Assumption 3. All constitutions require regular, periodic modification, whether through amendment, judicial or legislative alteration, or replacement.

Alteration (as noted earlier) refers to changes in a constitution through judicial interpretation or legislative action. However, I am initially more concerned with the use of a formal amendment process. Amendment rate, a key concept, refers to the average number of formal amendments passed per year since the constitution came into effect. As Albert L. Sturm (1970) points out, many scholars criticize constitutions that are much amended. However, constitutionalism and the logic of popular sovereignty are based on more than simplicity and tidiness. Any people who believe in constitutionalism will amend their constitution when needed, as opposed to using extraconstitutional means. Thus, a moderate amendment rate will indicate that the people living under it take their constitution seriously. The older a constitution is, under conditions of popular sovereignty, the more successful it has been but also the larger the number of amendments it will have. However, it is the rate of amendment that is important in this regard, not the total number of amendments.

A successful constitutional system would seem to be defined by a constitution of considerable age that has a total number of amendments which, when divided by the constitution's age in years, represents a moderate amendment rate—one that is to be expected in the face of inevitable change. A less-than-successful constitutional system will have a high rate of constitutional replacement.

This raises the question of what constitutes a "moderate" rate of amendment. Since I hope to illuminate the question empirically, rather than in an a priori manner, I must initially use a symbolic stand-in for "moderate rate of amendment." Since a moderate rate is likely to be a range of rates, rather than a single one, the symbol will define boundaries such that any document with an amendment rate above or below its limits will have an increasing probability of being replaced or an increasing probability that some extraconstitutional means of constitutional evolution is being used. I shall use <#> to represent this moderate range of amendment rates symbolically.

The first proposition is frequently found in the literature, but it has never been systematically verified, or its effect measured.

PROPOSITION 1. The longer a constitution is (the more words it has), the higher its amendment rate, and the shorter a constitution, the lower its amendment rate.

Commentators frequently note that the more provisions a constitution has, the more targets there are for amendment and the more likely that it will be targeted because it deals with too many details that are subject to change. While this seems intuitively correct, the data that are used usually raise the question, Which comes first: the high amendment rate or the long constitution? This is because a constitution's

length is usually given as of a particular year, rather than in terms of its original length. Is a constitution long because it had a high amendment rate, or did it have a high amendment rate because it was long to begin with?

My second proposition is also a common one in the literature, although it too has never been systemati-

cally tested before.

PROPOSITION 2. The more difficult the amendment process, the lower the amendment rate, and the easier the amendment process, the higher the amendment rate.

As obvious as this proposition is, it cannot be tested until one shifts from the number of amendments in a constitution to its amendment rate and until one develops an index for measuring the degree of difficulty associated with an amendment process. I shall present such an index as part of what is needed to develop a way of predicting the likely consequences of using one amendment process versus another.

The literature on American state constitutions generally argues that these documents are much longer than the national constitution because they must deal with more governmental functions. For example, if a constitution deals with matters like education, criminal law, local government, and finances, it is bound to be more detailed, longer and thus have a higher amendment rate than one that does not address these matters. From this, I generalize to the following proposition.

PROPOSITION 3. The more governmental functions dealt with in a constitution, the longer it will be and the higher its rate of amendment will be.

Constitutions are usually replaced for one of three reasons: (1) a regime change may leave the values, institutions, and/or implications of the old constitution seriously at odds with those preferred by the people now in charge; (2) the constitution may fail to keep up with the times; and (3) the old constitution may have been changed so many times that it is no longer clear what lies under the encrustations, so that clarity demands a new beginning. A moderate amendment rate is an antidote to all three.

PROPOSITION 4. The further the amendment rate is from the mean of <#>, either higher or lower, the greater the probability that the entire constitution will be replaced and thus the shorter its duration. Conversely, the closer an amendment rate is to the mean of <#>, the lower the probability that the entire constitution will be replaced and thus the longer its duration.

A low rate of amendment in the face of needed change may lead to the development of some extraconstitutional means of revision—most likely, judicial interpretation—to supplement the formal amendment process. I can now, on the basis of earlier discussion, generate several propositions that will prove useful toward the end of my discussion on the implications of the major competing forms of formal constitutional amendment. Proposition 5. A low amendment rate associated with a long average constitutional duration strongly implies the use of some alternate means of revision to supplement the formal amendment process.

PROPOSITION 6. In the absence of a high rate of constitutional replacement, the lower the rate of formal amendment, the more likely the process of revision is dominated by a judicial body.

Proposition 7. The higher the formal amendment rate, (a) the less likely that the constitution is being viewed as a higher law, (b) the less likely that a distinction is being drawn between constitutional matters and normal legislation, (c) the more likely that the document is being viewed as a code, and (d) the more likely that the formal amendment process is dominated by the legislature.

Proposition 8. The more important the role of the judiciary in constitutional revision, the less likely the judiciary is to use theories of strict construction.

I shall test propositions 1–4 using data from the American state constitutions and then seek further verification by examining the amendment process in nations where constitutionalism is taken seriously and does not serve merely as window dressing. The American state documents are examined first because data on them are readily available and easily comparable, because the similarities in their amendment process reduce the number of variables that must be taken into account, and because together they comprise a significant percentage of human experience with serious constitutionalism.

AMENDMENT PATTERNS IN AMERICAN STATE CONSTITUTIONS, 1776–1991

Albert L. Sturm (1970) summarizes the literature as seeing state constitutions burdened with (1) the effects of continuous expansion in state functions and responsibilities and the consequent growth of governmental machinery; (2) the primary responsibility for responding to the increasing pressure of major problems associated with rapid urbanization, technological development, population growth and mobility, economic change and development, and the fair treatment of minority groups; (3) the pressure of special interests for constitutional status; and (4) continuing popular distrust of the state legislature, based on past abuses, which results in detailed restrictions on governmental activity. All of these factors contribute to the length of state constitutions, and it is argued that not only do these pressures lead to many amendments-and thus to greater length-but that greater length itself leads to the accelerated need for amendment simply by providing so many targets for change. Thus, length becomes a surrogate measure for all of these other pressures to amend and is a key variable.

Appendix table A-1 shows basic data for duration,

length, and amendments for the U.S. constitution and the constitutions of the 50 states. The average amendment rate is much higher for the state constitutions than it is for the U.S. Constitution. Between 1789 and 1991 the Constitution was amended 26 times for a rate of .13 (26 amendments/202 years = .13 amendments per year). As of 1991, the current state constitutions had been in effect for an average of 95 years and had been amended a total of 5,845 times, or an average of 117 amendments per state. This produces an average amendment rate of 1.23 for the states, about 9 1/2 times the national rate.

Proposition 1 hypothesizes a positive relationship between the length of a constitution and its amendment rate: the longer a constitution when adopted, the higher its rate of amendment. The data on American state constitutions strongly support proposition 1 with a correlation coefficient of .6249 significant at the .001 level. Furthermore, the relationship holds whether we use the original or the current amended length.

The average length of state constitutions increases from about 19,300 words as originally written to about 24,300 as amended by 1991, which raises the interesting question of what difference it makes whether we use a constitution's original length or its current amended length. The surprising answer is that it makes no real difference. The curve of best fit for amendment rates using the original length of a constitution has a slope of .58, that of amendment rates using the amended length, a slope of .62. There is thus good reason, when testing the propositions against foreign national constitutions, for using either the original or the amended length.

Also, the correlation coefficient between amended and unamended rates is .9936 (significant at the .001 level), which strongly implies that the rate of increase in amendment rate resulting from increasing a constitution's length is virtually constant across all lengths. Finally, since at any point in time the set of constitutions used to test the propositions will vary considerably in age and thus be a mixture of documents ranging from slightly amended to highly amended, we should probably use a composite curve that reflects this inevitable mix. In the case of American state constitutions, the obvious composite curve would be one that averaged .58 and .62. The resulting amendment rate curve with a slope of .60 indicates that for every ten-thousand-word increase in a constitution's length, the amendment rate will increase by .60.

The relationship between the length of a constitution and its amendment rate is the strongest and most consistent one found in the analysis of data drawn from the American states. The strength of this relationship can be underscored by a partial listing of the variables examined that did not show any significant independent correlation with amendment rate. These variables include geographical size, population, level of industrialization, per capita personal income, per capita state expenditures, size of legislature, partisan division in legislature, geographical

region, geographical proximity, and the historical era in which the constitution was written. Controlling for these other variables, the importance of constitutional length remains, whereas controlling for constitutional length, the few weak correlations with these other variables disappear.

State constitutions, on average, are much longer than the U.S. Constitution. Can we account for this difference? Proposition 3 suggests that the wider range of governmental functions at the state level results in significantly longer documents and thus produces a higher amendment rate that makes them longer still (in line with proposition 1).

Data from a recent decade show that amendments dealing with local governmental structure (4.7%), state and local debt (4.3%), state functions (9.0%), taxation and finance (14.1%), amendment and revision (2.6%), and local issues (28%) comprise about 63% of all state amendments and pertain to topics that have not been part of national constitutional concern.²

If we exclude these categories of issues from the amendment count, we end up with an adjusted state rate of about .47. This figure is still a bit over three-and-a-half times the national amendment rate, but by eliminating the amendments peculiar to state constitutions we obtain a figure for comparison with the national rate (.13), using what amounts to the same base. The difference between .13 and .47 represents what we might term the "surplus rate" that still needs to be explained. An interesting question—one that never seems to be asked—is whether the state amendment rate is too high or the national amendment rate is too low.

The answer depends in part on one's attitude toward judicial interpretation. Propositions 5 and 6 suggest that for one who prefers judicial interpretation as a means of modifying a constitution over a formal amendment process, the amendment rate for the national document is not too low. However, for one who prefers a formal amendment process, such as an attachment to popular sovereignty, the amendment rate of the U.S. Constitution may well be too low and the amendment rate of the states is to be preferred.

Propositions 5 and 6 assume a low rate of amendment coupled with constitutional longevity. Proposition 4, on the other hand, posits a general relationship between the rate of amendment and constitutional longevity. Dividing the number of constitutions a state has had into the number of years it has been a state produces the average duration of the state's constitutions—a measure of constitutional activity that controls for a state's age. Table 1 shows that a high amendment rate is associated with low average duration and, thus, high replacement rate (r = -.3561, significant at the .01 level).

However, proposition 4 predicts that the rate at which constitutions are replaced will increase as the amendment rate moves up *or* down with respect to <#>. In Table 1, the amendment rate is the dependent variable. However, if we make it the independent

Amendment Rate of a State Constitution, by Average Duration								
	1	AVER	AGE :	DURA	TION	(YRS	.)	
	1–25				101- 25		151+	
Amendment rate	2.37	1.95 (13)		1.10 (6)	.93 (8)	.84 (5)	.64 (2)	

dent variable instead, we can test directly for the bidirectional effect. Table 2 supports proposition 4. The average duration of a state's constitution declines as the amendment rate goes above 1.00 and as it goes below .75. This means that for American state constitutions, an amendment rate between .75 and 1.00 is associated with the longest-lived constitutions and thus with the lowest rate of constitutional replacement. This range, then, will be defined as <#>. The 13 constitutions with amendment rates within <#> (as just defined) average .89, which we will define as # within <#>.

I turn now to developing an index with which to measure the difficulty of a given amendment procedure. I shall then be ready to look at the constitutions of other nations.

AMENDMENT PATTERNS AND THE CHARACTERISTICS OF THE AMENDMENT PROCESS

In the American states the method of ratifying an amendment can essentially be held constant since every state but one now uses a popular referendum for approval. However, amendments may be initiated by the state's legislature, an initiative referendum, a constitutional convention, or a commission. It is generally held that the more difficult the process of initiation, the fewer the amendments proposed and thus the fewer passed. It is also believed that the initiative has made the process of proposing an amendment too easy and opened a floodgate of proposals that are then more readily adopted by the electorate that initiated them. Another widely held

TABLE 3				
Method of Initiation	and	State	Amendment	Rate,

1970-79

RATE AND	METHO	METHOD OF INITIATION						
FREQUENCY OF AMENDMENT	PROPOSED BY LEGIS- LATURE	POPULAR INITIA- TIVE	SPECIAL CONVEN- TION					
Amendment Rate	1.24	1.38	1.26					
% of Amendments Using This Method	91.5	2.2	6.3					
No. of Constitutions in Category ^a	50	17	5					

Source: Data are derived from Sturm 1982, 78–79. "Total exceeds 50, since many states specify the possibility of more than one method for proposing amendments.

belief is that the stricter or more arduous the process a legislature must use to propose an amendment, the fewer the amendments proposed.

First of all, as Table 3 shows, during a recent decade, relatively few amendments were proposed by other than a legislature. One-third of the states use popular initiative as a method of proposing amendments, and yet in these states the legislative method was greatly preferred. The popular initiative has received a lot of attention, especially in California, but in fact it has had a minimal impact so far.

What has been the relative success of these competing modes of proposing constitutions? The relatively few amendments proposed through popular initiative have a success rate roughly half that of the two prominent alternatives (32% versus 64% for legislature initiated and 71% for convention initiated). The popular initiative is in fact more difficult to use than legislative initiative and results in proposals that are less well considered and thus less likely to be accepted.

What about the varying methods for *legislative* initiation? States differ in how large a legislative majority is needed for a proposal to be put on the ballot, and some states require that the majority be sustained in two consecutive sessions. Table 4 summarizes what we find in this regard.

BLE 2								
rage Duration of	a State C	Constitution	n by Amend	ment Rate				
	AMENDMENT RATE							
	05	.5175	.76-1.00	1.0125	1.2650	1.5175	1.76-2.00	2.00+
Average duration	71	90	100	86	79	57	40	38
•	(7)	(8)	(13)	(4)	(4)	(6)	(1)	(7)

TABLE 4

Comparative Effect of Majority Size on Amendment Rate in American State Constitutions

	REQ	REQUIRED LEGISLATIVE MAJORITY							
		50% + 1 TWICE	60%	67%	75%	67% TWICE			
Ratio of difficulty to simple maj.	1.00 (11)	1.04 (6)		1.62 (19)		3.56 (4)			

Note: The numbers in parentheses indicate the numbers of states using this required legislative majority.

In this table, the decline in the amendment rate produced by each type of legislative majority has been normed against that of the least difficult method. This norming is accomplished by taking the success rate of amendment proposals initiated by a simple (bicameral) legislative majority, and dividing it by the success rate of proposals initiated by a two-thirds legislative majority, a three-fourths majority, etc.-always keeping the other variables constant. For example, the data indicate that in the American states, when the method of initiation is stiffened to require approval by a simple (bicameral) legislative approval twice, the amendment rate is reduced from the baseline of 71% to a little over 68%. Dividing 71% by 68% results in an index score of 1.04. Likewise, a requirement for a three-fifths (bicameral) legislative majority results in a success rate of 56%. Dividing 71% by 56% produces an index score of 1.26. A score of 2.00 therefore indicates a method that is twice as difficult, a 3.00 indicates a method three times as difficult, and so on. Table 4 arrays the empirical results from lowest to highest level of difficulty rather than according to any theoretical prediction. The results are mostly in line with common sense expectations, (although why a second majority vote has so little effect while a second two-thirds vote has so much is not clear).

We can derive three conclusions from Table 4:

- 1. Generally speaking, the larger the legislative majority required for initiation, the fewer the amendments proposed and the lower the amendment rate.
- Requiring a legislature to pass a proposal twice does not significantly increase the difficulty of the amendment process if the decision rule is one-half plus one.
- 3. The most effective way to increase the difficulty of amendment at the initiation stage is to require the approval of two consecutive legislatures using a two-thirds majority each time.

Beyond these three interesting proposals, it is also useful to discover that the variance in the degree of difficulty between alternative legislative majorities is sufficient to establish the core of an *index of difficulty* for any amendment process. An attempt at such an

index is presented in Appendix B, which identifies sixty-eight possible actions that could in some combination be used to initiate and approve constitutional amendments, and that together cover the combinations of virtually every amendment process in the world. The state data on which Table 4 is based generate the index scores for actions 14 through 23, rounding off the score to the nearest .05. If we assume that legislative processes for approval are symmetrical with those for initiation, the index scores for actions 50 through 59 are the same as those for 14-23. If we assume that a unicameral legislative process is one half as difficult as a bicameral one, the index scores for actions 4-13 and 39-49 are as indicated. As reported earlier, amendments proposed by popular initiative have almost exactly one-half the success rate of those initiated by the legislature. If we weight the difficulty of legislative initiative according to the number of states using each type of majority, we obtain a combined, weighted index score of 1.50 for legislative initiative, and thus an index score of 3.00 for popular initiatives (action 24).

Also, we know from state data going back to 1776 that the success rate of amendment proposals after popular referenda became the standard means of approval is virtually the same as when the agent of approval was the state legislature. We can thus say that a popular referendum used as a means for approving a proposed amendment (as opposed to initiating one) is about as difficult as having the state legislature approve it. We have just seen that the weighted average for bicameral legislative action is 1.50, and so we assign an index score of 1.50 to action 60, adding further increments for larger popular majorities, action 61-62. If we assume that special bodies act much the same as unicameral legislatures, the assigning of index scores for actions 2-3 and 32–38 is straightforward.

The use of American state data, in combination with straightforward assumptions, allows the generation of index scores for all actions except numbers 27–30 and 63–68. These actions are assigned index scores that seem reasonable in the context of the other index scores.

The index score assigned to the amendment process found in a national constitution is generated by adding together the numbers assigned by the index in Appendix B to every step required by that particular amendment process. Where a constitution provides for more than one path to a formal amendment, the score for each amendment path is weighted according to the percentage of amendments passed by means of it during the relevant time period.

How the index works can be illustrated by using it with the amendment process described in Article V of the U.S. Constitution. There is more than one path to amendment, and each must be evaluated. A two-thirds vote by Congress, since it requires two houses to initiate the process, is worth 1.60; whereas initiation by two-thirds of the state legislatures is worth 2.25. The latter path leads to a national convention, which uses majority rule in advancing a proposal,

T	Λ	E	Б

	AMENDMENT RATE							
	.0024	.2574	.75–1.24	1.2574	1.75-2.24	2.2574	2.75-3.24	3.25+
Average Duration	43 (8)	50 (2)	96 (5)	47 (3)	19 (1)	17 (2)	26 (10)	-

Note: Index scores are normed to that of stop #14, simple majority approval in a bicameral legislature, by dividing the approval rate for this baseline by the approval rate for each of the other possible actions. The increments are the estimated increase in the difficulty of passing amendments relative to the baseline rules.

thus adding .75 (under the assumption that this special body is elected). The first path still totals 1.60, and the other now totals 3.00. Ratification by threefourths of the states through either their legislatures or elected conventions adds 3.50. The path beginning with Congress now totals 5.10, while the path beginning with the state legislatures and using a national convention totals 6.50. Even though the second path has never been successful (and one can see more clearly now why), it is still a valid option. For the total amendment process we can use the lower figure unless or until the more difficult procedure is ever used. That is, since the 6.50 path has never been used, a weighted composite score would be 5.10, which is what I shall use here. Appendix table C-1 shows the index of difficulty scores calculated for the national constitutions of 32 countries, along with other constitutional characteristics.

Performing the same calculation for the American states, I find that the average index score is 2.92, with very little variance. The highest state score is 3.60 (Delaware), and 26 states are tied for the lowest score at 2.75. Another 16 states have a score of 3.10. Thus, while one can detect variance between select subsets of states, the range of variance in general is very small compared with that found in the constitutions of other nations.

We have reached a point where we can now begin to test our propositions using data from the constitutions of other nations.

CROSS-NATIONAL AMENDMENT PATTERNS

Comparative cross-national data show that the U.S. Constitution has the second-most-difficult amendment process. This implies, if propositions 2 and 4 are correct, that the amendment rate for the Constitution may be too low, because its amendment procedure is too difficult, while the average amendment rate for the state constitutions is not too high.

There is an even stronger relationship between the length of a constitution and its amendment rate here than I found with the American state constitutions, with a correlation coefficient of .7970 (versus .6249 for the states) significant at the .0001 level.

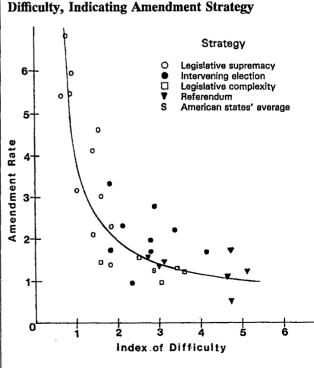
The curvilinear relationship found between the

amendment rate and average duration of American state constitutions is almost duplicated here in shape, strength, and high point. For the national constitutions, <#> is .75–1.24 (# = .95), and the high point is 96 years in average duration (see Table 5), while for the states, # is .75–1.00 (# = .89), and the high point is 100 years in average duration.³ Both sets of constitutions studied have a similar moderate range of amendment rate that tends to be associated with constitutional longevity.

There is enough variance in the index of difficulty among cross-national constitutions for us now to test proposition 2 with some degree of confidence. Figure 1 illustrates that there is a very strong relationship (significant at the .001 level) between the index of difficulty and the amendment rate. The more difficult the amendment process, the lower the amendment rate, and vice versa.

FIGURE 1

Cross-National Pattern for Amendment Rate and Difficulty, Indicating Amendment Strategy



	AMENDMENT STRATEGIES							
	LEGISLATIVE SUPREMACY	INTERVENING ELECTION (DOUBLE VOTE)	LEGISLATIVE COMPLEXITY (REFERENDUM THREAT)	REQUIRED REFERENDUM OR EQUIVALENT				
	Austria Botswana Brazil Germany India Kenya Malaysia New Zealand Papua N.G. Portugal Samoa	Argentina Belgium Colombia Costa Rica Finland Greece Iceland Luxembourg Norway	Chile France Italy Spain Sweden	Australia Denmark Ireland Japan Switzerland U.S. Venezuela				
Average Index score Length Amendment rate	1.23 59,400 5.60	2.39 13,000 1.30	2.79 18,300 1.19	4.01 11,200 .28				

That the relationship between amendment rate and difficulty of amendment process is highly curvilinear is more interesting than if it were simply a linear one, since there is a relatively small part of the curve where most of the effect is concentrated. This confirms the existence of a range of amendment rates that is more critical and toward which one should aim if constitutional stability is being sought. But it also suggests that one can with some confidence achieve a moderate rate of amendment by selecting an appropriate range of amendment difficulty. This, in turn, suggests that certain amendment strategies are better in this regard than others—a topic to which I now turn.

The difficulty of the amendment process chosen by framers of constitutions seems related to the framers' relative commitment to the premises used by the Americans when they invented the formal amendment process: (1) popular sovereignty, (2) a deliberative process, and (3) the distinction between normal legislation and constitutional matters. We can use these assumptions to group our 32 national constitutions into one of four general amendment strategies.⁵

Strategy 1 can be labelled *legislative supremacy* (Table 6, col. 1). Constitutions in this category reflect the unbridled dominance of the legislature by making one legislative vote sufficient to amend the constitution. The data reveal that the size of the majority required for this vote does not affect the amendment rate. The legislative supremacy strategy reflects a minimal commitment to the three American premises just listed. Strategy 2 is to require that the national legislature approve an amendment by votes in two sessions with an *intervening election* (col. 2). The national legislature is still basically in control, but the amendment process is made more deliberative, a clearer distinction is drawn between normal legislation and constitutional matters, and the people have

an opportunity to influence the process during the election, which implies a stronger commitment to popular sovereignty. Sometimes other requirements diminish legislative dominance; the introduction of a nonlegislative body in the process (e.g., a constitutional commission) is typical. The double vote with an intervening election is the key change, so strategy (2) is termed the Intervening Election Strategy. As we move from strategy 1 to strategy 2 the amendment rate falls by 77%. Approximately half (54%) of this drop is explained by the 78% reduction in the average length of a constitution, and about half is explained by the 94% increase in the index of difficulty that results primarily from the double-vote, intervening election strategy.

Strategy 3 relies on *legislative complexity* (col. 3), usually characterized by multiple paths for the amending process, which features the possibility of a referendum as a kind of threat to bypass the legislature. A referendum can usually be called by a small legislative minority, the executive, or an initiative from a small percentage of the electorate—and often any of the three. This complexity and easy availability of a referendum emphasizes even more strongly the deliberative process, the distinction between constitutional and normal legislative, and popular sovereignty.

The legislative complexity strategy produces only an 8% reduction in the amendment rate compared with the intervening election strategy, although this is a 31% improvement over what we would expect, given the increased average length of strategy 3 documents. The slight overall improvement is due to the modest (18%) increase in the difficulty of the strategy. Strategies 2 and 3 together show most clearly how one can achieve similar amendment rates

by trading off between constitutional length and amendment difficulty.

Strategy 4 institutionalizes the most direct form of popular sovereignty and also emphasizes to the greatest extent both the deliberative process and the distinction between constitutional and normal legislative matters. These countries have a required referendum as the final part of the process. The United States is placed in this category because the various appeals to the citizenry required by both amendment paths approximates a referendum and because the United States does not approximate any other strategy even remotely.

Compared to strategy 3, the referendum strategy's rate of amendment falls off 76%. This reduction to what is barely one-twentieth the rate for legislative-supremacy countries is about evenly explained by the 44% increase in difficulty vis-à-vis strategy 3 and a 39% reduction in average length.

Table 6 also shows that countries that use a referendum strategy, as well as those that use an intervening election strategy, have, on average, much shorter constitutions than the countries using the other strategies. They tend to have framework constitutions that define the basic institutions and the decision-making process connecting these institutions. The nations using strategy 1 tend to use a code-of-law form of constitution containing many details about preferred policy outcomes. These constitutions tend to be much longer. A code-of-law form of constitution implies a reduced distinction between normal legislation and constitutional matters vis-à-vis framework constitutions since normal legislation is put into the document. The code-of-law form, long documents, an easy amendment process, and legislative supremacy are all characteristics of the parliamentary sovereignty model that dominates the list of countries using strategy 1. New Zealand has perhaps the purest parliamentary sovereignty government in the world, and Kenya, India, Malaysia, Papua New Guinea, Botswana, and Western Samoa are not far behind. Although the countries using strategy 2 still have a fairly low index of difficulty, their much shorter constitutions indicate that a much greater divide has been crossed with respect to strategy 1 countries than the addition of a double vote with intervening election might imply.

Table 6 implies several interesting things that require emphasis. First, one can trade off between shorter length and greater difficulty to produce a similar amendment rate or use them together to produce a desired amendment rate. One can relax the level of difficulty and greatly reduce the rate of amendment simply by shortening a constitution. Second, it was determined earlier that the amendment rate is highly correlated with the degree of difficulty. It is now apparent that different amendment strategies, which reflect different combinations of assumptions about constitutionalism, have certain levels of difficulty associated with them. That is, institutions have definite (and in this case predictable) consequences for the political process.

Figure 1 demonstrates this rather clearly. The 11 nations that use the legislative supremacy strategy are grouped toward the low difficulty—high amendment rate end of the curve. These are left as an open o. The seven nations that use a referendum strategy, each indicated by an inverted triangle, are clustered toward the other end of the curve. Those that use legislative complexity are indicated by an open square, and those that use an intervening election strategy are represented by a filled-in o. The clustering of the various countries by amendment strategy shows that the averages reported in Table 6 represent real tendencies and are not merely statistical artifacts produced by averaging. The average for the American states is shown by an s.

Figure 1 shows the relationship between difficulty of amendment and amendment rate while controlling for the effects of length, which has the effect of shifting the curve upward a bit from that created by using raw index scores. The shifted curve is almost hyperbolic, which means that the relationship between difficulty of amendment and amendment rate can be approximated by the equation for a hyperbolic curve, x = 1/y.

An analysis of American state constitutions, with the difficulty of amendment held roughly constant by the similarity in their formal processes, reveals a relationship between the length of a constitution and its amendment rate that is described by a linear curve of best fit with a slope of .60—which is to say that on average, for every additional 10 thousand words, the amendment rate goes up by six-tenths of an amendment per year. The curve of best fit for the national constitutions, when controlling for the difficulty of the amendment process and when excluding the extreme cases of New Zealand and Japan, has a slope of .59. Those writing a new constitution can expect with some confidence, therefore, that there will be about a .60 increase in the amendment rate for every 10-thousand-word increase in the length of the doc-

Finally, we might conclude from Table 6 that both the length of a constitution and the difficulty of amendment may be related to the relative presence of an attitude that views the constitution as a higher law rather than as a receptacle for normal legislation. Certainly it seems to be the case that a low amendment rate can either reflect a reliance on judicial revision or else encourage such reliance in the face of needed change. It is possible that the great difficulty faced in amending the U.S. Constitution led to heavy judicial interpretation as a virtue in the face of necessity.

The theory of constitutional amendment advanced here has posited a connection between the four methods of constitutional modification. Propositions 1–4 developed the concept of amendment rate in such a way that we were able to show an empirical relationship between the formal amendment of a constitution and its complete replacement. Propositions 5–8 used amendment rate to relate these two methods of modification to judicial and legislative

alteration.⁶ At this point I can now systematically include these last two methods in the overall theory. Toward that end, it is worth reconsidering briefly propositions 5–8 in the light of my findings on the amendment process in national constitutions.

PROPOSITION 5. A low amendment rate, associated with a long average constitutional duration, strongly implies the use of some alternate means of revision to supplement the formal amendment process.

The countries that have an amendment rate below <#> (defined as .75-1.24 for national constitutions) and also have a constitution older than the international average of 51 years include Australia, Finland, Ireland, and the United States. The proposition implies that these four countries either have found an alternative means (judicial review in the United States) or are under strong pressure to find another means. Denmark, Germany, Iceland, Italy, and Japan are all within a few years of falling into the same category, and if the proposition is at all useful, they should experience progressively stronger inclinations toward either a more active judiciary or a new constitution in the coming decades. A trend toward an active judiciary is already well advanced in Germany and is also becoming apparent in Japan.

Proposition 6. In the absence of a high rate of constitutional replacement, the lower the rate of formal amendment the more likely the process of revision is dominated by a judicial body.

In the absence of further research, there is only indirect evidence for this proposition. Table 6 shows that the lower the rate of amendment, the less the legislature dominates. The executive is usually not a major actor in a formal amendment process, so we are left with the judiciary.

PROPOSITION 7. The higher the formal amendment rate, a) the less likely the constitution is being viewed as a higher law, b) the less likely a distinction is being drawn between constitutional matters and normal legislation, c) the more likely the constitution is being viewed as a code, and d) the more likely the formal amendment process is dominated by the legislature.

Discussion of Table 6 has supported all parts of this proposition.

Proposition 8. The more important the role of the judiciary in constitutional modification, the less likely the judiciary is to use a theory of strict construction.

In the absence of further research, proposition 8 is a prediction to be tested.

CONCLUSION

I have examined two sets of constitutions. Each set is composed of documents that are taken seriously as constitutions. Every document in these two sets has a formal amendment process that is self-sufficient; that is, it depends on no other constitution to carry

out a formal amendment of itself. The two sets of constitutions examined together comprise at least three-fourths of the existing documents defined by these two characteristics.⁷

A comparative, empirical study of the amendment process in these 82 documents leads to four specific conclusions about the amendment process, as well as four more general conclusions about constitutions. The first specific conclusion is that the variance in amendment rate is largely explained by the interaction of two variables: the length of the constitution and the difficulty of the amendment process.

Second, it is possible to manipulate these two variables to produce more or less predictable rates of amendment. The strong linear effects of length and the hyperbolic curve that describes the effects of difficulty—these two together allow us to formulate an equation that generates a pattern of amendment rates close to what we found empirically. If we let A represent the amendment rate, D the score on the index of difficulty, and L the length of a constitution in words, the equation representing their interrelationship is

$$A = [1/D + ((L/10,000) \times .6)] - .3.8$$

Third, there is evidence that the amendment rate affects the probability that a constitution will be replaced and that a moderate amendment rate (between .75 and 1.25 amendments per year) is conducive to constitutional longevity.

Fourth, beyond a certain point, making the amendment process more difficult is an "inefficient" way to keep the amendment rate in the moderate range. Rather, it is easiest to do so by avoiding the extremes of either the legislative dominance or the referendum strategies and combining either the legislative complexity or the intervening election strategy with a relatively short document (10–20 thousand words).

Among more general conclusions, the first is that institutions have consequences and that the effects of institutional definitions in constitutions can be studied empirically.

Second, the similarity in amendment patterns between the American state constitutions and the national constitutions raises the possibility that for other aspects of constitutional design, one set of documents may be useful in developing propositions for studying the other set and therefore that there are basic principles of constitutional design operating independently of cultural, historical, geographic, and short-term political considerations.

Third, the first two general conclusions together suggest the possibility of discovering a set of principles that can be used to design constitutions with predictable results.

Fourth, the study of the amendment process strongly suggests that constitutional institutions cannot be studied in isolation from each other. Just as the operation of the legislature may strongly affect the patterns we find in the amendment process, the design of the amendment process may affect the operation of the court; and seemingly unrelated aspects of a constitution (e.g., its length and formal amendment process) may be linked in their consequences.

Finally, it is interesting that Buchanan and Tulloch's rational cost analysis receives some empirical support, although with a twist. Their general principle holds that constitutional choice rests on a tradeoff between decision costs and external costs (Buchanan and Tulloch 1965). Since constitutions contain important political settlements, any amendment carries with it the danger of serious externalities. Although an apparently rational actor might seek a very difficult amendment process in order to minimize externalities, such a one might also attempt to minimize externalities by constitutionalizing their interests, since the more specific the policy content of the constitution on a topic, the less danger there is that unwanted externalities will be imposed. This latter analysis would imply a fairly easy amendment process. However, a process that allows one actor to safeguard its interests allows all actors to do so. The data in this study indicate that the more policy content there is in a constitution, the longer it becomes. Both an easy amendment process (which leads to greater length and thus a higher amendment rate) and a very difficult amendment process (which leads to a very low amendment rate) produce a higher probability that a constitution will be replaced entirely. Thus, the two short-range types of behavior likely to be engaged in by a rational actor are irrational in the long-run, because when a constitution is replaced, everything is once again up for grabs—a situation in which constitutional safeguards against external costs are no longer in effect at the very time externalities are threatened on all serious political matters. Therefore, a truly rational actor would seem to be one who attempts to avoid constitutional replacement and instead avoids an amendment rate that is too high or too low. This would seem to argue for constitutional brevity and a moderately difficult amendment process on grounds of rationality.

APPENDIX A

Table A-1 presents an index to measure the degree of difficulty associated with each amendment process.

APPENDIX B

Table B-1 lists the numbers assigned by the index to every step required by the amendment processes.

APPENDIX C

Table C-1 shows the index of difficulty scores tabulated for the national constitutions of 32 countries along with other constitutional characteristics.

TABLE A-1
Basic Data on American Constitutions, 1991

STATE	NO. OF CONSTITUTIONS	AVG.	CURRENT CONSTITUTION SINCE		ORIGINAL LENGTH IN WORDS	TIMES	AMENDMENT RATE
Alabama	6	29	1901	90	65,400	726	8.07
Alaska	i ·	35	1959	32	11,800	22	.69
Arizona	i	80	1912	79	28,900	109	1.38
Arkansas	5	31	1874	117	24,100	76	.65
California	2	72	1879	112	21,400	471	4.21
Colorado	1	115	1876	115	22,000	115	1.00
Connecticut	4	54	1965	26	8,800	25	.96
Delaware	3	72	1897	94	19,000	119	1.27
Florida	6	25	1969	22	18,900	53	2.41
Georgia	10	21	1983	8	26,000	24	3.00
Hawaii	1	41	1959	32	16,800	82	2.56
Idaho	1	102	1890	101	18,800	107	1.06
Illinois	4	43	1971	20	12,900	6	.30
Indiana	2	88		140			
lowa	2	73	1851 1857	134	9,100 9,700	38 48	.27 .36
Kansas	1	132					
	4	50	1861	130	10,200	87 80	.67
Kentucky	11		1891	100	21,800	29	.29
Louisiana		16	1975	16	47,300	27	1.69
Maine	1	172	1820	171	10,100	157	.92
Maryland	4	54	1867	124	25,200	200	1.61
Massachusetts	1	211	1780	211	11,600	116	.55
Michigan	4	39	1964	27	18,600	16	.59
Vinnesota	1	134	1858	133	8,500	112	.84
Mississippi	4	44	1890	101	20,100	102	1.01
Missouri	4	43	1945	46	39,300	74	1.61
Montana	2	51	1973	18	11,600	15	.83
Vebraska	2	63	1875	116	16,100	189	1.63
Vevada	1	127	1864	127	14,100	108	.85
New Hampshire	2	108	1784	207	8,000	142	.69
New Jersey	3	72	1948	43	16,400	39	.91
New Mexico	1	79	1912	79	22,000	120	1.52
New York	4	54	1895	96	26,800	207	2.16
North Carolina	3	72	1971	20	10,300	27	1.35
North Dakota	1	102	1889	102	18,100	125	1.23
Ohio	2	95	1851	140	14,200	145	1.04
Oklahoma	1	84	1907	84	58,200	133	1.58
Oregon	1	132	1859	132	11,200	188	1.42
^o ennsylvania	5	43	1968	23	20,800	19	.83
Rhode Island	2	108	1843	148	7,400	53	.36
South Carolina	7	31	1896	95	21,900	463	4.87
South Dakota	1	102	1889	102	21,300	97	.95
lennessee	3	65	1870	121	11,100	32	.26
Texas	5	29	1876	115	28,600	326	2.83
Jtah	1	95	1896	95	13,900	77	.81
/ermont	3	71	1793	198	5,200	50	.25
/irginia	6	36	1971	20	18,100	20	1.00
Vashington	1	102	1889	102	16,300	86	.84
Vest Virginia	2	64	1872	119	15,900	62	.52
Visconsin	1	143	1848	143	11,400	124	.87
Vyoming	i	101	1890	101	20,800	57	.56
Mean	2.9	77	1896	95	19,300	117	1.23

Sources: The data in this appendix are based on James Q. Dealey, Growth of American State Constitutions (New York: Da Capo, 1972); Walter F. Dodd, The Revision and Amendment of State Constitutions, 2d ed. (New York: Da Capo, 1970); Daniel J. Elazar, American Federalism: A View from the States, 2d ed. (New York: Thomas Y. Crowell, 1972); Fletcher M. Green, Constitutional Development in the South Atlantic States, 176–1860 (New York: Da Capo, 1971); Ellis Paxson Oberholzer, The Referendum in America (New York: Da Capo, 1971); Harold W. Stanley and Richard G. Niemi, Vital Statistics on American Politics (Washington: Congressional Quarterly, 1992); Albert L. Sturm, Thirty Years of State Constitution-Making: 1938–1968 (New York: National Municipal League, 1970).

TABLE B-1

An Index for Estimating the Relative Difficulty of an Amendment Process

ACTION	CONSTITUTIONAL REQUIREMENT	ADD
	Initiation Requires Action by	
1	An executive	.25
2	A special appointed body	.50
1 2 3	A special elected body	.75
•	A unicameral legislature	
	Legislative approval	
4	By a majority of 1/2 + 1	.50
5	Twice using 1/2 + 1	.50
6	By an absolute majority ^a	.65
7	Twice by absolute majority	.65
8	By a 3/5 majority	.65
9	Twice by 3/5 majority	.65
10	By a 2/3 majority	.80
11	By a 3/4 majority	.90
12	Twice by a 2/3 majority	1.75
13	If an election is required between	.25
	two votes	
	Action by a bicameral legislature	
	Legislative approval	
14	By a majority of 1/2 + 1	1.00
15	Twice using 1/2 + 1	1.00
16	By an absolute majority	1.25
17	Twice by absolute majority	1.25
18	By a 3/5 majority	1.25
19	Twice by a 3/5 majority	1.25
20	By a 2/3 majority	1.60
21	By a 3/4 majority	1.80
22	Twice by a 2/3 majority	3.55
23	If an election is required between	.50
	two votes	
	A petition	
24	Of 0-250,000 voters	3.00
25	By 250,000-500,000 voters	3.50
26	By more than 500,000 voters	4.00
	Multiple state	
27	Legislatures, 1/2 + 1	2.00
28	Conventions, 1/2 + 1	2.00
29	Legislatures or conventions, 2/3	3.00
30	Legislatures or conventions, 3/4	3.50
a" Abcolute =	najority/' is used to indicate a requirement for ann	rough by

^{a"}Absolute majority" is used to indicate a requirement for approval by 1/2 + 1 of the *entire body*, whereas "1/2 + 1" indicates a requirement for approval by 1/2 + 1 of those voting.

Notes

Research for this article was supported by a grant from the Earhart Foundation of Ann Arbor, Michigan. Assistance in data analysis was provided by Patricia Gail McVey of the University of Texas School of Public Health.

- 1. While this was the first explicit amendment process in a state constitution, a formal amendment process was first used in William Penn's 1678 Frame of Government, which may explain why Pennsylvania was the first state to adopt one (see Viel 1992, 11–12).
 - 2. These data can be found in Sturm 1982, 90.
- 3. The test for curvilinearity using cross-national data is neither strictly comparable to that used for the American states nor an adequate test of the relationship using national constitutions. Whereas the entire constitutional history for all

TABLE B-1 continued

An Index for Estimating the Relative Difficulty of an Amendment Process

	CONSTITUTIONAL	
ACTION	REQUIREMENT	ADD
	Approval Requires	
31	Action by an executive	.50
	Approval by a special body	
32	1/3 or less	.25
33	1/2 + 1	.50
34	Absolute majority	.65
35	3/5 majority	.65
36	2/3 majority	.80
37	3/4 majority	.90
38	3/4 majority	.90
	If any of the above acts a second time	.50
	Action by a unicameral legislature	
	Legislative approval	
39	1/3 majority or less	.25
40	1/2 + 1	.50
41	Twice by 1/2 + 1	.50
42	Absolute majority	.65
43	Twice by absolute majority	.65
44	3/5 majority	.65
45	Twice by 3/5 majority	.65
46	2/3 majority	.80
47	3/4 majority	.90
48	If an election is required between	.25
	two votes	
49	Legislative approval twice by 2/3 majorit	y 1.75
	Action by a bicameral legislature	
	Legislative approval	
50	1/3 majority or less	.50
51	1/2 + 1	1.00
52	Absolute majority	1.25
53	Twice by absolute majority	1.25
54	3/5 majority	1.25
55	Twice by 3/5 majority	1.25
56	2/3 majority	1.60
57	3/4 majority	1.80
58	Twice by 2/3 majority	3.55
59	If an election is required between	.50
	two votes	
00	A popular referendum	4 50
60	1/2 + 1	1.50
61	Absolute majority	1.75
62	3/5 or more	2.00
00	Multiple state	0.00
63	Legislatures, 1/2 + 1	2.00
64 65	Coventions, 1/2 + 1	2.00
65 66	Legislatures or conventions, 2/3	3.00 3.50
66 67	Legislatures or conventions, 3/4	3.75
67	Majority of voters <i>and</i> majority of states	3.75
60		4.00
68	Unanimous approval by state	4.00
	governments	

50 American states was used, only the most recent period of constitutional stability exceeding 15 years was used for the cross-national data. The arbitrary use of a 15-year minimum may well exaggerate the average longevity of national constitutions, and the use of only the most recent minimum period may weaken the results.

4. Figure 1 was generated using the statistical package

TABL	E C-1				
Racic	Data on	Selected	National	Constitution	25

	AMENDMENT	INDEX OF	AMENDED LENGTH		TIME
COUNTRY	RATE	DIFFICULTY	IN WORDS	YRS.	PERIOD
Argentina	1.04	2.10	10,600	87	1853-1940
Australia	.09	4.65	11,500	91	190192
Austria	6.30	.80	36,000	17	197592
Belgium	2.30	2.85	10,700	15	197388
Botswana	2.44	1.30	35,600	18	1966–84
Brazil	6.28	1.55	58,400	18	1969–87
Chile	.64	3.05	24,200	45	1925-70
Columbia	1.73	2.75	25,100	95	1886-1981
Costa Rica	1.26	4.10	15,100	33	1949–82
Denmark	.17	2.75	6,000	39	1953-92
Finland	.86	2.30	18,300	73	1919–92
France	.19	2.50	6,500	24	1968-92
Germany	2.91	1.60	22,400	43	1949–92
Greece	1.32	1.80	22,100	17	1975–92
Iceland	.21	2.75	3,800	48	1944-92
India	7.29	1.81	95,000	42	1950-92
Ireland	.55	3.00	16,000	55	1937–92
Italy	.24	3.40	11,300	46	194692
Kenya	3.28	1.00	31,500	18	1964-81
Japan	0.00	3.10	5,400	47	1945–92
Luxembourg	1.80	1.80	4,700	19	196887
Malaysia	5.18	1.60	91,400	35	1957–92
New Zealand	13.42	.50	180,000	40	194787
Norway	1.14	3.35	6,500	178	1814–1982
Papua New Guinea	6.90	.77	53,700	17	197592
Portugal	6.67	.80	26,700	15	1976–91
Spain	.18	3.60	8,700	24	1968-92
Sweden	4.72	1.40	40,800	18	1974–92
Switzerland	.78	4.75	13,300	119	18731992
United States	.13	5.10	7,400	203	17891992
Venezuela	.24	4.75	20,500	25	196792
Western Samoa	.95	1.80	22,500	22	1962-84
Average	2.54	2.50	29,400	52	

Note: Cross-national constitutional data have been taken from the constitutions themselves and from commentaries on these documents, found primarily in Albert P. Blaustein and Gisbert H. Flanz, Constitutions of the Countries of the World, 19 vols. (Dobbs Ferry, NY: Oceana, 1987) and supplements.

STATA, and the figure was then reproduced by hand using Geotype overlay techniques to enhance readability.

5. These broad categories were constructed using the theoretical premises developed herein and thus are independent of any categorization schemes developed previously by others. For an instructive comparison, see Lijphart 1984, 189–91. In order for a country to be included, it had to have at least one 15-year period free of military rule or serious instability, during which constitutionalism was taken seriously. Reliable data on the number and nature of amendments for that country also had to be available to the researcher. The unavailability of such data explain the absence of the Netherlands, for example, or for Austria before 1975.

6. On this topic, see Antieu 1982 and McWhinney 1956.

7. Canadian provincial and Australian state constitutions are prominent among those remaining to be examined. Also, Israel, Canada, and the United Kingdom, although lacking a simple written constitution, remain to be included. The problem in each of these three cases lies in determining what has constitutional status. An initial attempt to do so, using the content of the New Zealand Constitution as a template, yielded the following very preliminary estimates for two of these legislative supremacy countries:

	Israel	United Kingdom
Amendment rate	2.5+	7.5+
Index of difficulty	.50	.60
Length	10,000+	250,000+
Relevant years	1949–91	1900–1991

8. One part of the equation factors the effects of length in by dividing the number of words in the constitution by 10,000 and multiplying by .60. The second part approximates the effects of amendment difficulty by using the formula for a hyperbolic curve: A = 1/D. However, this is only approximate, and subtracting .30 from the effects of amendment difficulty results in the curve of best fit for the raw data scores.

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EXPECTATIONS AND PREFERENCES IN BRITISH GENERAL ELECTIONS

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Te address two questions: How do people form their expectations about the likely winner of the next general election? and What are the links between expectations and votes? Using data collected by the Gallup organization in Great Britain, we find that the expectations formation process (1) has a significant inertia component but also a rapid adjustment to current information; (2) reflects voters' ability to translate economic expectations into political forecasts; and (3) is "time-bounded," possessing special characteristics immediately before and after a general election. The analysis also confirms the existence of a small bandwagon effect, whereby expectations that one party will win inflate that party's vote. The ability of voters to make reasonable forecasts without being unduly influenced by their own preferences suggests that under normal circumstances voters are expressing real preferences and not simply following the crowd.

The idea that observed contemporary behaviors are strongly linked to expectations about future outcomes has become so central for economists that much of economic theory has been rewritten to take explicit account of this concept (Begg 1982; Hudson 1982, chap. 3). Political scientists have also shown a growing interest in expectations, with future prospects proving to be particularly useful in studying certain aspects of electoral behavior. The now-common use of the notions of viability and electability, as well as the classification of voters as straightforward, sincere, or strategic, exemplify this trend (Abramson et al. 1992; Bartels 1988; Niemi, Whitten, and Franklin 1992). The simple act of voting is increasingly conceived of in terms of utilities (voters' preferences) and probabilities (voters' assessments of candidates' chances of winning).

Two crucial questions are at the nexus of the votes-expectations literature: How do people form their expectations and What are the links between expectations and votes? We address these questions with data collected by the Gallup organization. This unique data set includes not only the usual measurement of voters' preferences among parties but also a systematic tracking of voters' expectations about the outcome of the next general election. The fact that this information is available on a monthly basis for more than a decade permits systematic study of the expectations formation process and of the links between expectations and votes not only during election periods but also in less intense, interelection times.2 The data are thus attractive despite their obvious limitations.³

We shall present a simultaneous-equation votes—expectations model,⁴ paying particular attention to the expectations formation process.⁵ We then present our findings, which show a modest effect of expectations on voting behavior and stronger and more variegated effects—including "instantaneous" adjustments—of voting intentions on expectations. We

discuss the theoretical implications of our findings in the conclusion.

A VOTES-EXPECTATIONS MODEL

The votes-expectations literature has been mainly concerned with the existence and form of a pattern of reciprocal causation between voters' preferences and their assessments of the electoral campaigns (i.e., the parties' standings). Consistent with this perspective, we examine here two possibilities: (1) that voting intentions are affected by expectations about the likely winner of the next general election (bandwagon effect) and (2) that expectations about which party will win the next election are dependent on current vote intentions (whether through a sophisticated treatment of information or a simple projection effect).

Practically speaking, what is required is that we devise and estimate a simultaneous equations system formed of a vote equation and an expectations equation. As is standard in simultaneous equations models, we estimate our model using two-stage least squares. To obtain consistent estimates, we must pay particular attention to model specification and estimation, especially the less-frequently-studied expectations equation. Substantive questions concerning the appropriate specification of the model will be discussed. Identification issues, estimation strategy, and diagnostic tests are described in Appendices A and B.

The Vote Equation

To specify the vote equation of our simultaneousequation model, we build from the extensive literature on the short-term dynamics of political party popularity in Britain (Alt 1979; Clarke, Mishler, and Whiteley 1990; Clarke, Stewart, and Zuk 1986; Dunleavey and Husbands 1985; Frey and Schneider 1978; Goodhart and Bhansali 1970; Hibbs 1982; Hudson 1985; Lewis-Beck 1988; Mishler, Hoskin, and Fitzger-ald 1989; Miller and Mackie 1973; Norpoth 1991; Pissarides 1980; Sanders, Ward, and Marsh 1991). In particular, we searched for a satisfactory mix of economic and political indicators to serve as control variables whose inclusion would prevent us from inflating the impact of the expectations term. The specification we arrived at follows closely the prescription of Clarke, Mishler, and Whiteley:

A properly specified model of government popularity requires four broad classes of explanatory variables: . . . the long-term predispositions of voters, . . . issue perceptions which include retrospective and prospective thinking about economic and noneconomic issues, . . . leadership images, which relate both to the government and opposition parties, . . . [and] various systematic and random shocks to the system. (1990, 66)

Our specification is shown in equation 1:

$$VCL_{t} = \beta_{0} + \beta_{1}VCL_{t-1} + \beta_{2}MPSHARE_{t} + \beta_{3}UNEMP_{t-1}$$

$$+ \beta_{4}INF_{t-1} + \beta_{5}PEXP_{t} + \beta_{6}LEADERS_{t}$$

$$+ \beta_{7}FALK + \beta_{8}WCL_{t}, \qquad (1)$$

where VCL is the percent intending to vote Conservative minus percent intending to vote Labour (percent calculated after excluding don't knows, undecideds, refusals); MPSHARE is the ratio of the percent intending to vote for a minor party to the sum of the percents intending to vote Conservative or Labour; UNEMP is the unemployment rate, monthly, seasonally adjusted; INF is the monthly inflation rate calculated from the adjusted retail price index; PEXP is the percent thinking that personal economic conditions will improve over the next 12 months minus percent thinking that personal economic conditions will worsen over the next 12 months; LEADERS is the percent approval of the prime minister minus percent approval of the opposition leader. FALK is a dummy variable equal to 1 for May–June 1982 (the Falklands War), 0 otherwise; and WCL is the percent thinking that the Conservatives are most likely to win the next general election minus percent thinking that Labour is most likely to win.⁶

The coding of the variables and the form of the equation deserve brief explanation. Our major concern, of course, is with the impact of expectations about the winner—WCL.⁷ It is assumed that this impact is instantaneous, that is, that the gap measured in the current poll is already reflected in vote intentions. The assumption of immediate effects will be discussed in considerable detail, when we make a similar assumption about the effects of vote intentions on expectations. The expectations variable itself is expressed as a percentage difference because the popularity gap between the parties is at the core of thinking of one as a potential winner and the other as a loser. Likewise, the vote intentions variable is measured as a percentage difference.

The most interesting of the remaining variables in the vote equation is VCL_{t-1} . Under normal circum-

stances vote intentions "do not move abruptly from one moment to the next one, but at a sluggish pace instead" (Norpoth 1991, 146). We include the lagged dependent variable to capture this inertia component. Following Norpoth, this variable may be conceived as reflecting a "short-term memory" process, whereas our constant term may be an indicator of the "long-term component of popularity" (pp. 146–47).

Based on Mishler, Hoskin, and Fitzgerald's (1989) results, we also checked, by including the MPSHARE variable, whether the party balance between Conservatives and Labour was affected by the electoral fortunes of third parties. As theory and recent empirical work suggest that voters' evaluations of government economic performance may be a blend of both objective conditions and subjective perceptions,8 we added to our vote equation the unemployment rate, the inflation rate, and a variable reflecting voters' prospective assessments of their personal economic situations over the next 12 months. 9 The inclusion of unemployment and inflation appears sensible because both are "highlighted by the media, and salient to the voter" (Clarke, Mishler, and Whiteley 1990, 66; see also Mosley 1984). 10 The subjective perception variable (PEXP) was selected after various tries with similar variables. 11

Inclusion of the variable LEADERS follows what has become standard practice in British studies. Given the finding by Mishler, Hoskin, and Fitzgerald that "the relationship between leader satisfaction and party support is highly significant" (1989, 228), it is also consistent with our goal of not inflating the expectations term in our vote equation. Moreover, this inclusion also seems justified on theoretical grounds, as "recent research suggests that although related, leadership satisfaction and party popularity are conceptually distinct and empirically separable (Clarke, Mishler, and Whiteley 1990, 68). Finally, the Falklands War exemplifies a major disruption to the usual pattern of stability, and a variable was included to take it into account.

The Expectations Equation

In the search for an appropriate specification for our expectations equation, we faced more difficulties than for the vote equation. First, while popularity functions have long been available and have been extensively studied, such is not the case for expectations; there is no extensive body of research on expectations comparable to that for vote intentions. Second, our dependent variable—the expectations question—lacks the immediate time horizon of the vote question (who will win "the next general election" versus one's vote if there were a general election "tomorrow").

Some insight can be derived from work on expectations in economics. Early attempts to model the expectation formation process centered on the "adaptive" expectations model, where lagged values of a variable were used to predict current inflation or other relevant economic quantities (e.g., Cagan 1956 and Nerlove 1958; a good summary is Nerlove, Gre-

ther, and Carvalho 1979, chap. 10). These initial attempts were subsequently criticized as being "atheoretical" and inconsistent with any explicit optimizing behavior on the part of individuals within the model. Later research in economics argued that expectations were formed "rationally" or in a "model consistent" manner (Lucas 1973; Nerlove, Grether, and Carvalho 1979, chap. 10; Sargent and Wallace 1975); that is, expectations of an economic variable should be based upon all the variables in the model and not just the past values of that variable. 14

Our approach to modeling the expectations formation process lies somewhere between these two polar cases. Like the early critics of the "adaptive" expectations process, we recognize that more than lagged values of a variable are required to explain how individuals make forecasts of quantities they find important. However, we also recognize that the rational expectations model used in economics may be too strong for our explicitly political analysis. In particular, an empirical economic model that incorporates the rational expectations assumption will typically include a series of economic variables in the expectations equation. This is because economic agents are assumed to use these variables in forecasting, say, inflation or unemployment. Given the time horizon of the economists' expectations model, this strategy makes perfect sense. One is typically interested in forecasting inflation or any other economic variable for the *next* period.

In our political model the argument is different. The agents in our model are voters, and they are often asked to forecast an event (the election outcome) many periods from now. Consequently, economic information in the form of inflation and unemployment rates will not be particularly useful since current values provide very little predictive power for economic variables possibly two or three years from today. 15 Other restrictions to the expectations equation appear plausible as well. The leader variable used in the vote equation is strongly retrospective rather than prospective, ¹⁶ and the minor party share variable certainly does not capture the complexities related to the translation of small parties' short-term success into major parties' future electoral prospects. Finally, we theorize that single events such as the Falklands War are unlikely to influence expectations except insofar as they leave their imprint on the parties' standings.

The variables that will be important in forming our "political" expectations are those with an avowedly political component, including such factors as parties' levels of support, party reputation, and the period in the electoral cycle, plus one economic variable that asks respondents to look forward a full year. The following equation reflects this mode of thinking:¹⁷

WCL_t =
$$\alpha_0 + \alpha_1$$
WCL_{t-1} + α_2 POSTELECT * WCL_{t-1}
+ α_3 VCL_t + α_4 PREELECT * VCL_t + α_5 PEXP_t + α_6 D8387,

where WCL, VCL, and PEXP are defined as in equation 1; POSTELECT is a dummy variable equal to 1 in

May–July 1979 and July–September 1983, 0 otherwise; PREELECT is a dummy variable equal to 1 in February–April 1979 and April–June 1983 and 1987, 0 otherwise; and D8387 is a dummy variable equal to 1 from July 1983 to June 1987, 0 otherwise.

The equation suggests, first of all, that there is an inertia component to expectations about the winner of the next election. Some individuals—through general lack of attention to politics, a failure during specific periods to absorb new information about the parties, or a heavy discounting of new information for partisan or other reasons—fail to reassess their expectations (or do so only after some delay). The lagged dependent variable taps this inertia.

In addition, one might include an inertia component in the expectations formula as used here because of the form of the measurement available to us. Our dependent variable reflects the aggregation of individual responses to a question allowing only nominal choices of the most likely winning party. Yet we would hypothesize that individuals form their expectations on a continuous scale (e.g., from 0 to 100). Thus, when asked about the most likely winner of the next election, one might considerably reassess the subjective probability that a party will defeat its opponents without crossing the 50% threshold. Such individuals, constrained by the format of the question in the Gallup surveys, will appear unchanged even though they have thoroughly reassessed a party's chances. Thus, in addition to capturing inattentiveness to political events, the lagged term will reflect hypothesized fine changes that are masked by the available measurement.

The second term in the expectations equation reflects the fact that electoral expectations—including lagged effects—are not "time-free." They are, on the contrary, "time-bounded" by the event about which they are formed, namely, the next election (see, e.g., Lewis-Beck and Skalaban 1989, 150-51). One aspect of this timing is that voters adopt a different perspective in forming their expectations in the months immediately after an election. To illustrate this point, consider the 1983 election. In the last Gallup poll available, fully 87% of the respondents believed that the Tories would win the election, and only 5% thought Labour would win. (WCL, our dependent variable, reaches its highest point, 82%, at that time.) In the month immediately following the election, the same percentages were, respectively, 49 and 16. After an election, voters clearly keep in mind that a new ball game has begun. To represent this discontinuity in expectations after the election, we included an interactive term, POSTELECT * WCL_{t-1}, in our

Of greatest interest in the expectations equation are the next two variables, which together constitute an adjustment for *current* voting intentions. Theoretically, we are adopting here the portion of the rational expectations view that says that individuals "use current available and relevant information in forming their expectations and do not rely purely upon past experiences" (Shaw 1983, 47). Specifically, we are

assuming that *current* information about vote intentions affects perceptions about which party is likely to win the next election.¹⁹ Of course, in a literal sense individuals cannot be reacting to the results of a survey that is only then being conducted. But people do have very recent, even simultaneous, bases for their answers.

Information about candidates, parties, and politics bombards voters on a daily basis. Some of it comes directly in the form of news reports from the media; some of it is filtered through friends and acquaintances. Some of it is directly in the form of horse-race reports;²⁰ some of it is in the form of stories about candidates' characteristics and activities. Any single story, of course, may reach only a small minority of the population, and only a fraction of those may be persuaded to update their expectations about which party is likely to win the next election. Thus, as indicated, there is a degree of inertia in people's expectations. But along with this inertia, very recent and contemporary events and reports are indeed likely to be incorporated into current expectations.²¹

In addition, individuals may reason that their current evaluations of the parties are likely to be shared by others. For example, a Labour identifier dissatisfied with his party's leaders and policies to the point of expressing a preference for the Tories may quite reasonably believe that other Labourites feel the same way (and that Conservatives, on the other hand, feel good about their party) and—as a consequence—feel that his party's electoral prospects are quite poor. The correspondence over time of voting intentions and expectations may thus be the result of a quite sophisticated treatment of political information.

Of course, simple projection may occur as well, as voters unconsciously align their expectations with their preferences. To this degree, also, expectations will mirror the current proportions intending to vote for each party. Unfortunately, it may be impossible ever to separate these processes completely, even with individual-level data. We can show quite convincingly, however, that what we are tapping goes well beyond that kind of projection. ²²

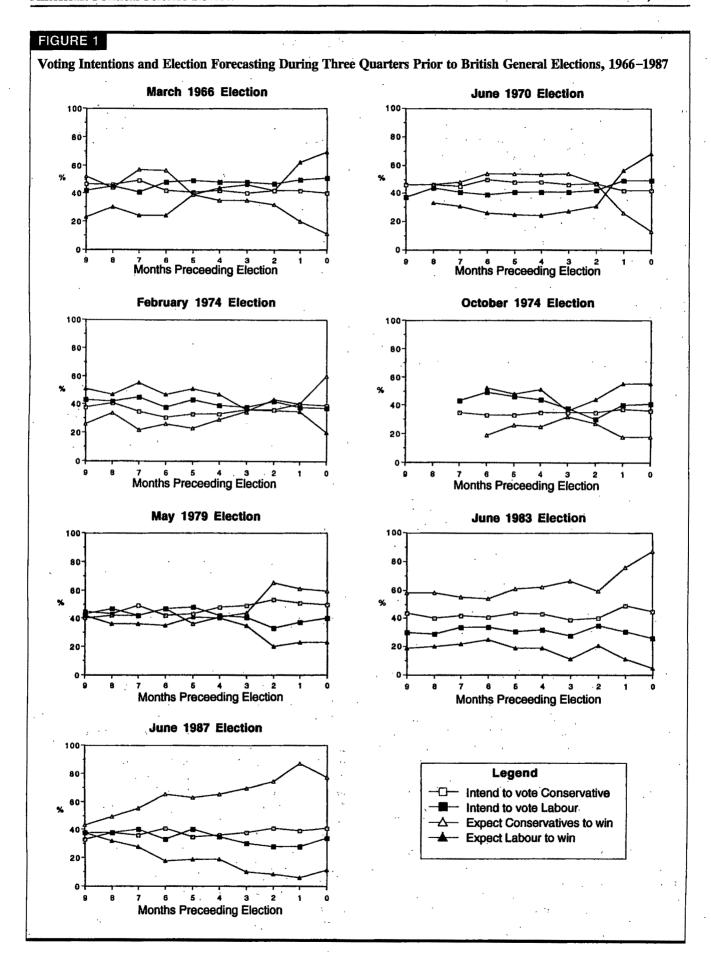
A hypothesis of simple projection would make no allowance for variations between election and interelection periods. But there are, as noted, strong theoretical reasons to believe that the process of forming expectations will have a different character in the neighborhood of the forthcoming election. First, as election day approaches, information about the parties' standings becomes a cheap and easily available commodity. Second, the incentive to use this information increases, as some voters might want to engage in tactical voting. If the degree of connection represents more than mindless projection, the electoral period should be characterized by a more pronounced disconnection between preferences and expectations as voters adjust their views about party's winning chances more quickly and more sensitively to relevant political information than in more quiet, nonelection times.

That wishful thinking is harder to maintain as the

election approaches can be seen in Figure 1, which show the relationship between voting intentions and expectations during the nine months prior to each of the general elections in Britain between 1966 and 1987. In every case, one finds the expected disconnection between votes and expectations, as even loyal partisans are forced to admit that they will lose their electoral bid. The form, duration, and "spread" of the party's chances of winning vary from one election to the other, but the graphs seem to show a consistent pattern, one that would not be evident if projection was the only force at work. To take this pattern into account, we have included an interactive term, PREELECT * VCL_t, 25 to capture the disconnection between preferences and expectations during the electoral period. 26

This disconnection should manifest itself in a positive coefficient for the PREELECT * VCL, variable. To understand this, consider an example in which the Conservatives have the support of 50% of the electorate and Labour the support of 40% during the interelection period (VCL = 10) and in which there is a great deal of wishful thinking. Conservative and Labour supporters alike may suppose their party will win, suggesting a coefficient of approximately 1 between vote intentions and expectations of which party will win, so that net of other influences, WCL should be approximately 10 as well. Now suppose that as the election approaches, there is no change in vote intentions: it is still the case that the Conservatives have the support of 50% of the electorate and Labour the support of 40%. Those who support the Conservatives have no incentive to change their minds about which party should win the election; all 50% should still expect a Conservative victory. However, the connection between vote intentions and expectations should weaken for Labour supporters. Some of the 40%, despite their preference, will concede that the Conservatives are likely to win. This weakening of the connection between preferences and expectations means that a given vote-intention difference between the Conservatives and Labour (VCL remains 10) should translate into a larger proportion of individuals expecting the advantaged party to win (WCL becomes greater than 10); that is, the coefficient of the interaction term should be positive.27

The two remaining variables in the expectations model have similarities to adjustment and inertia components, respectively. First, we include prospective economic thinking because the link between the state of the economy and party popularity has been demonstrated repeatedly by scholars in any number of countries. And inasmuch as these judgments are about the next 12 months, they are less subject to the problems we pointed to earlier in justifying the exclusion of unemployment and inflation variables. In addition, forecasting on the basis of future personal economic situations makes sense both in terms of retrospective and prospective thinking. If one forecasts outside the electoral period, the performance of the economy over the next 12 months is relevant



information, as it will influence the judgments of retrospective thinkers in a forthcoming election. In electoral periods as well, prospective perceptions are relevant as one may hypothesize that other voters evaluate the performance of the incumbent government in terms of the future.28

Finally, what might be regarded as another form of inertia would manifest itself when a political party was able to establish a reputation as a winner. This reputation can develop from the subtle interplay of consecutive victories, ²⁹ the opposition's weakness, ³⁰ leadership strength, ³¹ and past accomplishments. ³² These requirements seem to have been met during the second mandate of Margaret Thatcher as voters perceived her party as the likely winner of the next election even in light of poor showings in contemporary polls. This winner's reputation component is tapped in our model by a dummy variable equal to 1 for this period. 33 As this variable intends to capture a phenomenon that is clearly specific to the expectations formation process, namely the rigidity of expectations under particular circumstances, it is not included in the vote equation. This does not mean that rigidity is without consequences on the parties' standing, as it will have an impact through the WCL variable in the vote equation.

RESULTS

Tables 1 and 2 show our main results. 34 With respect to vote intentions (Table 1), the first point is that the mix of economic and political variables in equation 1, along with the lagged dependent variable, appear to form an appropriate group of control variables to assess the feedback effect of expectations on voting intentions. Our strategy was to devise a vote equation that would prevent us from inflating the impact of the expectations variable, and we seem to have accomplished that goal. First, the tests reported in Appendix B for possible misspecification are reassuring on this point. Second, our results are sufficiently in accord with theoretical expectations and with other empirical results to suggest that the more standard portion of the equation yields a reasonable account of vote intentions sans expectations.

As in most models of party support, for example, a lagged or autoregressive effect is clearly significant and sizable (e.g., Mishler, Hoskin, and Fitzgerald 1989).35 That some—but not necessarily all—economic factors are significant is a common result (Clarke, Mishler, and Whiteley 1990, 69-80; Mishler, Hoskin, Fitzgerald 1989, 231; Norpoth 1991, 151–52). Likewise, other studies have found only small effects of minor-party variables, whereas comparative leadership evaluations have been found to be substantial (Clarke, Mishler, and Whiteley 1990, 71; Mishler, Hoskin, and Fitzgerald 1989, 231). There are some differences between our results and those of other studies, but they are most likely attributable to different definitions of the sample period or to differences in variable definitions.

TABLE 1

Voting Intentions in Britain as a Function of Economic, Leadership, System Factors, and Vote **Expectations**

VARIABLE	COEFFICIENT
Intercept	3.69** (1.25)
Vote intention (VCL) at time t - 1ª	.21** (.06)
Minor party share at time t ^b	06 (.06)
Unemployment rate at time t - 1°	33** (.09)
Inflation rate at time t - 1 ^d	.08 (.42)
Personal economic expectations at time t ^e	.10** (.04)
Leadership approval at time tf	.27** (.03)
Falklands War period ^g	4.64* (2.73)
Forecast of expected winner (WCL _t) ^h	.10** (.02)

Note: $R^2=.91$, N=138. Entries are unstandardized regression coefficients; standard errors are in parentheses. Dependent variable (VCL_t) is vote intention calculated after excluding don't knows, undecideds, and "refusals"). See also n. 10.

^a% intending to vote Cons. – % intending Lab. ^b% minor party/(% Cons. +% Lab.).

Monthly, adjusted seasonally.

Monthly rate, retail price index.

"Monthly rate, retail price ind

*Instrumental variable. Instrument is the predicted value generated by regressing WCL on all exogenous variables in the system; WCL is defined in Table 2. See Appendix A for details.

*p < .05.
**p < .01 (one-tailed test).

Assuming, then, that our model is a reasonable one, we find a relatively small but clearly statistically significant effect of expectations on voting intentions. Every 10-point gap between the percentages thinking that the Conservatives rather than Labour will win the next election contributes one point to the electoral margin itself. Since 20-to-30-point gaps are not infrequent even in the interelection periods (see Figure 1) and since the margin of vote intentions is often fairly small, such an impact is far from negligible. Of course, one should not overestimate the bandwagon effect either. Even in the weeks immediately prior to the election, when the expectation gap may reach as high as 50-60%, the change in WCL from the interelection period may only account for an additional two or three percentage points. Moreover, as we shall shortly note, expectations themselves are highly dependent on vote intentions.³⁷ Nonetheless, it is clear that the bandwagon effect, while relatively small in

TABLE 2

Forecasts of the Expected Winner of the Next General Election in Britain as a Function of Economic Expectations, Vote Intentions, and **Electoral Periods**

VARIABLE	COEFFICIENT
Intercept	.78 (1.16)
Forecast of expected winner (WCL) at time $t - 1^a$.54** (.05)
Postelection * expected winner forecast (t - 1) ^b	30** (.08)
Vote intention (VCL _i) ^c	1.33** (.18)
Preelection * vote intention (VCL) ^d	.92** (.24)
Personal economic expectations at time t ^e	.21** (.09)
Conservative winning reputation ^f	9.39** (1.94)

Note: R2 = .91, N = 138. Entries are unstandardized regression coefficients; standard errors are in parentheses. Dependent variable (WCL,) is forecast of expected winner.

magnitude, may be of no small consequence in a closely contested election.

Turning to the expectations equation, we noted earlier that there was less certainty about how to formulate it properly. As a consequence, all of the findings are of interest. Overall, the results (Table 2) indicate that the expectations formation process (1) exhibits both significant inertia and adjustment components, (2) reflects the voters' ability to translate expected economic conditions into political forecasts, and (3) is "time-bounded," possessing special effects both before and after the electoral period.

First, note the strength of the inertia effects. To a greater extent than vote intentions, expectations about which party will win depend on prior expectations (i.e., the lagged dependent variable). An exception is in the months immediately following an election, in which the negative coefficient for the interactive term, postelect * WCL_{t-1} , means that voters recognize that the next election is a long way off and that it may therefore have little in common with what has just transpired. The consistent ninepoint bulge toward the Conservatives during the 1983–87 period suggests that there may be some long periods in which one party has a kind of built-in edge. Though additional time and data are required before we can explore this effect more thoroughly, we suspect that it may take on different forms in different times and places. 38

Next note that despite these multiple sources of inertia, winning expectations are also subject to several sources of adjustment. As anticipated, there is a strong connection between current vote intentions and expectations. A 10-point gap in the proportions presently intending to vote for the two parties typically translates into a more than 13-point difference in which party is expected to win. During the months immediately preceding an election, this effect is boosted, by the coefficient on the PREELECT * VCL. interaction term, to nearly a 25-point difference.

As noted, it is not possible completely to disentangle the degree to which the effect of VCL represents rational adjustment to contemporary information or blind projection. But the strength of the interaction term means that voters are able to disconnect themselves to a significant degree from their basic partisan loyalties when they perform political forecasts in the neighborhood of a forthcoming election. At least in the context of relative informational clarity of the immediate preelection period, losers as well as winners generally reach the same conclusion about the identity of the likely winner of the next election.

Finally, the significant coefficient for personal economic experiences, PEXP, indicates that election expectations, like vote intentions, are tied to projections of future economic conditions. In general, optimistic economic forecasts boost the incumbent's expected chances of reelection. While the precise form of this relationship may vary from time to time, the general message is that voters are able to link economic forecasts and expectations much as they link economic forecasts with their own vote intentions.

CONCLUSION

Heretofore, most evidence about the interplay between election preferences and expectations has been derived from the study of American primary elections. The emphasis has been on the effects of expectations on preferences, as well as on the role of week-to-week events and of publicity in the mass media (Abramson et al. 1992; Bartels 1987, 1988; Brady and Johnston 1987; Shanks et al. 1985). This emphasis makes considerable sense in the context of primary elections, in which the contests are between individuals, not parties, the candidates are often poorly known at the start, and elections are held on a rolling basis.

Though some of the same forces are obviously at work, one cannot readily generalize from these studies of American primaries to the nature of expectations in general elections. Therefore, we developed a model to fit the latter circumstances. In doing so, we placed greater emphasis on the effects of preferences on expectations and on the nature of the expectations-formation process in less intense, interelection times as well as during election periods.

[&]quot;% expecting Cons. to win - % expecting Lab. to win.
"Postelection = 1 for three months, 0 otherwise.

Instrumental variable. Instrument is the predicted value generated by regressing VCL on all exogenous variables in the system; VCL is defined in Table 1. See Appendix A for details.

**Presidential of the property o

^dPreelection = 1 for three months, 0 otherwise.

[%] expecting better - % expecting worse.

f1 for July 1983-June 1987, 0 otherwise.

^{*}p < .05.
**p < .01 (one-tailed test).

Our findings show, first of all, that the expectations-formation process takes on a different character during interelection times. Similarities exist between the electoral period and more quiet times (inertia exists throughout, and the economic forecast and winning reputation variables remain unchanged) but there is a major disconnection between voter preferences and expectations in the neighborhood of an election. Particularly revealing is the preelection adjustment (PREELECT * VCL_t). It may be tempting to interpret the quick adjustment component (VCL_t) simply as a manifestation of what Bartels called "the resilience of projection . . . even in situations where objective cues about the shape of political reality are quite powerful" (1987, 811). Indeed, the fact that only the intensity of the electoral campaign introduces a strong reality check lends credence to the idea that most of the time, most of the people think that future outcomes will move in the direction of their own preferences. But the larger, combined effect of aggregate preferences and simple projection (and whatever other psychological mechanisms link current preferences and expectations) in periods close to the election tells us that reality rapidly takes over when the election is actually in sight.39

Our findings also reveal the contemporaneous nature, even during interelection periods, of the effect of vote intentions on expectations and vice versa. Before they were even completed, much less publicized by the media, the outcomes of polls of vote intentions were already internalized by respondents and reflected in the degree to which they thought that one party held the advantage going into the next general election. Likewise, the anticipated election outcomes had already influenced, though to a smaller degree, those same vote intentions.

While the contemporaneous impact of vote intentions on expectations is consistent with the notion of a simple projection effect, it also suggests how readily available is information about party popularity and related matters. Despite the little attention that ordinary voters pay to political matters, the aggregate movements studied here appear to reflect an ongoing awareness of and reaction to the political world. More generally, the results are also consistent with the view that people accommodate political information quickly and efficiently.

In making much the same point, Lewis-Beck and Skalaban suggested that about all that voters needed to perform competent electoral forecasts was "regular membership of the [American] polity" (1989, 153). Our own work goes one step further by suggesting that voters use quite simple forecasting rules, weighting, when it is relevant, the burden of the past (the winning reputation component) with current facts and evaluations about who they support, what they expect their future financial situation to be, and when the next election will occur.

With respect to vote intentions, our finding of a bandwagon effect, even though small, adds support to the contention that some voters respond to the implicit bonus of being on the winning side even in

high-salience general elections. Bandwagon effects have been found primarily in U.S. presidential primaries (Abramson et al. 1992, 64-65; Bartels 1988; Skalaban 1988, 136-50), but as noted, those elections represent a very different setting from most electoral situations. In British studies, evidence consistent with bandwagon effects has been found, but the absence of panel data made it impossible to distinguish with certainty between bandwagon and projection effects (McAllister and Studlar 1991, 736). And in a Canadian study, there was almost no evidence of bandwagon effects (Johnston et al. 1992). It may well be that their small size not only makes bandwagon effects difficult to detect but that they are sometimes swamped by other election factors. At any rate, the existence of a bandwagon effect in our study suggests that in the future, properly specified popularity functions should include an expectations term whenever possible.

Finally, the results of the overall model are refreshing from a normative point of view. They indicate that many among the "losers" are able to make reasonable forecasts without being in turn heavily influenced by this disappointing outcome. That so many losers recognize themselves as such and nevertheless stick to their preferences is refreshing in that it strongly suggests that under normal circumstances voters are expressing preferences and not simply following the crowd.

APPENDIX A: IDENTIFICATION AND ESTIMATION OF THE EXPECTATIONS AND VOTE MODELS

This appendix gives details of the identification and estimation of our equations.

Identification

Identification of our model is complicated by the fact that we have a system of equations that is nonlinear in the variables. In particular, the expectations equation contains a current endogenous variable on the right-hand side that enters in both an additive and an interactive fashion. It is well known that the usual necessary-and-sufficient rank condition for identification of linear structural equations needs to be amended in this context (Achen 1986, 141–44; Greenberg and Webster 1983, 230–33; Kelejian and Oates 1989, 304–19). Fortunately the required rank condition is a simple extension of the linear case.

In a general linear simultaneous-equations model with G endogenous variables, the rank condition requires that for each equation in the system rank $(A \Phi) = G - 1$ where $A = [\Gamma B]$ is a matrix comprised of the coefficients on the endogenous and exogenous variables in the system and Φ is a conformable matrix representing the exclusion restrictions present in the system. In the linear-in-the-variables case, we require that the endogenous variables enter the A matrix

(through Γ) additively. This rules out models like ours where the A matrix contains an interactive term due to the specification of the expectation equation. However, a simple extension of the rule rank $(A \Phi) =$ G-1 will cover our nonlinear-in-the-variables case. (For a general proof see Fisher 1966, chap. 5). Provided we include the nonlinear function of the endogenous variable as an additional column of A- Γ , we can continue to use the rule rank $(A \Phi) = G - 1$ to check the identification status of the model. In more practical terms, provided we treat the interactive term in the expectations equation as an additional endogenous variable holding constant the number of rows of $A = [\Gamma B]$, it is simple to check the identification status of our expectations and vote equations. The reader will find that both our equations are overiden-

Estimation

The estimation of our model is a relatively straightforward application of two-stage least squares—an instrumental-variables method in which the "instrument" is simply the predicted value of the endogenous variable(s) from a reduced-form regression equation. As with the identification procedure, estimation is slightly more complicated when the model is nonlinear-in-the-variables. In the linear simultaneous equations model the instrument is formed by regressing each endogenous variable in the model on all exogenous variables in the system. In the nonlinear case the instrument is often formed by regressing each endogenous variable on all the exogenous variables plus a set of polynomials and interactive terms in the exogenous regressors (see, e.g., Achen 1986, 142-43). We computed the instrument by the standard method, eschewing the more complex nonlinear-in-the-variables reduced form (although we tried this specification and it made little difference in the results). A justification for our choice of procedure can be found in Kelejian and Oates 1989, 319-24. Provided the nonlinearity is not too severe (and our reduced-form estimates indicate that this is the case), the linear approximation in the instrument equation will not cause problems in formulating the instrument.

As noted, the actual estimation procedure is twostage least squares, which is simply a particular form of instrumental variables. The asymptotic theory that underlies estimation and inference in linear and nonlinear expectations models can be found in Pagan 1984, 1986. Our model is a variant of Pagan's (1984) model 2 with some additional attention being paid to the nonlinearity. Standard asymptotic theory on the convergence of the normalized moment matrix of regressors and the cross-moment matrix of regressors and disturbances leads to consistency results in the location (slope) parameters. As is standard, the residuals are calculated from the original variables leading to consistency for the scale (variance/standard error) parameter. Proofs are straightforward and can be found in Pagan 1984, 227-29 (the linear model) and Pagan 1986, 523-30 (the nonlinear model). The diagnostics, which indicate that our specification is sound, can be found in Appendix B.

APPENDIX B: DIAGNOSTIC TESTING **PROCEDURES**

To check for general adequacy of our model specification we used the diagnostic testing procedures advocated by Spanos (1986) and applied in political science by Granato (1991). The PC-GIVE package provides a series of regression diagnostics designed to alert one to general model misspecification. The tests provided include RESET tests for functional misspecification, a Lagrange multiplier test for general patterns of serially correlated errors, a test for ARCH (Autoregressive Conditional Heteroscedasticity) patterns in the residuals, a test for general heteroscedasticity in the residuals (the "White" test), and a test for residual normality. Because we are operating in a simultaneous-equations framework, we use the residuals based on the original variables, not their predicted values. The relevant statistics based on these residuals converge to the chi-squared distribution, as do their single equation counterparts (Spanos 1986, chaps. 25-26). The substantive aspects of our regression diagnostics follow.

First, our model appears to be well specified: the RESET test for both vote and expectation equations indicates that there is no obvious misspecification in terms of functional form or omitted variables. The relevant statistics fall well below conventional levels of significance ($\chi_5^2 = 3.30$ for the expectations equation, and $\chi_4^2 = 1.53$ for the vote equation).

Second, serially correlated errors do not plague our analysis: given that our model includes a lagged dependent variable, the standard Durbin-Watson statistic is not very meaningful. A more appropriate and general test is the Lagrange multiplier test. Using PC-GIVE, we performed a joint test on lags 1-7 of the autocorrelation coefficient. The resulting statistics fall well below conventional levels of significance (χ_7^2 = 1.49 for the expectations equation, and $\chi_7^2 = .22$ in the vote equation). Additional evidence confirming the absence of serial correlation can be seen by looking at the magnitude of the autocorrelation coefficients at lags 1-7. No estimated coefficient is larger than .20, and most of them are in the -.05 to .05 range.

There does appear to be extremely mild heteroscedasticity for the expectations equation $(\chi_{13}^2 = 22.96)$ significant at the .05 but not at the .01 level) and no visible traces of it in the vote equation $(\chi_{16}^2 = 14.56)$. The evidence on lack of heteroscedasticity, while not quite as good as that on autocorrelation, seems to be reasonable.

Finally, both the expectations and the vote equations have residuals that pass a χ^2 test for normality ($\chi_2^2 = 1.40$ for expectations and $\chi_2^2 = 5.61$ for vote). In summary, model diagnostics indicate that our

two-equation model is theoretically well specified,

does not have serious problems in the stochastic component, and possesses residuals that are normally distributed.

Notes

We would like to thank Kirke Lawton for excellent technical assistance. We also wish to thank Neal Beck, Helmut Norpoth, and William Keech for their comments on a draft of this article.

- 1. The voting intention question is, "If there were a General Election tomorrow, how would you vote?" The expectations question is, "Regardless of how you are going to vote yourself, which party do you think is most likely to win the next general election?"
- 2. The expectations question was asked irregularly (mostly in months just preceding an election) from 1945 to 1973. Since 1975, it has been asked every month except seven months after the 1979 elections and five months after the 1987 elections. We limit our attention to the period starting in January 1976 and ending in June 1987, for two basic reasons: (1) including the period after the 1987 election would have been demanding in econometric terms as it would have created a second string of missing data; and (2) since the popularity function of the British parties has been extensively studied for different subperiods up to the end of the second mandate of Margaret Thatcher, it will be easier to specify a satisfactory vote equation, a preliminary condition in any study about the links between votes and expectations. On the popularity function, see Clarke, Mishler, and Whiteley 1990; Mishler, Hoskin, and Fitzgerald 1989; Norpoth 1991; Sanders, Ward, and Marsh 1991.
- 3. The original expressions of both preferences and expectations are categorical rather than thermometers measuring intensity of feelings and estimated probabilities. In addition, we have available only aggregate rather than individual-level preferences and expectations (though aggregate data may well be better than individual data, as noted in Kramer 1983 and Lewis-Beck and Skalaban 1989, 153).
- 4. We shall sometimes use the terms votes and votes equation rather than the more precise but longer terms voting intentions or popularity function. Similarly, we shall use expectations about future elections and election forecasts interchangeably.
- ably.
 5. We formulate the model in terms of general elections. So far, the most intense scrutiny of expectations formation has been in U.S. primary elections. We discuss the differences between those models and our own in the conclusion.
- 6. VCL, WCL, LEADERS, and MPSHARE are taken from Butler and Butler 1986 updated to 1987 from the Gallup Poll Monthly. UNEMP and INF are taken from the Organization for Economic Cooperation and Development's Main Economic Indicators: Historical Statistics. PEXP is taken from the Gallup Poll Monthly. As indicated in n. 2, WCL was unavailable for seven months during the period under study. We estimated our models both with and without fitted values (computed from the model's parameters) for the missing data and obtained nearly identical results in terms of the parameters and the goodness-of-fit measures. Results with fitted values are reported here.
- 7. For the purposes of estimation, the endogeneity of WCL_t and VCL_t poses a problem when they appear as explanatory variables in the vote and expectation equations, respectively. For this reason, an instrumental variable in the form of a predicted value is created when either variable appears on the right-hand side of our equations. See Appendix A for details.
- 8. Inclusion of both objective conditions and personal expectations has been common in the popularity function literature in Great Britain ("A Computer's Election Advice," *Economist*, 17 August 1991, 53; Clarke, Mishler, and Whiteley 1990; Sanders, Ward, and Marsh 1991). Some work on U.S.

- presidential popularity argues in favor of including only retrospective (Beck 1989) or only prospective (MacKuen, Erikson, and Stimson 1992) economic variables. Our interest here is not in testing these alternative views but to take reasonable precautions against overestimating the effects of expectations.
- 9. How do you think the financial situation of your household will change over the next 12 months? (Get a lot better, Get a little better, Stay the same, Get a little worse, Get a lot worse). Following Clarke, Mishler, and Whiteley 1990 and Sanders, Ward and Marsh 1991, we operationalized the variable as the percentage difference between the first two categories (a lot better or a little better) and the fourth and the fifth categories (a little worse or a lot worse).
- 10. Note also that both variables (as well as the subjective perception and the LEADERS variables) were designed in accordance with a reward-punishment model. To do so, "we reverse the sign of unemployment and inflation during Conservative governments thereby predicting that the Conservative . . . [lead] will increase both: a) as inflation and unemployment rise during Labour governments and b) as inflation and unemployment decline during Conservative governments" (Mishler, Hoskin, and Fitzgerald 1989, 224). Note finally that the rival hypothesis, the issue priority model, was also examined. (We also checked the possibility that the Tories may have benefitted from an "incumbency effect" by adding a dummy variable taking the value of 1 for the two Conservative mandates. It was not significant.) A useful summary of the differences between the issue priority model and the standard reward-punishment model is Clarke, Stewart, and Zuk 1986, 127-30. See also Kiewiet 1983; Kiewiet and Rivers 1984; Mishler, Hoskin, and Fitzgerald 1989, 222-24; Peffly 1984.
- 11. Retrospective perceptions about both personal and general economic conditions, as well as a variable measuring prospective perceptions about general economic conditions, were also examined.
- 12. Lafay, in a French study, follows a rather similar practice by introducing the popularity index of the prime minister in an equation for the popularity function of the president (1991, 135). Micro analyses also show that evaluations of party leader performance in Britain have independent effects on party support (see Clarke, Stewart, and Zuk 1986; Graetz and McAllister 1987). For evidence that leader images in Britain affect not only short-term intentions such as voting but also long-term intentions such as party identification, see Clarke and Stewart 1984.
- 13. There has been a debate about the magnitude and form of the impact of the Falklands War on parties' standings. Consequently, we tried various specifications for this variable and selected the best one using standard criteria. We think it likely that our simple dummy variable, together with the LEADERS variable, adequately takes into account the immediate Falklands War effect and its lasting impact through the sustained popularity of Mrs. Thatcher. On the debate over the war's effects, see Clarke, Stewart, and Zuk 1986; Clarke, Mishler, and Whiteley 1990; Norpoth 1987a and b; Sanders, Ward, and Marsh 1991.
- 14. Specifically, one would write down a structural model of the economy and use all the variables in the system to help predict the expected value of the endogenous variable of interest. Technically, this would impose a series of nonlinear cross-equation restrictions between the "reduced form" equation and the "expectations" equation. This is not the strategy we pursue here, for reasons outlined in the text.
- 15. This would be especially true if the data were "difference-stationary" rather than "trend-stationary." However, in our model only the inflation variables might reasonably be viewed as a difference-stationary process (but see Ostrom and Smith 1993).
- 16. First, the component satisfaction with the prime minister is basically retrospective. Second, the question about the opposition leader is also retrospective in that it does not ask the respondent to think about the opposition leader as a potential prime minister. Third, the two components do not tie the leaders together in an explicit inquiry about their comparative

potential as future prime minister. The question, "Who would make the best prime minister?," followed by a list of potential leaders, would perhaps be a better choice in an expectations equation, but it has not been asked on a regular basis. An example will illustrate these various points. In December 1985, satisfaction with Prime Minister Thatcher was 39%, with Kinnock at 47%; yet when asked who would make the best prime minister, Thatcher received the support of 33%, Kinnock of 24%.

- 17. Our expectations model is consistent with the argument outlined in Beck 1991. Beck argues that one ought to think of economic forecasts as conditional expectations where the conditioning variables include competence, administration, and other political variables. He goes on to criticize the strong rational-expectations approach in the popularity-function literature (e.g., Chappell and Keech 1985; Chrystal and Peel 1985) and presents empirical evidence indicating that such a model is suspect. (See also the criticisms of the rational-expectations approach in Hudson 1982, chap. 3.) We believe that our expectations model, which is more than an adaptive expectation but less than the economists' rational expectations, is an appropriate forecasting model.
- 18. This approach is similar to that used in studying presidential popularity functions, in which some account needs to be taken of changes in administrations. See, e.g., Beck 1991, 91, n. 11).
- 19. The high correlation between current intentions and expectations (.80 for the period under study) supports the view that individuals update their views about the likely winner of the next election very quickly, even continuously.
- 20. Evidence about the availability of poll results can be found in Butler and Kavanagh 1987. Evidence about public exposure to polls for the elections under study here can be found in McAllister and Studlar 1991, 725.
- 21. Indeed, to exclude VCL_t from the equation would require, instead, that we include variables representing sources of all the new information used to form the voter's current vote intention. It is likely, of course, that some voters are sufficiently insulated from contemporary information that they seldom update their understanding of voting intentions. Our argument is that on balance, the most immediate measure of vote intentions (i.e., at time t) is a superior predictor of current expectations. We tested this reasoning, however, by trying an alternative specification in which VCL_{t-1} replaced VCL_t. The resulting coefficient was not significant despite the high correlation (.78) between the two vote measures.
- 22. In fact, if simple projection from one's own vote intentions were the only process involved, then only individual-level VCL would be appropriate in the expectations equation. However, our interpretation is much broader. As we noted, individuals probably feel that their current evaluations are shared by others, but surely they do not think that everyone else views the parties identically. Thus, it seems fully justifiable to include aggregate-level VCL in the expectations equation. The interaction of VCL with PREELECT is also a strong indication that aggregate VCL belongs in the expectations model.
- 23. We expand our time frame here because data for nine months prior to each election are available for the longer period.
- 24. Note that voters put great reliance on polls to form their expectations even if they were misleading, as in 1970 and 1974.
- 25. Do the additive variable PREELECT and the variable POSTELECT (introduced earlier) need to be included as well for proper specification? Not only is it unnecessary always to include each of the components of an interactive term (see, e.g., Lewis-Beck 1980, 54–56), but it is relatively meaningless in this case, as the correlation between PREELECT and PREELECT * VCL is .93, and that between POSTELECT and FOSTELECT * WCL is .63. As to the choice of which variables to include (additive or interactive), theory clearly suggests the interactive form. If two parties are perceived as having exactly the same support, there is no reason to believe that the type of "blips" illustrated in Figure 1 will occur (as a dummy variable

would suggest). Moreover, a dummy variable would imply that these blips will have the same magnitude whatever the vote difference between the main contenders. An interaction term is more flexible, as it allows the sudden increase of "nonprojecting" voters to be linked to the closeness of the election.

- 26. Throughout, the election period is operationalized as the two months preceding an election as well as the electoral month itself (with a slight adjustment in 1979, when the election was on 3 May). Though electoral campaigns as such are shorter than that in Great Britain, rumors and polling activities justify this definition. The graphs show that the election effect does not occur smoothly over the interelection period but rather abruptly in the neighborhood of a forthcoming election—presumably under pressure of the unmistakable realities of the electoral campaign, the enhanced attentiveness of the public, and the decreased likelihood that time will permit a trailing party to rebound significantly. The uniqueness of the electoral context also explains the abruptness of the postelection adjustments we have discussed.
- 27. That a comparable interaction term would be negative in a model of individual vote decisions is entirely consistent with our argument, as it is precisely the weakening of projection effects (during election periods) among those who are likely losers that will generate the widening of the gap between preferences and expectations illustrated in Figure 1 and in the example in the text.
- 28. We checked for a possible interaction between time (operationalized both as one year before the election or not and number of months until the next election) and PEXP and found it not significant; perhaps the effect of this variable is about the same in election and nonelection periods precisely because it can be relevant for both retrospective and prospective reasons.
- 29. The two consecutive victories by Margaret Thatcher followed a period during which the incumbent party had been defeated four times out of six (1964, 1970, 1974, and 1979). Moreover, her 1983 victory was the most impressive one in the postwar period both in terms of seats and the popularity gap with Labour. Conversely, Labour's performance in this election was the poorest one in seats and popular support for the same period.
- 30. Labour was perceived during this period as much more divided than the Tories. In fact, in all the surveys during Thatcher's second mandate, the percentage of respondents perceiving Labour as a divided party outnumbered by extremely large margins the percentage of those thinking of it as united (-55 on average). The Tories were basically perceived as a united party for the same period, with exceptional and short periods of negative balance. (see Gallup Poll Monthly).
- 31. The high personal popularity of Thatcher, as well as the Tory's advantage in questions about the best leaders, speak to this point (see *Gallup Poll Monthly*).
- 32. The victorious Falklands War, the decrease of the inflation rate, and the economic recovery are possible candidates here.
- 33. That we found out-of-sample confirmatory evidence of the rigidity of expectations during Thatcher's third mandate buttresses our confidence that the type of inertia measured by our winning reputation variable is a genuine characteristic of expectations. Moreover, a preliminary inspection of the data after the Conservatives' fourth consecutive victory in 1992 apparently shows less rigidity, which is also consistent with our hypothesis that a string of victories is a necessary—though not a sufficient—condition to produce rigidity in expectations.
- 34. R² is estimated as 1-Regression Sum of Squares/Total Sum of Squares.
- 35. Should we formally test for integration of order one (I(1)) or a "stochastic nonstationarity" in the data series? We feel that such integrated behavior is highly unlikely, given the departure of our lagged coefficients from unity and the fact that our data are rates or ratio-type variables. For a discussion of this point and some general problems with the unit root methodology involved, see Cochrane 1991, 207–9.

36. The reported effect of the Falklands variable may seem small compared to other work. However, it is important not to lose sight of two facts. First, the results will be different depending on the definition of the dependent variable. The war had a different impact on Thatcher's approval rate (immediate and lasting) from what it did on the Tories' popularity (immediate but rapidly declining). Second, the results will be greater if the approval rate of Thatcher is excluded from the

regressors rather than included, as here.

37. In addition, we tested for the possibility that bandwagon effects were greater in the period immediately prior to elections (by including an interactive term, PREELECT * WCL). The coefficient was not significant. Following Norpoth, we also checked the possibility that electoral campaigns drive disenchanted partisans back to the governing party (1991, 150-51); we did so by adding separate dummies for the electoral periods under study. None of these variables was

significant.

38. For example, in a Canadian study, a simple counting variable denoting the number of consecutive polls in which a given party had led in preelection polls was important. We tried such a variable here, but it was not significant (perhaps because of the much greater time between polls, which meant that winning streaks were probably remembered less well, and, in any event, were not so long lived (in terms of numbers of polls) as those in Canada. We also tried a variable measuring the proportion of times during the last six months in which the Conservatives were leading in the polls, but it was not significant (see Johnston et al. 1992, chap. 7).

39. Sometimes, reality becomes overwhelming, and "almost everyone" correctly forecasts the election result, as in Britain in 1983 (see Figure 1) or in the U.S. elections of 1964 and 1972 (Lewis-Beck and Skalaban 1989, 148). Of course, even in the case of a landslide, there will be some "true believers." Moreover, a more adequate measure of expectations (0-100 scale) would probably capture projection effects not visible with the measure we have available; for example, in the U.S. presidential election of 1964, a staunch Republican might have given Goldwater a 25% chance of winning, while most others would have given him close to a 0% chance.

40. These authors also point out that British work on this topic has produced conflicting results; see McAllister and Studlar 1991, 724. That work has often been based on simple comparisons between election results and public opinion polls conducted during the campaign.

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COLLECTIVE IDENTITY FORMATION AND THE INTERNATIONAL STATE

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The neorealist—neoliberal debate about the possibilities for collective action in international relations has been based on a shared commitment to Mancur Olson's rationalist definition of the problem as one of getting exogenously given egoists to cooperate. Treating this assumption as a de facto hypothesis about world politics, I articulate the rival claim that interaction at the systemic level changes state identities and interests. The causes of state egoism do not justify always treating it as given. Insights from critical international relations and integration theories suggest how collective identity among states could emerge endogenously at the systemic level. Such a process would generate cooperation that neither neorealists nor neoliberals expect and help transform systemic anarchy into an "international state"—a transnational structure of political authority that might undermine territorial democracy. I show how broadening systemic theory beyond rationalist concerns can help it to explain structural change in world politics.

The collective action problem dominates world politics. The state is often thought to solve it in domestic society by forcing or socializing people to identify with the common good, but the problem seems endemic to the anarchic world of international relations, where each state reserves the right and the force to do as it pleases. Realists and idealists have long debated the conditions under which states can overcome this problem, the former taking a materialist line that power and human nature preclude significant cooperation, the latter arguing that knowledge and institutions make it possible. The discussion has become very sophisticated, with neorealists and neoliberals today dissecting the effects of pursuing relative versus absolute gains, the shadow of the future, transaction costs, and so on (Baldwin 1993).

An important but neglected feature of the recent debate is the shared belief that collective action should be analyzed in terms of Mancur Olson's (1965) definition of the problem, which takes self-interested actors as constant and exogenously given and focuses on the selective incentives that might induce them to cooperate. This reflects the rationalist strategy of explaining behavior in terms of changing prices or constraints, rather than tastes (Stigler and Becker 1977). While some neoliberals have questioned the assumption of state egoism (Keohane 1984, 120-32; see also Lumsdaine 1993), few treat state interests as endogenous to interaction. They either bracket the formation of interests, treating them as if they were exogenous, or explain interests by reference to domestic politics, on the assumption that they are exogenous, although not necessarily constant (e.g., Moravcsik 1992). In both cases, the effect on systemic theory is captured by what Jeffrey Legro (1993) calls the rationalist "two-step": first interests are formed outside the interaction context, and then the latter is treated as though it only affected behavior. This can be merely a methodological presumption, but given its pervasiveness in the current debate it may also be seen as an implicit hypothesis about world politics: systemic interaction does not transform state interests.

Neoliberals make things hard for themselves by accepting this constraint, and their efforts to explain cooperation under it are admirable. But it also brackets an important line of argument against realists, namely, that through interaction, states might form collective identities and interests, redefining the terms of Olson's problem altogether (Calhoun 1991). We cannot know, a priori, which argument is more appropriate. A rationalist approach makes sense when state interests really are exogenous to interaction, which is sometimes the case. When they are not, however, it may ignore important possibilities and/or strategies for cooperation, as well as misrepresent the latter's dynamics.

It would be useful to discuss potential anomalies for the rationalist hypothesis, but ultimately it can only be assessed against its rival, which has not been adequately articulated in the literature. With a view toward theoretical pluralism, my goal herein is to formulate such a rival by reframing the collective action problem among states in terms that make interests endogenous to (or part of the problem in) interaction. In so doing, I hope to put in sharper relief the underappreciated implications of a rationalist assumption (exogeneity).

I draw on two literatures for this purpose. The first is integration theory, which focuses on the formation of community at the international level. Moribund since the early 1970s, it is undergoing a revival today, thanks in part to neoliberals (Keohane and Hoffmann 1991). Collective identification is an essential variable in this theory, however, since without changes in identity the most we can expect is behavioral cooperation, not community (Deutsch et al. 1957; Lasswell 1972; Russett 1963). A rejuvenated "sociology of international community" will therefore ultimately have to go beyond a rationalist vocabulary (Linklater 1990). This exists in what is variously called critical, reflectivist, or *constructivist* international relations scholarship (see Keohane 1988; Wendt 1992). Con-

structivists are interested in the construction of identities and interests and, as such, take a more sociological than economic approach to systemic theory. On this basis, they have argued that states are not structurally or exogenously given but constructed by historically contingent interactions.²

In building a bridge between these two literatures I take a state-centric approach, which integration theorists have often avoided in the past because of its emphasis on anarchy. I do so for two reasons. First, notwithstanding the growing importance of nonstate actors in world politics, states remain jealous of their sovereignty and so may resist collective identification more than other actors, which poses a harder case for theory. Second, I argue that collective identification is an important condition for the emergence of "international states," which would constitute a structural transformation of the Westphalian states system. In effect, constructivism shows how the concern of integrationist theorists with the formation of community can be addressed from a state-centric perspective and the latter thereby made into a critical theory of world politics.

I shall define my dependent variable—forms of identity and interest—then briefly discuss some causes of state egoism at various levels of analysis, arguing that none justifies treating it as given. I shall then sketch a "pretheory" of collective identity formation among states and, finally, suggest how this points to an internationalization of the state, with implications for the states system and democratic theory.

IDENTITY AND COLLECTIVE ACTION

Constructivism is a structural theory of the international system that makes the following core claims: (1) states are the principal units of analysis for international political theory; (2) the key structures in the states system are intersubjective, rather than material; and (3) state identities and interests are in important part constructed by these social structures, rather than given exogenously to the system by human nature or domestic politics. The second claim opposes realism. The third opposes systemic theories that are rationalist in form, whether they are "as if" theories that bracket interest formation, or unit-level, "reductionist" ones (Waltz 1979) that say interests "really are" exogenous. The result is one form of structural idealism or "idea—ism". 3

The claim that states are socially constructed can take various forms. In an effort to avoid an oversocialized approach, I distinguish between the corporate and social constitution of state actors, which parallels the distinction between the "I" and the "me" in symbolic interactionism (Mead 1934). In both cases, I argue that interests are dependent on identities and so are not competing causal mechanisms but distinct phenomena—in the one case, motivational, in the other, cognitive and structural—and, as such, play different roles in explaining action.

Corporate identity refers to the intrinsic, self-organizing qualities that constitute actor individuality. For human beings, this means the body and experience of consciousness (Schwalbe 1991); for organizations, it means their constituent individuals, physical resources, and the shared beliefs and institutions in virtue of which individuals function as a "we" (Douglas 1986). Corporate identities have histories, but these do not concern me here; a theory of the states system need no more explain the existence of states than one of society need explain that of people. The result is a weak or essentialist social constructionism, but one that still leaves the *terms* of state individuality open to negotiation. (There is no space to defend it here, but for a start, see Leplin 1988 and Wendt n.d.)

The corporate identity of the state generates four basic interests or appetites:

- 1. physical security, including its differentiation from other actors
- 2. ontological security or predictability in relationships to the world, which creates a desire for stable social identities
- 3. recognition as an actor by others, above and beyond survival through brute force
- 4. development, in the sense of meeting the human aspiration for a better life, for which states are repositories at the collective level.⁴

These corporate interests provide motivational energy for engaging in action at all and, to that extent, are prior to interaction, but they do not entail self-interest in my sense, which is an inherently social phenomenon. How a state satisfies its corporate interests depends on how it defines the self in relation to the other, which is a function of social identities at both domestic and systemic levels of analysis.

Social identities are sets of meanings that an actor attributes to itself while taking the perspective of others, that is, as a social object (McCall and Simmons 1978, 61–100). In contrast to the singular quality of corporate identity, actors normally have multiple social identities that vary in salience. Also in contrast, social identities have both individual and social structural properties, being at once cognitive schemas that enable an actor to determine "who I am/we are" in a situation and positions in a social role structure of shared understandings and expectations. In this respect, they are a key link in the mutual constitution of agent and structure (Wendt 1987), embodying the terms of individuality through which agents relate to each other. These terms lead actors to see situations as calling for taking certain actions and thus for defining their interests in certain ways.

Some state identities and interests stem primarily from relations to domestic society ("liberal," "democratic"), others from international society ("hegemon," "balancer"). Foreign-policy role theorists (e.g. S. Walker 1987), as well as more recently a number of neoliberals, have emphasized the domestic (and thus systemically exogenous) roots of state identities. I am interested in showing how state identities may be endogenous to the system but (as we shall see) this is

ultimately an empirical question that depends on the depth of structures at each level of analysis. It is also important to note that the intersubjective basis of social identities can be cooperative or conflictual. What matters is how deeply the social structures they instantiate penetrate conceptions of self, not whether self and other are normatively integrated. The Cold War was a social structure in virtue of which the United States and the USSR had certain identities. These were embodied in "national security worldviews" (in terms of which each defined self and other) and in role positions in a social structure (see Weldes 1993). The content of national interests was in part a function of these structurally constituted identities (as well as of domestic ones). The United States had an interest in resisting Soviet influence in Angola because the Soviets were an enemy and enmity is a social relation.

Social identities and interests are always in process during interaction. They may be relatively stable in certain contexts, in which case it can be useful to treat them as given. However, this stability is an ongoing accomplishment of practices that represent self and other in certain ways (Ashley 1988), not a given fact about the world. Rationalism depends on practices having stabilized identities, which we cannot know a priori.

Collective Identity and Action

The ability to overcome collective action problems depends in part on whether actors' social identities generate self-interests or collective interests. Selfinterest is sometimes defined so as to subsume altruism, which makes explanations of behavior in such terms tautological. Instead, I shall define self-interest and collective interest as effects of the extent to which and manner in which social identities involve an identification with the fate of the other (whether singular or plural). Identification is a continuum from negative to positive-from conceiving the other as anathema to the self to conceiving it as an extension of the self. It also varies by issue and other: I may identify with the United States on military defense but with the planet on the environment. In any given situation, however, it is the nature of identification that determines how the boundaries of the self are

In the absence of positive identification, interests will be defined without regard to the other—who will instead be viewed as an object to be manipulated for the gratification of the self. (Note that this does not preclude action that benefits others, as long as it is done for instrumental reasons.) This can take more or less virulent forms. The neorealist claim that states define their interests in terms of relative gains assumes that states tend toward the negative end of the identification continuum, whereas the neoliberal claim that absolute gains predominate assumes that states tend toward the center (neither positive nor negative identification). In both cases, however, self-interest stems from a particular representation of the

relationship of self to other—from social, not corporate identity. Self-interest presupposes an other.

Though little discussed in recent international relations scholarship, an extensive literature exists on collective identity in sociology, social psychology, philosophy, and even economics (see, e.g., Calhoun 1991; Chase 1992; Jencks 1990; Melucci 1989; Morris and Mueller 1992; Oldenquist 1982; Sen 1985; Taylor and Singleton 1993; Wartenberg 1991).5 It refers to positive identification with the welfare of another, such that the other is seen as a cognitive extension of the self, rather than independent. Because of corporate needs for differentiation, this identification will rarely be complete (although some people do sacrifice their lives for others), but to the extent that it exists, there will be an empathetic rather than instrumental or situational interdependence between self and other (Keohane 1984, 122-123; see also Russett and Sullivan 1971, 851-52). This is a basis for feelings of solidarity, community, and loyalty and thus for collective definitions of interest. Having such interests does not mean that actors are irrational or no longer calculate costs and benefits but, rather, that they do so on a higher level of social aggregation. This discourages free-riding by increasing diffuse reciprocity and the willingness to bear costs without selective incentives, an effect supported by empirical research in various fields (e.g., Caporael et al. 1989; Dawes, Kragt and Orbell 1990). This is hardly surprising: if collective action depended solely on coercion or selective incentives, it would be a miracle that society existed at all. The state itself is testimony to the role of collective identity in human affairs.6

The distinction between alliances and collective security arrangements provides an instructive illustration. Alliances are temporary coalitions of selfinterested states who come together for instrumental reasons in response to a specific threat. Once the threat is gone, the coalition loses its rationale and should disband. In contrast, in collective security systems, states make commitments to multilateral action against nonspecific threats. Collective identity is neither essential nor equivalent to such a multilateral institution but provides an important foundation for it by increasing the willingness to act on "generalized principles of conduct" and diffuse reciprocity (Ruggie 1993a). (Less identification would be necessary in pluralistic security communities, in which the commitment is merely not to settle disputes by war, and more, in amalgamated security communities, in which states join formal unions [Deutsch et al. 1957].) Is NATO merely an alliance or a collective security system? There are good reasons for thinking the latter (Chafetz 1993), but data on collective identification would help us answer this question.

The difficulties of achieving pure collective identity make it unlikely that the motivational force of egoistic identities among states can be eliminated, as the recent debate over the Maastricht Treaty made clear. The tension between particularism and universalism is not specific to international relations, however, being inherent in the relationship of individuals to groups (Brewer 1991; Chase 1992; Wartenberg, 1991). Group membership can be costly to the individual and even lead to his being killed, all the while that it is highly valued. Identification is a continuum along which actors normally fall between the extremes, motivated by both egoistic and solidaristic loyalties. The existence of multiple loyalties is at the heart of the debate over "European identity" and may generate substantial role conflict (Smith 1992). Resolutions of such conflicts are never permanent or fixed, however, and their evolution cannot be studied if we take interests as given. Thus, I am not suggesting that collective interests replace egoistic ones as exogenously given constants in a rationalist model but, rather, that identities and interests be treated as dependent variables endogenous to interaction. This would allow us to treat collective action not merely as a problem of changing the price of cooperation for self-interested actors but as a process of creating new definitions of self.

EXPLAINING SELF-INTEREST IN WORLD POLITICS

States, of course, often do define their interests in egoistic terms. Constructivists would emphasize that these are always in process, sustained by practice, but to the extent that practice is stable, the rationalist assumption that interests are given may be useful. To claim this, however, we need to justify that assumption. I shall briefly examine five explanations for state egoism (three domestic and two systemic), arguing that their strength varies historically and often leaves room for collective identity formation.

Domestic Determinants

Perhaps the most fundamental explanation is that self-interest, even if not presocial, stems from the essential nature of states. One might make this argument with reference to human nature, but more relevant here is corporate nature. In a recent critique of constructivism, for example, Jonathon Mercer (1993) draws on the social psychology of intergroup relations to argue that the mere perception of being in a group is sufficient to generate in-group favoritism and out-group discrimination, suggesting that states are cognitively predisposed to be self-interested when they come into contact. This is an important reminder of the importance of corporate identity and interests in constructing social ones. It does not entail permanent group egoism, however, since the boundaries of the self are not inherently limited to corporate identity and scholars in this same tradition have done important work on the conditions under which groups develop common identities (Gaertner et al. 1993).

Second, states typically depend heavily on their societies for political survival, which may induce them to place societal interests before those of other states and treat the latter as instruments for realizing the former. This is undoubtedly pervasive and reflects a general relationship between dependency and identity formation that I shall discuss. Nevertheless, much depends on the nature of state–society relations. Some states depend more on international than domestic society (Jackson and Rosberg 1982; Wendt and Barnett 1993), while others are embedded in societies in which domestic welfare commitments are projected onto altruistic foreign policies (Lumsdaine 1993) and still others are part of democratic societies that identify through shared norms and political culture. To the extent that the boundaries of society are porous, in other words, states might be propelled toward collective identification by "domestic" factors.

These two arguments come together in a third, which focuses on nationalism, that is, a sense of societal collective identity based on cultural, linguistic, or ethnic ties. Nationalism may be in part "primordial" and thus inherent to societies' self-conceptions as distinct groups. In addition, the dependence of states on their societies may be such that they cultivate nationalist sentiments in order to solidify their corporate identities vis-à-vis each other (Anderson 1983). Nevertheless, the depth and exclusivity of national identities varies greatly. German national identity in 1939 was chauvinist and exclusivist, while today it coexists with a significant European component; Serbian identity has moved in the opposite direction, from coexistence with a Yugoslavian identity to chauvinism. This suggests that how nationalism affects state interests should be treated as an open, empirical issue, not assumed a priori to produce egoism inevitably.

None of this is to deny that these (and other) domestic factors often dispose states to be self-interested. The point is that this is socially constructed and historically contingent, not inherent to stateness. Much the same conclusion follows from a consideration of two systemic determinants of state identity.

Systemic Determinants

Neorealists and institutional idealists might each make systemic arguments for treating self-interest as given. Waltz (1979), for example, argues that anarchies are self-help systems in which states that do not think egoistically will be selected out by those that do. This allows him to assume that surviving states will be self-interested, justifying its treatment for subsequent theoretical purposes as exogenously given. In effect, in Waltz's view anarchy so tightly constrains identity formation that the latter becomes uninteresting, which is how his would-be structuralism begets rationalist individualism (Wendt 1987).

The key to this story is the assumption that anarchies are inherently self-help systems, in which actors do not identify positively with each other's security. I have argued elsewhere that there is no necessary connection between the two (Wendt 1992). An anarchy may be a self-help system, but it may also

be a collective security system, which is not self-help in any interesting sense. Which logic obtains depends on conceptions of self and other; an anarchy of friends is different from one of enemies. If, because of unit-level factors, states threaten each other's security in their first encounter, then competitive dynamics may ensue, generating egoistic conceptions of self. New anarchies may even be particularly susceptible to such outcomes. But if states bring a friendly or respectful attitude to that encounter, then different dynamics of identity formation may ensue. Anarchic structure explains little by itself; what matters is the identities and interests that states bring to their interactions and the subsequent impact of the latter on the former. Self-help presupposes self-interest; it does not explain it. Anarchy is what states make of it.

In contrast to the realist's Darwinian view of world politics as an asocial "system," institutionalists emphasize the norms and shared understandings that constitute international "society" (Bull 1977). The foundation of this society is the principle of sovereignty. Sovereignty is a social identity, and as such, both a property of states and of international society. Its core is a notion of political authority as lying exclusively in the hands of spatially differentiated states, in which sense it is an attribute of the state implying territorial property rights (Ruggie 1983a). Territorial control is only a "right," however, if it is recognized by other states, in which sense sovereignty is an institution. This institution mitigates the dangers of anarchy, helping states to survive that otherwise might not (Jackson and Rosberg 1982; Strang 1991).

Sovereignty has an ambiguous relationship to selfinterest. On the one hand, it is a highly open-ended institution compatible with a diversity of interests, by virtue of empowering states juridically to determine their own interests and creating a measure of de facto security in which they have the luxury of doing so. This indeterminateness of sovereignty is important to the argument I shall make about the internationalization of the state. On the other hand, an important rationale for sovereignty is to meet corporate needs for security and recognition. Moreover, it does so in a particular way, namely, by defining rigid spatial boundaries between the rights of self and other. Because of the intimate connection between these rights and corporate needs, states may perceive threats to their ability to exercise such rights as threats to their corporate individuality-in effect, conflating social with corporate identity. One might expect this especially in the early days of a sovereignty regime, when states are still unsure whether their rights will be acknowledged by others. This, along with the fact that by accepting sovereignty, states give up the right to protection by others, will tend to promote egoistic over collective conceptions of interest, or "possessive" over "social" individualism (Shotter 1990).

If and when a society of egoistic sovereigns has been created, it will resist redefinition in more collective terms for at least two additional reasons. First, the need for ontological security motivates actors to hang onto existing self-conceptions because this helps stabilize their social relationships. Second, once Waltz's self-help world is in place, it will reward egoism and punish altruism. Thus, even though a states system is self-help only in virtue of intersubjective knowledge, the latter confronts states as a social fact that resists easy change. Nevertheless, sovereignty may also promote collective identity formation in the long run. Like individuals, states do not want to be engulfed by a collective. This concern may be mitigated, however, by being recognized as sovereign, which increases the confidence that others will respect corporate individuality and leaves the decision to join the collective up to each state. In this way, sovereignty may make it "psychologically" possible for states to develop collective attachments (see Wartenberg 1991), paving the way for an internationalization of the state.

To sum up the argument so far, (1) egoistic interests are based on representations of the relationship between self and other and as such are not an essential feature of individuality; (2) these representations are always in process, even if their relative stability in certain contexts makes it possible to treat them as if they were sometimes given; and (3) many factors dispose states toward egoism, but these do not always preclude collective identities. Still, given that international history has produced mostly egoistic states, collective identity formation must start with and overcome that social fact. I turn now to how such an endogenous transformation of Olson's problem might occur.

COLLECTIVE IDENTITY FORMATION AMONG STATES

The forces disposing states toward particularism sometimes confront others disposing them toward collectivism. Collective identities vary by issue, time, and place and by whether they are bilateral, regional, or global. I cannot examine such variations here, nor can I assess the weight of their determinants relative to those of state egoism. Thus, I specifically do not impute any directionality or teleology to the historical process. I shall merely identify some causal mechanisms that, to the extent they are present, promote collective state identities, although their impact may be more lumpy than linear. In view of my concern with endogenizing identity change to systemic theory, I shall limit my focus to factors at the systemic level, even though domestic factors may matter, as well. Some of my arguments will repeat claims made by integration theorists (though here applied to a state-centric framework), while others will reflect general principles of identity formation in "structurationist" and symbolic interactionist social theory. I shall differentiate three types of mechanisms by the causal roles they play (structural contexts, systemic processes, and strategic practice) and address two factors under each. The discussion will again be suggestive rather than exhaustive.

Structural Contexts

The structures of regional or global international systems constitute interaction contexts that either inhibit or facilitate the emergence of dynamics of collective identity formation, and as such they play an indirect causal role. Whereas neorealists define structure in material terms, constructivists emphasize intersubjective structure, while leaving room for the emergent effects of material capabilities.

Intersubjective systemic structures consist of the shared understandings, expectations, and social knowledge embedded in international institutions and threat complexes, in terms of which states define (some of) their identities and interests. They vary in thickness or depth, depending on the density and/or salience of interactions for state actors. Many international relations scholars acknowledge a role for such structures only in cooperative settings, implicitly equating intersubjectivity with normative integration and leaving conflict to be treated in materialist terms. In contrast, I would argue that conflicts are also intersubjective phenomena, partly in virtue or rules shared by the parties (Bull 1977) but especially in virtue of shared perceptions of issues and threat (see Ashley 1987; Walt 1987). The Cold War was fundamentally a discursive, not a material, structure. Whether cooperative or conflictual, intersubjective structures constitute what kind of anarchy states are in: Hobbes's anarchy is constituted by one such structure, Locke's by another, and Kant's by a third. To say that worlds are defined intersubjectively is not to say they are malleable, however, since intersubjective constructions confront actors as obdurate social facts. Sometimes structures cannot be changed in a given historical context. My idealism is that of Durkheim and Mead, not Pollyanna and Peter Pan.

Intersubjective structures help determine how much "slack" exists in a states system for dynamics of collective identity formation to develop. The greater the degree of conflict in a system, the more the states will fear each other and defend egoistic identities by engaging in relative gains thinking and resisting the factors that might undermine it. In a Hobbesian war of all against all, mutual fear is so great that factors promoting anything but negative identification with the other will find little room to emerge. In the Lockean world of mutually recognized sovereignty, however, states should have more confidence that their existence is not threatened, creating room for processes of positive identification to take hold. The ability of states to create new worlds in the future depends on the old ones they created in the past.

Intersubjective structures give meaning to material ones, and it is in terms of meanings that actors act. British nuclear capabilities were a very different social fact for the United States from Soviet nuclear capabilities. Nevertheless, material structures can have sui generis effects on collective identity formation.

On the one hand, within a conflictual intersubjective context actors will tend to infer intentions from capabilities, such that the latter may become emergent sources of insecurity (Jervis 1978). The deployment of SS-18s by the USSR was a new threat to the West, even though its meaning was a function of the Cold War. To this extent, material capabilities may be part of the problem in a conflict, inhibiting the emergence of positive identifications. On the other hand, material structure can facilitate the latter when it provides incentives for collective problem solving (as Dan Deudney [1993] argues with respect to nuclear weapons) or is sufficiently asymmetric that powerful states can coerce weaker ones to identify with them (Ikenberry and Kupchan 1990; Wendt and Barnett 1993). Such coercion may produce hegemony in the Gramscian sense, but hegemony is a form of collective identity, one less easily created when material power is equally distributed.

Systemic Processes

Structures are always being reproduced or transformed by practice and thus are not static background conditions for collective identity formation. To the extent that practice does reproduce them, however, they merely inhibit or facilitate collective identity formation. The remaining factors have more direct causal impacts. By systemic processes I mean dynamics in the external context of state action. Both of these arguments are long-standing liberal ones. My point is that they relate to identity, not just behavior—a point lost in neoliberalism's rationalist "two-step."

The first process is rising interdependence. This can take at least two forms. One is an increase in the "dynamic density" of interactions due to, for example, trade and capital flows (Buzan 1993; Ruggie 1983a). A second is the emergence of a "common Other," whether personified in an external aggressor or more abstract threat like nuclear war or ecological collapse (see Lasswell 1972, 24). While the one generates "dilemmas of common interests" and the other "dilemmas of common aversions" (Stein 1983), both increase the objective vulnerability and sensitivity of actors to each other (Keohane and Nye 1987) and, with these, the thickness of systemic structures. This reduces the ability to meet corporate needs unilaterally and increases the extent to which actors share a common fate. These changes in the context of interaction will sometimes affect only the price of behavior (as rationalism assumes), but they may also change identities and interests. Indeed, dependency, whether intersubjective or material, is a key determinant of the extent to which an actor's identity is shaped by interaction, which is why a child's development is normally far more influenced by its parents than by others actors. As the ability to meet corporate needs unilaterally declines, so does the incentive to hang onto the egoistic identities that generate such policies, and as the degree of common fate increases, so does the incentive to identify with others. As interdependence rises, in other words, so will the

potential for endogenous transformations of identity, with consequences that go beyond those analyzed by rationalists.

A second systemic process that may encourage collective identity formation is the transnational convergence of domestic values. This can take place in various issue areas, but two of the most salient are cultural (the rise of global consumerism) and political (the spread of democratic institutions, welfare statism, concern with human rights, etc.). Societal convergence can result from rising interdependence, in which case, its effects will be hard to separate from the latter. But it may also stem from demonstration effects, diffusion, and "lesson drawing" (in which one society learns from another that one form of social organization is "better" than another). The effect is to reduce the heterogeneity (or increase the similarity) among actors. As with interdependence, this change in the interaction context may sometimes affect only behavior, but it may also change identities and interests. As heterogeneity decreases, so does the rationale for identities that assume that they are fundamentally different from us, and the potential for positive identification increases on the grounds that "they're no different from us, and if it could happen

Despite the incentives that rising interdependence and societal convergence may create to adopt more collective forms of identity, however, neither is a sufficient condition for such a result. The vulnerabilities that accompany interdependence may generate perceived threats to self-control, and rising similarity may generate fears that the state has no raison d'être if it is not different from others. States may respond to these systemic processes, in other words, by redoubling their efforts to defend egoistic identities. (This may be one reason why students of intergroup relations have found that increased contact alone does not ensure cooperation.) The key to how states deal with the tension between corporate fears of engulfment and the growing incentives for collective identification, therefore, lies in how they treat each other in their changing interaction context.

Strategic Practice

In the last analysis, agents and structures are produced or reproduced by what actors do. Systemic structures and processes may affect the context of interaction, but specific actions are rarely dictated by them. Actors sometimes act as though they were in a "game against nature", but more interesting here is strategic practice, in which others are assumed to be purposive agents with whom one is interdependent. This is, of course, the traditional province of game theory, which normally does the rationalist "twostep" of treating identities and interests as exogenous to interaction. Without minimizing the usefulness of game-theoretic models when identities and interests are stable, I want to suggest that more is always "going on" in strategic interaction than such models convey, namely, the production or reproduction of

identities and interests. When these change as a result of interaction, game-theoretic models will misrepresent the possibilities for, and mechanisms of, cooperation. In what follows, I shall focus on two forms of interaction differing in their form of communication: behavioral and rhetorical.

The first argument involves a constructivist rereading of Robert Axelrod's (1984) "evolution of cooperation." Taking an iterated, two-person prisoner's dilemma as his model, Axelrod shows how a strategy of reciprocity, or "Tit for Tat," can generate behavioral cooperation. This process becomes attenuated in the n-person case of interest here, where it is harder to target specific others and so may only work in small or "k-groups" within a larger context (Olson 1965). But the general logic is transportable: through repeated acts of reciprocal cooperation, actors form mutual expectations that enable them to continue cooperating. Like Olson, however, Axelrod's basic model assumes that actors remain egoistic,8 and as such that interaction only affects expectations about others' behavior, not identities and interests. This gives the argument greater generality, applying to pigeons as much as to people, but it brackets the possibility that interaction may transform the interests constituting a game.

In contrast, if we treat identities and interests as always in process during interaction, then we can see how an evolution of cooperation might lead to an evolution of community. This can occur as an unintended consequence of actions carried out merely to realize self-interests or as a result of a conscious strategy of collective self-transformation (Gamson 1992, 60). Repeated acts of cooperation will tend to have two effects on identities and interests. First, the symbolic interactionist concept of "reflected appraisals" suggests that actors form identities by learning, through interaction, to see themselves as others do (Berger and Luckmann 1966; Mead 1934; Rosenberg 1981). The more significant these others are, as measured by the material and/or intersubjective dependency of the self upon them, the faster and deeper this process works. By showing others through cooperative acts that one expects them to be cooperators too, one changes the intersubjective knowledge in terms of which their identities are defined. Second, through interaction actors are also trying to project and sustain presentations of self (Goffman 1959). Thus, by engaging in cooperative behavior, an actor will gradually change its own beliefs about who it is, helping to internalize that new identity for itself. By teaching others and themselves to cooperate, in other words, actors are simultaneously learning to identify with each other—to see themselves as a "we" bound by certain norms. As with Axelrod's argument, this process may be attentuated in an n-person context, but the fact that humans do associate in communities suggests that repeated interaction can transform an interdependence of outcomes into one of utility.

Many examples of such transformations might be cited, but a particularly apposite one here is John

Ruggie's (1983b) analysis of the postwar free-trade regime as "embedded liberalism." In keeping with its rationalist basis, conventional regime theory treats the rules, norms, and principles defining regimes as external constraints affecting merely the price of behavior. Ruggie shows that the free-trade regime does more than this, having institutionalized in statesociety relations new state identities and interests that conform to the regime, in effect creating new subjects of international relations. Insofar as these define a transnational community of interest, states have incentives for compliance that go beyond the transaction costs of defection. The evolution of such a community can only be explained, however, if we examine the effect of practice on identities and interests.

The discussion so far has focused only on the potential effects of strategic behavior on identities and interests, and as such it is consistent with the concept of a noncooperative game in game theory. This may be realistic in situations where actors do not have available more direct forms of communication (as in the prison of the prisoner's dilemma), but this is not typically the case. What might be called rhetorical practice may have effects similar to those of behavioral practice but it does so through a different mode of communication, variously enacted as consciousness raising, dialogue (Offe and Wiesenthal 1980), discussion and persuasion (Caporaso 1992, 614-6), education, ideological labor (Hall 1986), political argument (Plotke 1992), symbolic action (Johnson 1988), and so on. Despite differences, all of these processes presuppose that the social world is constituted by shared meanings and significations, which are manipulable by rhetorical practices. These practices may involve power, but of the "third-dimensional," rather than the "first-dimensional" kind (Lukes 1974); that is, they are efforts to change others' conceptions of their interests. The goal of rhetorical practices in collective action is to create solidarity; thus they may have an important expressive function independent of their instrumental value in realizing collective goals.

A good part of the "action" in real-world collective action lies in such symbolic work. When leaders of the G-7 hold annual but substantively trivial meetings to discuss economic policy, when European statesmen talk about a "European identity," when Gorbachev tries to end the Cold War with rhetoric of "New Thinking" and a "common European home," when Third World states develop an ideology of "nonalignment," or when the United States demonizes Saddam Hussein as "another Hitler," states are engaging in discursive practices designed to express and/or to change ideas about who "the self" of self-interested collective action is. These practices may ultimately serve an instrumental or strategic function, but they cannot be understood from a strictly rationalist standpoint, since they are at base about redefining identity and interest.

Rival Hypotheses

There is nothing inevitable about collective identity formation in the international system. It faces powerful countervailing forces, and I do not mean to suggest that the logic of history is progressive; there are too many examples of failed collective identities for that. My point is only that to the extent that mechanisms are at work that promote collective identities, models that ignore them will understate the chances for international cooperation and misrepresent why it occurs. In that sense, my argument is a rival to the rationalist hypothesis about collective action. The precise nature of the rivalry depends on how the latter is formulated—whether as the claim that egoistic state interests are, in fact, given and constant (which instructs us to ignore the formation of interests) or as the claim that states might develop collective interests but only as a result of domestic factors (which leads to reductionism about state interests). Despite the differences between these claims, both treat interaction as affecting only the price of behavior and thus assume that a rationalist research agenda exhausts the scope of systemic theory. My rival hypothesis is not merely that states might acquire collective interests (which dissents from realism) but that they might do so through processes at the systemic level-which dissents from rationalist versions of systemic theory, realist or liberal. Indeed, by focusing on the systemic origins of collective identity, I have made systemic rationalism rather than realism my primary target.

Which hypothesis is more appropriate in a given context is an empirical question that may change over time. Two strategies for comparison suggest themselves. One would avoid the direct measurement of identities and interests and focus instead on testing the different behavioral predictions of the two models about how much collective action should occur, and, taking into account problems of revealed preference, on that basis infer whether interests are collective or egoistic. A problem with this strategy is that it is difficult to specify exactly how much collective action even a well-worn theory like Olson's would predict (Green and Shapiro 1994). This strategy also tells us little about the process and causes of collective identity formation. Thus a second strategy would be to focus more directly on identities and interests as the dependent variable and see whether, how, and why they change. The challenge here, of course, is to construct measures of state identity and interest capable of sustaining inferences about change, which I cannot take up here (see Wendt, n.d.).

THE INTERNATIONAL STATE

This essay so far has been a creature of the "anarchy problematique" (Ashley 1988), which theorizes about states in anthropomorphic terms as purposive actors interacting under anarchy. Despite my "sociological"

departure from the dominant "economic" vocabulary, like rationalists I treat states as agents having identities, interests, rationality, and so on. There are important objections to this analogy (Ferguson and Mansbach 1991, 370), but I believe it is legitimate for many purposes (Geser 1992; Wendt n.d.). It becomes problematic, however, if it so dominates our thinking that we automatically treat whatever is external to territorial state actors as "not-state" and therefore anarchic. This may obscure the emergence of state powers at the international level that are not concentrated in a single actor but distributed across transnational structures of political authority and constitute a structural transformation of the Westphalian states system. Collective identity formation is an essential aspect of such a process.

All but the most hard-core neorealists would probably say that the international system is characterized by "particles" of governance and that as such it is not a pure anarchy (compare Waltz 1979, 114–6). With the deepening and proliferation of international institutions, these particles are rapidly becoming "sediment." There is widespread agreement that this does not represent a move anytime soon toward centralized authority on a "domestic analogy" (Bull 1977), but neither does it represent a persistence of anarchy, since it does involve authority and "governance" (Rosenau and Czempiel 1992). This suggests that the anarchy-hierarchy dichotomy that has organized the field for so long is problematic (Milner 1991) and opens the door to new thinking about the foundations of international politics.

In response to this challenge, some scholars advocate non-state-centric thinking on the assumption that the concept of the state is inherently tied to centralized authority (Ferguson and Mansbach 1991). In contrast, other emphasize the mutability of state forms, differentiating the state from sovereignty and even territoriality (Ruggie 1993b; R. Walker 1990). One way to develop this latter suggestion would be to define the state as, at base, a structure of political authority that performs governance functions over a people or space (Benjamin and Duvall 1985; Katzenstein 1990). The enactment and reproduction of this authority structure may or may not be centralized in a single actor. In the Westphalian system, state agents and authority structures did coincide spatially, which leads to the familiar billiard ball imagery of "states" (actors, under which authority structures are subsumed) interacting under anarchy. But the two concepts need not correspond in this way: political authority could in principle be international and decentralized (Ruggie 1983b; Pasic 1993). Following Robert Cox, we might call such transnational structures of political authority that lack a single head "international states" (1987, 253-65).10

The concept of authority has a dual aspect: legitimacy (or shared purpose) and coercion (or enforcement) (Ruggie 1983b, 198). This suggests that the internationalization of the state requires the development of two qualities: identification with respect to some state function, be it military security, economic

growth, or whatever and a collective capacity to sanction actors who disrupt the performance of that function. The result of such developments would be an institutionalization of collective action, such that state actors would regard it as normal or routine that certain problems will be handled on an international basis (Nau 1993). This expectation is likely to be expressed and met in various ways: norms, rules, and principles that define expectations for behavior; routine discussions of collective policy; and interorganizational networks among bureaucratic agencies (Geser 1992; Hopkins 1978). By themselves, these forms are not international states, but to the extent that they manifest and contribute to collective identity and enforcement, they will be part of an internationalization of political authority.

This is present today in various degrees among advanced capitalist countries in both security and economic issues. The provision of security (and more specifically the maintenance of a territorial monopoly on organized violence) is a key function of the state. "Monopoly" normally refers to control by a single actor, but it can also denote control by multiple actors if they are not rivals and engage in institutionalized collective action. A collective security system is just that—joint control of organized violence potential in a transnational space. Such control with respect to external security has long characterized NATO, and recently European Community states have begun to internationalize internal security as well (Den Boer and Walker 1993). This system has a high degree of legitimacy among its members, as well as some capacity for enforcing its policies on them. It is based on multilateral norms that give even its weakest members a say in decisions (Risse-Kappen 1991), its leaders routinely formulate policies together, and its militaries are linked organizationally for both operations and procurement. Its capacity for institutionalized collective action is certainly also lacking in many respects, as the recent difficulties of defining a European defense policy attest, but the internationalization of political authority is a continuum, and NATO is far from anarchy along it.

The provision of an institutional framework for capitalist production is another function of the state that is today being internationalized. Historically, capitalism was largely a territorial phenomenon, in which capitalists could direct their political efforts toward corresponding domestic authorities. As competition drove them to expand overseas through trade and investment, however, they created a demand for international rules and regulations. State actors responded with a network of regimes: a trade regime to govern the flow of goods and services; a monetary regime to govern the value of transactions; and, increasingly, a "capital regime" to govern property rights and capital flows (Duvall and Wendt 1987). Today these regimes do more than merely affect the prices of certain behaviors; they embody a degree of collective identity ("embedded liberalism"), routinized discussions of collective policy, and networks of interorganizational linkages. Their principal weakness remains enforcement, but even here there are emerging sanctioning systems that enable us to speak of an internationalization of political authority with respect to global capital (Cox 1987).

Let me emphasize that I am not saying that international state formation has gone very far, any more than has the formation of collective identities that is one of its prerequisites. It is a process, and even if it continues we are only in its early stages. It is issuespecific (though it may "spill over" into new issue areas), mostly regional in nature, and a matter of degree. Moreover, there are strong arguments for thinking that it will not continue, since it creates fundamental tensions between the national and transnational responsibilities of state actors (Taylor 1991). My point is merely to suggest some ways of thinking about certain dynamics in the contemporary world system that do not privilege the dichotomy between anarchy and hierarchy in statist theory (or its counterpart of intergovernmentalism versus supranationalism in integration theory). The key to such thinking is recognizing that political authority need not be centralized, a point recognized implicitly long ago by the integrationists and, since the neorealist interregnum, it is being rediscovered by a variety of scholars today (Pasic 1993). To be sure, centralization or supranationality may facilitate an internationalization of political authority, and the latter may even create some incentives for it. But one might also argue that decentralized governance arrangements will facilitate the process, since by preserving the forms if not substance of sovereignty, they may pose less tangible political threats to state actors against which resistance might coalesce.

The internationalization of political authority has at least two broad implications for international relations theory. First, it points toward a gradual but structural transformation of the Westphalian states system from anarchy to authority. The basis of that system is the institution of sovereignty, which constitutes an anarchy of mutual recognition. Even when international state formation does not involve the formal cession of sovereignty to supranational institutions, it does relocate individual state actors' de facto sovereignty to transnational authorities. The result in practice might be a "disarticulated" sovereignty in which different state functions are performed at different levels of aggregation (Pogge 1992), and/or a "neo-Medievalism" in which political authority is shared by both state and nonstate actors (Bull 1977, 254-94). Either way, the result is neither anarchy nor hierarchy but the emergence of a new form of state—and thus states system—which breaks down the spatial coincidence between state-as-actor and state-as-structure. Thus the erosion of individual state sovereignty does not imply the erosion of the state. Sovereignty is not an intrinsic feature of state agency but one social identity a state may have. By transferring it upward to a collective, states may actually strengthen their capacity to solve problems. Internationalization is a way of reorganizing and

redeploying state power, not a withering away of the state.

A second implication is how this calls into question the premises of contemporary democratic governance. The Westphalian approach to sovereignty allowed democratic and international relations theorists to ignore each other. The former were concerned with making state power democratically accountable, which Westphalia constituted as strictly territorial and thus outside the domain of international relations theory; the latter were concerned with interstate relations, which were anarchic and thus outside the domain of political theory. Under this worldview, democrats could celebrate the "end of history" with hardly a peep about democracy at the international level.

The internationalization of the state makes this silence problematic. 11 As state actors pool their de facto authority over transnational space, they remove it from direct democratic control. Territorial electorates may still retain the formal right to "unelect" their leaders, but the ability to translate this right into tangible policy change (versus merely changing the faces in power) is constrained by the commitments that state elites have made to each other. New elites could in principle break those commitments, but often only at the cost of external sanctions and reductions in their own effectiveness (Dahl 1993). Various interpretations of the threat this poses to territorial democracy can be imagined. The current debate in the European Community over the "democratic deficit" (Williams 1991) treats it in largely liberal terms as one of controlling an emerging centralized power, but radical democratic theory might be more relevant for the more decentralized authority structures cropping up all over the international system. In either case, the attempt to solve international collective action problems by creating collective identities among states creates an entirely new problem of making those identities democratically accountable, a problem ultimately of transforming the boundaries of political community (Linklater 1990; Wolfe 1992). Solutions might take transferritorial or functional-corporatist forms but will somehow have to expand "the people." In contrast to their splendid isolation in the sovereignty regime, democratic and international relations theorists might want to work on this one together.

CONCLUSION

It is widely thought that state-centric systemic international relations theory cannot explain structural change and so ought to be abandoned. In my view, the problem lies not with statism but with two other commitments that inform contemporary understandings of structural theory: realism and rationalism. The essence of the former is materialism, *not* a willingness to confront the ugliness of world politics as its proponents would have it. If system structure is reduced to a distribution of material power, structural change can mean nothing more than shifts in polarity that

will not end the dreary history of conflict and despair over the millennia (but see Deudney 1993). Since authority is an intersubjective concept, the nature and implications of its internationalization will elude a materialist theory. The essence of rationalism, in contrast, is that the identities and interests that constitute games are exogenous and constant. Rationalism has many uses and virtues, but its conceptual tool kit is not designed to explain identities and interests, the reproduction and/or transformation of which is a key determinant of structural change. We should not let our admiration for rationalist methodology dictate the substantive scope of systemic international relations theory (Shapiro and Wendt 1992).

Scholars should not ignore the state as they grapple with explaining structural change in the late twentieth century, but for their part, statists should recognize that when states interact, much more is going on than realism and rationalism admit. Yes, international politics is in part about acting on material incentives in given anarchic worlds. However, it is also about the reproduction and transformation-by intersubjective dynamics at both the domestic and systemic levels—of the identities and interests through which those incentives and worlds are created. Integration theorists appreciated this suggestion long ago, but their nascent sociology of international community has been lost in the economics of international cooperation developed by realists and rationalists. The latter has contributed important insights into the dynamics of interaction under anarchy, but is ill-suited as a comprehensive basis for systemic theory precisely because it brackets some of the most important questions such a theory should address. Constructivists bring renewed interest and sharpened analytic tools to those questions.

Notes

An earlier version of this article was presented at the 1993 meeting of the APSA in Washington. The article would not have been possible without the many helpful comments of Emanuel Adler, Mike Barnett, Lea Brilmayer, Mlada Bukovansky, James Caporaso, Martha Finnemore, Mark Laffey, Roy Licklider, David Lumsdaine, James Marino, Jonathon Mercer, Debra Morris, Bruce Russett, Rogers Smith, and seminar participants at Cornell University, Johns Hopkins University, New York University, University of Pennsylvania, Rutgers University, and Yale University; thank you.

- For important exceptions, see Nye 1987 and Jervis 1988.
- 2. For varying expressions of this argument, upon which I draw freely, see Adler 1991; Alker n.d.; Ashley 1987; Katzenstein 1990; Kratochwil 1989; Ruggie 1993b; R. Walker 1990; and Wendt 1992.
- 3. Other traditions of international relations theory that might fall under this heading include poststructuralism, world society theory, neo-Gramscianism, Deutschian integration theory, and perhaps the society of states school. For elaboration of this and other issues discussed in this section, see Wendt n.d.
- 4. For a useful discussion of the importance and nature of basic needs at the individual level see Turner 1988, 23-69.
- 5. As noted, collective identity was a centerpiece of classical integration theory, but the latter's insights have been pushed aside by the realist and rationalist approaches that

- dominate contemporary international relations theory. For recent international relations scholarship that begins to revive the concept, see Alker n.d. 1986; Caporaso 1992, 617-20; Keohane 1984, 120-34.
- 6. Although collective identification operates at the level of social identity, by enhancing the capacity for collective action it can help create corporate identity, which in one sense is simply a (temporarily) "solved" collective action problem. The distinction between the corporate and social identity of groups is therefore itself a construction, signifying the higher self-organization of the former rather than timeless and essential.
- 7. An interesting exception is the rationalist literature on "endogenous preference formation"; see Cohen and Axelrod
- 8. Later in his book, Axelrod discusses the possibility of actors internalizing new interests, but this important point has not been picked up by mainstream regime theorists in international relations.
- 9. Another is Schroeder's (1993) splendid discussion of how the European great powers learned to think in "systemic" (in my terms, essentially "collective") terms in the early
- 10. This concept has been used primarily by neo-Marxists (Cox 1987; see also Picciotto 1991) though see the interesting discussion of Kant's usage in Hurrell (1990). My own thinking on this score, as on many others, owes much to conversations and work with Bud Duvall (see esp. Duvall and Wendt 1987).
- 11. For an early appreciation, see Kaiser 1971; the recent literature includes Connolly 1991; Held 1990; and R. Walker

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RESEARCH NOTES

REASSESSING MASS SUPPORT FOR POLITICAL AND ECONOMIC CHANGE IN THE FORMER USSR

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Ising survey data collected in Russia, Ukraine, and Lithuanian in 1990–92, we reexamine findings reported by Finifter and Mickiewicz (1992). Our analysis indicates a significant link between political and economic reform orientations. Individuals who prefer political reforms of a democratic nature also favor a decreased role of the state in guaranteeing social well-being. In addition, our finding that better-educated Soviet and post-Soviet citizens are more likely than the less-well-educated to prefer individual responsibility for well-being is in direct contradiction to the findings reported by Finifter and Mickiewicz. The differences in the two sets of conclusions give rise to very different substantive conclusions regarding the economic and political changes now occurring in these societies.

inifter and Mickiewicz (1992) present data from a survey of public opinion in the former USSR. They use these data to analyze some of the critical issues in the transition toward new political arrangements, including popular views of whether the state or the individual should be responsible for securing the well-being of the citizenry, as well as popular orientations toward political and economic reform. Their article warrants attention from a wide audience because it presents data from the "first large-scale true personal interview survey in the Soviet Union" (p. 859), because of the centrality of the theoretical issues they address, and because of some unexpected findings. They conclude, for example, that those who have obtained a higher level of education, as compared with the less-well-educated, may be more likely to prefer a strong governmental role in promoting social well-being. They also find significant similarity in the political and economic attitudes expressed by different ethnic subgroups across various republics of the former Soviet Union. Both of these findings are surprising in light of conclusions drawn earlier from the Soviet Interview Project and from research based outside of this region.

Given the importance of the issues that Finifter and Mickiewicz tackle, it is crucial to compare their findings with those obtained from other data so as to prevent preliminary conclusions from becoming accepted as "received wisdom" before they have been verified. Therefore, we reexamine several key arguments put forth by Finifter and Mickiewicz using data both from a series of three large-scale surveys conducted by the University of Iowa and the USSR Academy of Sciences (now Russian Academy of Sciences) in 1990–92 and from American National Election Studies (NES) data, which permit a broader cross-systemic comparison. Based on these analyses, we are able to reinforce and confirm some of their findings, but more importantly, our results contradict

several notable conclusions reached by Finifter and Mickiewicz.

Specifically, our reexamination of these issues demonstrates that the proportion of those who believe that the individual, as opposed to the government, should be responsible for producing social well-being is rather close to the levels found among Americans. The average level of this individualism increased from 1991 to 1992, thereby casting doubt on the argument that 70 years of societywide Soviet indoctrination (although the socialization was admittedly less than fully effective) account for the levels found. In addition, differences in support for individual versus governmental responsibility are influenced significantly by variables (such as income and gender) that Finifter and Mickiewicz found to be unrelated to locus of responsibility and, even more significantly, by variables that they did not even include in their analyses (personal economic situation and rating of the Communist party). Furthermore, the better educated are more likely than the less-well-educated to prefer individual responsibility for well-being over governmental responsibility, thus directly contradicting the findings reported by Finifter and Mickiewicz. Another contradiction involves popular support for political reform, which, contrary to Finifter and Mickiewicz, is influenced by the respondent's nationality. Finally, our data call into question Finifter and Mickiewicz' argument that attitudes toward the locus of responsibility and toward political reform represent distinct dimensions, thereby permitting economic and political reform to move in different directions. Rather, our analyses show strong, positive correlations between the two even when controlling for other key factors.

The discussion and data analysis supporting these contrary conclusions proceed by first examining more fully the concept and measurement of locus of responsibility. Then we turn to testing the four major hypotheses guiding the analysis. The first hypothesis involves reexamining the relationship between edu-

cation and locus of responsibility. Our expectation is that the better-educated will be more supportive of individual responsibility. Second, using a multivariate analysis we investigate the hypothesis that political and economic conditions influence preferences for individual rather than governmental responsibility for well-being. Third, to test the hypothesis that nationality differences are a factor influencing political and economic attitudes, we include nationality as one of the independent variables predicting the locus of responsibility and political reform measures. The expectation here is that attitudes toward political reform should be influenced more strongly by nationality differences than are economic orientations toward the locus of responsibility. Finally, we examine the fourth hypothesis, which involves an expected relationship between attitudes toward political and economic reform.

Our analysis differs from that of Finifter and Mickiewicz in certain respects. Because we have data from only three former republics (Russia, Ukraine, and Lithuania), we cannot fully reproduce their analyses. We can, however, offer a more in-depth understanding and analysis of the relevant hypotheses in these three countries because our sample was designed to be representative of each of these former republics. (See the Appendix for a description of the data.) The Finifter and Mickiewicz sample, on the contrary, was not designed to be representative of particular republics but was supposed to represent the broader Soviet Union. Thus very few respondents were found in any given republic, except Russia. Also, with regard to questionnaire construction, although several of the questions asked by Finister and Mickiewicz were very similar to those asked in the University of Iowa surveys, none of the items that they reported employed exactly the same wording as was used in our interviews. Because specific survey questions are slightly different, we do not reproduce any of their earlier published tables here for direct comparison. We do encourage the reader, however, to make those direct comparisons on their own. Also, to avoid problems associated with attempts to make direct comparisons across individual questionnaire items, we will employ multiple-item scales to tap equivalent underlying concepts. Our time frame is also different: the Finister and Mickiewicz interviews were undertaken only once, at the end of 1989, while our interviews were administered in three annual waves beginning in the summer of 1990 and finishing in the summer of 1992. Because the 1990 survey included fewer survey items relevant to the analysis presented here, we will draw most heavily from the 1991 and 1992 survey data.1

LOCUS OF RESPONSIBILITY FOR WELL-BEING

As Finifter and Mickiewicz note, a core element of socialist ideology involves the locus of responsibility

for social well-being. In order to gauge public support for ideological tenets, we focus on public perceptions of who should be responsible for individual economic well-being: the state or the individuals themselves? Under socialist systems, the state has assumed the major role in caring for individual well-being. As a matter of ideology (and also practice), the socialist state controls property, the means of production, and most aspects of the economy that directly influence one's economic well-being. Socialization within this system, therefore, could be expected to promote both an expectation that the state will continue to provide for social well-being and a preference for the state to assure a high standard of living for every citizen. On the other hand, preferences for individual responsibility are likely to be more prevalent in systems characterized by relatively more laissez-faire market economies. Given these cross-system expectations, the distribution of public attitudes toward the locus of responsibility for the former Soviet Union is of considerable theoretical importance.

The distribution of responses to the question on locus of responsibility for a sample of Russians, Ukrainians, and Lithuanians interviewed shortly before the August 1991 coup attempt and one year later are presented in Table 1. Comparing the responses of the total sample from the 1991 and 1992 studies to those reported by Finifter and Mickiewicz reveals a great deal of similarity. The comparison cannot be made directly, however, because the question asked in our surveys had a middle (pro/con) category, whereas their individual-versus-state-responsibility indicator was dichotomous. But when the percentages for our data are recalculated after excluding the middle category, the distributions are nearly the same as what Finifter and Mickiewicz report. In 1991 only slightly more than half (53.7%) preferred state responsibility and in 1992 the division was virtually the same as the 50.7% found by Finifter and Mickiewicz (see Table 1). Across the three former republics, only Russians displayed a statistically significant, although slight, shift toward individualism between 1991 and 1992. Lithuanians, however, expressed more individualism than did Russians or Ukrainians in both years, thereby confirming a finding reported by Finister and Mickiewicz. Despite the collapse of the Soviet Union at the end of 1991, these attitudes, at least at the aggregate level, appeared quite stable across time. At first glance, therefore, this stability may be attributed to basic, enduring social-

Comparable data from the NES, however, provide a cross-national comparison suggesting that the distribution of preferences may have less to do with ideological indoctrination than with current economic circumstances and realistic judgements regarding what the Soviet state and successor governments could be expected to do under existing economic conditions. The 1988 NES dichotomous distribution excluding the middle category (see Table 1) had 64.1% preferring individual responsibility, signifi-

ı	TABLE 1					
l	State or In	dividual Shou	ld Be Respon	sible for Job	and Standa	ard of Living

RESPONSIBILITY FOR ECONOMIC	TO	ΓAL	RUS	SSIA	UKR	AINE	LITH	JANIA	_	TED TES
WELL-BEING	1991	1992	1991	1992	1991	1992	1991	1992	1988	1990
Trichotomized ^a State (%) Pro/con Individual N	40.7	36.6	41.4	35.5	39.8	38.1	35.0	35.0	28.3	35.5
	24.2	27.8	25.7	30.2	21.6	25.6	21.8	28.5	21.3	21.1
	35.1	35.6	32.9	34.3	38.6	36.3	43.2	36.5	50.4	43.3
	2,911	2,580	1.384	1,243	953	845	574	492	1,725	1,710
Dichotomized ^b State (%) Individual	53.7	50.7	55.7	50.9	50.8	51.2	44.7	48.9	35.9	45.0
	46.3	49.3	44.3	49.1	49.2	48.8	55.3	51.1	64.1	55.0

cantly different from the 46.3% found in the Soviet Union for 1991. Moreover, the difference fits with the theoretical expectations for the two countries (i.e., Socialist countries showing the stronger orientation toward state responsibility). The situation changes, however, in 1990, when the United States was in a deep recession and many more people were expressing the feeling that the state, rather than individuals, should provide for social well-being (see Table 1). Clearly the difference between U.S. and Soviet attitudes on locus of responsibility is much smaller when the 1990 U.S. data are used in the comparison.

What is surprising about these data is not the fact that Soviets were so evenly divided between state and individual responsibility for individual well-being but that a relatively high percentage of Soviets (as compared to Americans) preferred an individual solution. After 70 years of assumed socialist indoctrination and a tendency for people to turn to the state for help in periods of economic crisis, we would have expected that a much higher percentage of Soviets would view the state as the preferred provider of social well-being. Of course, given that the distributions in Table 1 rely upon a single survey item to measure locus of responsibility, the critic might be rightfully skeptical of these results. Fortunately, the surveys we are employing as evidence each include several questions designed to measure the concept of locus of responsibility. Therefore, we can expand the analysis and are not limited to a single item (as were Finifter and Mickiewicz). Indeed, an exploratory factor analysis revealed that four items from our 1991 and 1992 surveys formed a cohesive, moderately intercorrelated set of questions that could be used in an index that ranged from strong preferences for state responsibility to strong preferences for individualism.² The set of four items included the variable discussed in Table 1, a question on concerns

about economic inequality, an item measuring preferences for maintaining state or collective property versus the establishment of private property, and a question on attitudes toward "businessmen" (understood in the Soviet context to mean entrepreneurs). The text of the questions are presented in the Ap-

Briefly, the pattern of responses to the three questions not already discussed tends to indicate a slight shift in an individualistic direction between 1991 and 1992. For example, 43% of the total respondents in 1991—as compared with 35% in 1992—felt that economic inequality was a grave danger to social stability. Also, 43% of the 1991 respondents said they had nothing in common with businessmen, whereas in 1992 that figure declined to 40%. On the question of owning property, among the 1991 sample 33% preferred state or collectively owned property, while 31% favored private property (the remainder wanted some combination). In 1992, the data revealed only 26% favoring state-owned property, while 43% preferred private property. This overall shift in the direction of individualism is also clearly evident in the mean value of the four-item locus of responsibility index. (Overall, the mean shifted from 6.72 to 7.13, where high values indicate support for individualism.) The index means also differed across republics. In both years, Lithuanians were more individualistically oriented (7.77 and 7.84 in 1991 and 1992 respectively) than Russians or Ukrainians (means of 6.67 and 6.73 for the two countries in 1991 and 7.07 and 6.83 in 1992), a difference that is not surprising given Lithuania's historically closer ties with Europe and their early movement to secede from the Soviet Union. In 1991, Russian and Ukrainian preferences were not significantly different. However, the most rapid across-time shift in these attitudes occurred among Russians. By 1992, therefore, Russians were more individualistically oriented than Ukrainians.

Source: University of Iowa's Post-Soviet Citizen Surveys.

"The survey question scale initially ranged from 1 = state responsibility to 5 = individual responsibility for economic well-being. Here we have collapsed the scale as follows: 1–2 = state, 3 = pro/con, 4–5 = individual. The U.S. data employed a seven-point scale, so the collapsing was 1–3 = state, 4 = pro/con, 5-7 = individual.

In this collapsing of the initial scale, we eliminated those responding 3 = pro/con and recalculated the percentages, still having 1-2 = state, 4-5 = individual. The U.S. data also eliminated the center category.

TABLE 2

Percentage Saying the Individual Should Be Responsible for Well-Being by Level of Education (%)

EDUCATION	FORI SOV UNI	IET	UNI' STA	
LEVEL	1991	1992	1988	1990
Secondary education Higher education Pearson r	19.2 43.4 .19*	24.7 45.7 .15*	41.1 56.8 .13*	33.7 49.4 .12*

Source: University of Iowa's Post-Soviet Citizen Surveys. Note: Table entries are the percentage giving the individual response to the trichotomous version of the question presented in Table 1. Education is dichotomized here into those with secondary education (or less) versus those with more-than-secondary education. $^*p < .001$.

EDUCATION HYPOTHESIS

Our major concern with the Finister and Mickiewicz report, however, is not the distribution of responses to the locus-of-responsibility questions. On the contrary, in the case where we have comparable questions, the estimate of the distribution derived from their data and that presented in Table 1 are very similar. Rather, the major problem is how these attitudes on responsibility for well-being are related to other variables, particularly such demographic variables as education and income.

Most surprising is the negative correlation they find with education, namely, that the better-educated favored state responsibility for well-being, whereas the lesser-educated favored individual responsibility (Finifter and Mickiewicz, 1992, 866). As Finifter and Mickiewicz point out, this negative correlation is contrary to that found earlier with data on Soviet emigres (Silver 1987). In addition, Duch recently found a correlation between education and endorsement of free-market culture variables (1993, 102) that was the opposite of the finding reported by Finister and Mickiewicz (although we are not arguing here that the measures of free-market culture and locus of responsibility are tapping the same attitudes). The negative relationship reported by Finifter and Mickiewicz also runs counter to the correlation obtained with the single-item measure in our 1991 and 1992 data and even contradicts the relationship found with U.S. data (see Table 2). Moreover, the correlation between the four-item locus-of-responsibility index and education is positive and significant (.19 and .15 in 1991 and 1992 respectively). Given this overwhelming evidence, one can only conclude that the better-educated in both the former Soviet republics and in the United States are significantly more likely to endorse individual responsibility for providing a high quality of well-being than are the lesser educated. Thus, the results in Table 2 are not only consistent across time and cross-culturally, but they

better match theoretical expectations than those presented by Finister and Mickiewicz.

The interpretation that Finifter and Mickiewicz brought to the education relationship they found was twofold. First, they argued that the better-educated were really the ones who were benefiting from the material and psychic gratification provided by the Soviet state (1992, 866). Second, the better-educated were altruistically concerned about providing a safety net for the less fortunate in society, so that they were more supportive of the state bearing the burden of providing social welfare for its citizens (p. 867).

The data in Table 2, however, suggest a very different interpretation. It is more likely that the less-fortunate in the former Soviet societies realized their dire need for state support simply to survive. The better-educated, on the other hand, preferred a system that allowed for more individual initiative and entrepreneurship either because of materialistic motivations or because they desired more freedom and self-determination. Since they were more likely to benefit from a system that gave more emphasis to individualism, they were also more willing to accept greater individual responsibility for their own well-being.

ECONOMIC AND POLITICAL CIRCUMSTANCE HYPOTHESES

The analysis presented by Finifter and Mickiewicz, however, went well beyond the correlation of education and locus of responsibility. To explain the variation and differences in locus of responsibility, Finifter and Mickiewicz utilized a multivariate regression analysis (1992, 869, Tbl. 8) incorporating a number of demographic variables and a measure of life satisfaction. A comparable analysis using our 1991 and 1992 data is presented in Table 3 (cols. 1-2, 5-6). As might be expected with a dependent variable that is a multiple-item index and thus is more reliable and has less-restricted variance than a single question, the explained variance for the equations presented in Table 3 is three times the magnitude of that found by Finifter and Mickiewicz. More importantly, only two of the six (excluding ethnicity controls) independent variables included in the equations—age and life satisfaction—exhibit relationships with the locus-ofresponsibility measure that confirm the Finister and Mickiewicz analysis. In both 1991 and 1992, younger Soviets and those with a more positive sense of life satisfaction were more likely than older and dissatisfied people to prefer individual orientations to government responsibility for social well-being. The regression coefficients for these two variables are similar to those displayed by Finifter and Mickiewicz (1992, tbl. 8).

The major discrepancies between the Finister and Mickiewicz study and the data reported here arise with respect to education, gender, income, and ethnicity. The contradiction involving the education relationship was mentioned earlier. It is sufficient,

TABLE 3					
Regression Equation	Predicting	Locus o	f Responsibility	for Socia	l Well-Being

INDEPENDENT		19	91			19	92	
VARIABLES	b	BETA	b	BETA	b	BETA	b	BETA
Personal Finances ^a								
Past		_	.41**				.74**	
			(.03)	.10			(.03)	.18
Future		_	.56**				.53**	
			(.03)	.14			(.03)	.14
National economy past ^a	_		.01		_	_	.18	
ramena seemeny paer			(.05)	.00			(.05)	.02
Rating of CPSU ^b	_		−.69**				53**	
riding or or oo			(.02)	28			(.02)	22
Life satisfaction ^c	.30**		.23**		.61**	,	.17*	
Life Satisfaction	(.02)	.09	(.02)	.07	(.02)	.18	(.02)	.06
Age	~.05**		03**		~.05**		04**	
nge .	(.00)	28	(.00)	19	(.00)	29	(.00)	23
Education	.16**		.16**		.09*	140	.07*	.20
Ludcation	(.01)	.12	(.01)	.11	(.01)	.06	(.01)	.06
Gender ^d	.82**		.71**		.71**	.00	.56**	.00
Gender	(.04)	.14	(.03)	.12	(.04)	.12	(.03)	.10
l lab a a la mal		, . 1 -		.12		.12		.10
Urban/rural	33* (.04)	05	16 (.04)	03	06 (.03)	04	07 (.02)	02
	(.04)	05		03	(.02)	04		02
Income	.00*	05	.00*	05	.00*	45	.00*	10
	(.00)	.05	(.00)	.05	(.00)	.15	(.00)	.12
Ethnicity	07				0.4		45	
Ukrainian	.07 (.04)	.01	.13	.02	34 (OF)	05	45 (.04)	0E
		.01	(.04)	.02	(.05)	05		05
Lithuanian	1.47**	07	.81	04	.54	00	.19	01
Constant	(.14)	.07 3.6	(.13)	.04 .09	(.14)	.03 4.8	(.14)	.01 7.7
Adjusted R ²		3.6 .15		.09 .24		4.6 .19		7.7 .27
Number of cases	2,4		. 2,4			97		53

Source: University of Iowa's Post-Soviet Citizen Surveys.

Note: The locus-of-responsibility index ranges from 1 to 15, where 1 indicates state responsibility and 15 indicates individual responsibility for well-being. Standard errors are in parentheses. b = unstandardized regression coefficient, beta = standardized coefficient. "Better off = high.

therefore, to note that the additional controls for other variables do not change the relationship observed in Table 2; that is, better-educated Soviets favored individual solutions to attaining well-being, while the lesser-educated were more likely to look to the state—exactly opposite to the finding reported by Finifter and Mickiewicz. Furthermore, Finifter and Mickiewicz reported that gender and income had no significant impact on locus of responsibility. The data in Table 4, on the contrary, reveal a consistent and relatively strong effect for gender: in both years, women were more likely than men to favor the state as the provider of social well-being. Likewise, Soviets with lower incomes, particularly in 1992, expressed the stronger preferences for the state as the guarantor of social well-being. Regarding nationality differences, Lithuanians were more supportive of individual responsibility than were Russians and Ukrainians. However, the impact of nationality will become more meaningful when we return to a broader discussion of this variable.

Except for life satisfaction, all the predictors included in the Finifter and Mickiewicz analysis were social-structural variables. As they point out, life satisfaction was included because it has been seen as a cultural attitude that influences the development of political democratization and economic reform (Inglehart 1990). We shall not here attempt to discuss or empirically examine a number of other theoretically relevant political and economic variables that go well beyond the largely demographic variables utilized by Finifter and Mickiewicz. We point out, however, that none of the variables used by Finifter and Mickiewicz indicate the extent to which locus of responsibility

^bPositive = high. ^cSatisfied = high.

 $^{^{}d}$ Male = high.

p < .01.

was a response to recent change in economic conditions. Also, none of their variables are explicitly political in nature, so they failed to capture the potential impact of contemporary political circumstances. In an effort to address these limitations, we shall briefly extend our investigation to an analysis of how public response to the economic and political circumstances of the time may have affected locus of responsibility orientations.

Earlier findings from political-economy research suggest that both economic and political circumstances could potentially influence public attitudes toward the role of government in guaranteeing the basic economic well-being of citizens, as well as support for a particular type of economic system. (For extensive reviews of this literature, see Kiewiet 1983 and Lewis-Beck 1988.) This research implies that perceived changes in national economic circumstances (sociotropic considerations) will influence collective economic evaluations more strongly than personal or "pocketbook" concerns (Kinder and Kiewiet 1979). Additionally, the earlier research suggests that prospective expectations of how economic conditions may develop in the future can influence political and economic orientations as much as retrospective assessments (Fiorina 1981; MacKuen, Erikson, and Stimson 1992; Miller and Wattenberg 1985).

Duch has recently applied these various arguments to an analysis of survey data collected in the former Soviet Union in 1990 (1993, 591). In doing so, he hypothesized that Soviet citizens who were experiencing a deterioration in their own economic situation or perceived the national economy as deteriorating, as well as those who expected these economic failures to continue in the future, would be more likely to support free-market changes because they would blame the command economy for the current and future economic woes. This same argument could be extended to incorporate the impact of economic circumstances on attitudes toward the locus of responsibility. Also, if the Soviet regime was being blamed for economic failures, both national and personal, then we should expect that those citizens who were most negative toward communists should be most supportive of economic reform. We would hypothesize, therefore, that anticommunist sentiment should promote support for an individually oriented locus of responsibility throughout the late 1980s and early 1990s.

However, by 1991—and certainly by 1992, after the collapse of the Soviet Union—the impact of deteriorating economic conditions may have taken a different turn than that hypothesized by Duch (1993). Under *perestroika* (Gorbachev's economic restructuring program), a good deal of lip service was given to new economic freedoms and the encouragement of new entrepreneurship. Indeed, by 1991, there were emerging signs of these changes. For example, citizen-owned cooperatives were starting to spring up. The collapse of the Soviet Union at the end of 1991 also produced a dramatic increase in the possibility for new entrepreneurship. Real economic conditions

continued to deteriorate, but the possibility for individual economic improvement changed significantly during this period. An alternative hypothesis, therefore, is that those who were hardest hit by the collapsing economy needed to look to the state for assistance, whereas those who were actually doing equally well or better during this period, relative to the past, could only hope that they would do even better under a market-oriented, private enterprise system that gave more emphasis to individual responsibility.

The empirical evidence from 1990 to 1992 confirms that this was a period in which former Soviet citizens perceived a dramatic deterioration in their economic circumstances. The percentage of respondents reporting that their personal financial situation worsened "relative to the past year" rose from 40% in 1990 to 56% in 1991 and 61% in 1992. Over this same period, only 8-10% said their economic situation had improved relative to the past. Nonetheless, expectations regarding future economic circumstances actually became slightly more optimistic during this period. In 1990, when respondents were asked to look ahead one year, 63% said they would be worse off in the future. By 1991, respondents giving that pessimistic assessment had dropped to 49% and it fell to 45% in 1992. At the same time, respondents saying they would be better off in the future rose from 7% in 1990 to 11% and 15% in 1991 and 1992 respectively. Likewise, over this period of time former Soviet citizens were increasingly negative in their evaluations of the Communist party. Ratings of the communists became sharply more negative between 1990 and 1991 (rising from 49% to 60% rating communists negatively in the two respective years) and remained predominantly negative through 1992, when 62% rated the communists negatively.

Clearly economic and political circumstances in the former Soviet Union changed significantly between 1990 and 1992—but with what impact on broad economic orientations such as the preferred locus of responsibility? To answer this question and to test the hypotheses we have raised, the regression equations presented in Table 3 were recomputed after adding four new predictor variables to the analysis. The first two items are the retrospective and prospective assessments of one's personal financial situation. The third measure indicates the respondent's judgment of whether the national economy had improved or worsened during the past year, while the fourth measures evaluations of the Communist party on a five-point scale.

The resulting coefficients demonstrate that both personal economic change measures and attitude toward communists had a strong impact on preferred locus of responsibility in 1991 and 1992. Statistically, these new variables contribute nearly 10% more explained variance beyond that provided by the original set of variables in Table 3. The results suggest that changing economic conditions had a major impact on attitudes toward responsibility for economic wellbeing. Between the times when the two surveys were

in the field, Russian president Boris Yeltsin and his administration undertook a series of governmental measures that liberalized prices, ended government subsidies of some industries, and capped some wages. The result was hyperinflation that reduced or eliminated many people's savings and replaced limited supplies of cheap goods with readily available goods that few could afford. To maintain an adequate standard of living for one's family required most adults to scramble for extra income. This new challenge produced tension, fears for the future, and (for some) a higher appreciation of Soviet-era conditions. Those who succeeded in securing adequate income to keep pace with inflation were likely to enjoy the wider range of available goods, but those whose living standards fell were likely to be disillusioned with what is often called "barbarian capitalism." Similar trends were underway during this period in Lithuania and in Ukraine—the latter because its economy was closely interconnected with the Russian economy. For these reasons, we would expect that individual success in surviving harsh economic circumstance would condition one's outlook on market reforms. Indeed, the coefficients in Table 3 demonstrate that Soviets who felt that their own personal economic situation had improved or stayed the same were more inclined toward individualism than those who perceived a worsening situation. Similarly, respondents who were optimistic about future economic improvements were more likely to express support for individual responsibility. Also, those who rated communists very negatively were much more likely to place responsibility for social wellbeing with the individual than were those who rated communists more positively.

An unexpected result revealed by Table 3 is the weak statistical effect for concerns about national economic conditions.³ This finding contradicts those studies arguing that sociotropic concerns generally outweigh the impact of pocketbook concerns. The results obtained here, however, point to an important set of circumstances, relevant to contemporary Eastern European and post-Soviet societies, that condition the relative impact of pocketbook and sociotropic assessments. Because the economic deterioration in these countries has been so widespread and visible, a large majority perceive that the national economy has declined and will most likely decline even farther before it begins to improve again. Under these circumstances, similar to those of a depression in Western democracies, individual survival and opportunity become far more important to each person than concerns about what is happening at the national level. Indeed, by 1991 the perception that the Soviet national economy had worsened during the past year had become almost universal (92% of the survey respondents said it had worsened), thereby providing virtually no explanation for differences in attitudes toward the locus of responsibility. Despite slightly more variance in the 1992 public assessments of the national economic performance in these new post-Soviet countries, 85% of the combined respondents said that the national economy of their country had further deteriorated during the past year. Given such widespread pessimism about the national economy, it is understandable that differences in more immediate, personal concerns are predominant.

Including the personal economic situation variable in the multivariate analysis does more than just demonstrate the strong impact that short-run changes in the economy can have on broader economic orientations such as locus of responsibility. The addition of the personal economic variable also changes the interpretation that can be given to the social structural variables included in the equation. Variables such as age, education, and place of residence (urban/rural) have previously been used by some writers as indicators of longer-term change, such as socialization or modernization effects (see e.g., Hahn 1991; Lapidus 1989). However, each of these indicators can also be expected to be correlated with short-term fluctuations in economic conditions. For example, the quality of life among elderly people on fixed incomes will be influenced far more by rapid price increases than will the life-style of younger people. Having controlled for the confounding effects of recent economic change, the statistically significant social structural variables actually take on increased relevance as indicators of longer-term socialization effects and the impact of social change (e.g., modernization) on the development of individualistic responsibility.

Yet more revealing, however, is the substantial impact of anticommunist sentiment on locus of responsibility. Even without trying to impose a particular causal interpretation on this relationship, it is important because it demonstrates a substantial link between broader economic ideology and political evaluations. Support for the communist system and striving for individualism were clearly at odds. Indeed, a separate analysis for 1991 that went beyond Table 3 reveals that the highest levels of endorsing individualistic effort as the source of well-being occurred among those who were alienated from the all-union government while trusting the republiclevel leadership. Clearly political orientations were among the most important factors affecting locus of responsibility. Moreover, the strong impact of anticommunist sentiment on locus of responsibility demonstrates that materialistic benefits were not the only motivations underlying broader economic orientations.

SUPPORT FOR REFORM: NATIONALITY DIFFERENCES

The inability to confirm the results presented by Finifter and Mickiewicz also extends to their analysis of Soviet attitudes toward political reform, especially as political reform relates to nationality differences. For example, Finifter and Mickiewicz conclude that "Russians, Ukrainians and Balts did not differ very much from each other in the degree of support for

TABL	E 4				
Rapid	Versus	Slow	Reform	by	Nationality

	RUSSIANS IN RUSSIA		UKRAINIANS IN UKRAINE		LITHUANIANS IN LITHUANIA				
RAPID/SLOW REFORM	1990	1991	1992	1990	1991	1992	1990	1991	1992
Pro-rapid reform (%)	43	43	37	39	46	37	86	75	45
Neutral (%)	16	19	18	20	17	20	9	15	27
Pro-slower reform (%)	41	38	45	41	37	43	5	10	28
Number of cases	397	979	821	284	495	507	340	347	242
Means	5.16	5.2 9	4.84	4.99	5.33	4.87	7.65	6.80	5.41

Source: University of Iowa's Post-Soviet Citizen Surveys.

Note: Means are calculated for a scale that ranges from 1 to 9, with high values indicating stronger pro-reform orientations.

political change" (1992, 862). Yet recent scholarship reveals that major forces for reform came from social and regional groups mobilized on the basis of nationalist symbols, grievances, and demands (Clemens 1991; Lapidus 1992; Motyl 1992).

Our hypothesis, which arises from our earlier work on identities (Miller, Hesli, and Reisinger 1993), is that nationality has a profoundly different impact depending upon the issue or policy area under consideration. More specifically, attitudes that relate directly to political reform can be expected to be more strongly influenced by nationality differences, while orientations related to economic reform and responsibility are less subject to nationality differences. These expectations arise from an understanding that the notion of political reform at the end of the Gorbachev era had clear anti-Soviet-state underpinnings and that support for political change often implied support for a restructuring of the Soviet system of authority. Thus political change became a rallying point for the multiple nationalist movements operating in the period from 1989 through 1991. Economic reform, however, presumably was less closely tied to nationalist aspirations, and a broader range of orientations toward state versus private ownership and economic responsibility should be apparent within national groupings.

To test the hypothesis that the relative impact of nationality depends upon the issue area, we included nationality as one of the independent variables predicting the locus of responsibility and the political change indices. Table 4 reports the distribution of a multiple-item index constructed by drawing from our surveys those items most similar to those employed by Finifter and Mickiewicz. The items include three questions from an agree/disagree battery:

- 1. It is better to live in an orderly society than to allow people so much freedom that they become disruptive.
- 2. These days Stalin is not given adequate credit for building of socialism.
- 3. Political reform in this country is moving too rapidly.

Also included in the index is a rating given of the Communist party, according to the following instructions:

Now we would like to get your feelings toward certain groups using a five-point scale, where 5 indicates a very positive view and 1 a very negative view. You may use any number between 1 and 5 to tell me how favorable or unfavorable your feelings are for each group. If you have difficulty answering, just tell me and we will go on to the next one.

These items were selected because of their internal theoretical consistency: each item represents an important aspect of the reform process. Item 1 corresponds most directly with Finifter and Mickiewicz item q22d, which makes reference to the trade-off between freedom and order. Item 3 corresponds to Finifter and Mickiewicz item q18, which makes specific reference to the pace of change. Item 2 is included because the repudiation of Stalin played a critical role in early efforts to legitimize the new reform effort. The reference to the Communist party is included because the focus of much of the reform effort was the intransigence, corruption, and lack of responsiveness of the Communist party.

It is noteworthy that both Russians and Ukrainians were generally less supportive of rapid change than were Lithuanians. Many of the explanations that have been offered in the literature for the relatively weaker manifestations of perestroika-era nationalism in Ukraine as compared with the Baltic countries, may account as well for the hesitancy of Ukrainians whole-heartedly to support rapid reform (see Armstrong 1990; Laitin 1992; Motyl 1987; and Nahylo 1991). Ukrainians from the eastern and southern regions exhibited levels of assimilation (russification) unmatched anywhere else in the Union, with the possible exception of Belorus. Adoption of Russian as their native language by Ukrainians in these regions was extremely high, while in West Ukraine (which was incorporated late into the Soviet Union during World War II and where most of the nationalists tended to reside) knowledge of Russian was much lower. Thus the majority of the population of Ukraine (found in the southern and eastern regions) were uncomfortable with the demands for radical change emanating from outside of their own region.

As of 1992, however, the republics were independent and formally in control of their own destiny. Following the collapse of the Soviet Union, we might

expect the notion of political change to take on a different meaning—a shift that is suggested by the 1992 data in Table 4. In general, the data reveal a decline, as of 1992, in support for rapid reform (across all three national groups). Nevertheless, even after taking the general trend of declining support for political change into consideration, Lithuanians continued to exhibit stronger pro-reform orientations, thereby contradicting the Finifter and Mickiewicz findings.

Our longitudinal data also fail to support the Finifter and Mickiewicz findings regarding the impact of context on the attitudes of Russians residing in non-Russian republics. At the outset of the 1990s, approximately 24 million Russians lived in the non-Russian republics. This substantial presence of Russian minorities in a variety of places where the culture and climate of opinion is very different from that found in Russia raises the possibility that the local context may have influenced the attitudes of the nonindigenous Russians in a way that would produce preferences that differ from those expressed by Russians in Russia. Indeed Finifter and Mickiewicz contend that their data support the general argument that "the political attitudes of individuals who change their residential locations tend to be influenced by the political and social context into which they move; such individuals tend to change their attitudes in the direction of conformity with their new neighbors" (1992, 864). The evidence they provide in support of this contention is that Russians living in Kazakhstan, Uzbekistan, and Ukraine were very similar to the indigenous nationalities in terms of their attitudes toward political change (see p. 863). Likewise, they conclude that "the Baltic peoples . . . do not differ significantly from Russians in Estonia on support for political change" (p. 864).

Given that we have already demonstrated a similarity between Russians and Ukrainians in 1990 and 1991 in outlooks on the pace of change, the most telling comparisons involve a focus on differences between Russians and Lithuanians. As indicated in Table 4, indigenous Russian and indigenous Lithuanian attitudes toward change do differ, so that it is possible to test a hypothesis that predicts a similarity of attitudes between Russians living in Lithuania and Lithuanian nationals. Among Russians living in Lithuania in 1992, the group mean on the reform index is 4.96. This indicates that Russians living outside of Russia are very similar (as a group) to those living inside of Russia (see Table 4) but are different from Lithuanians. In fact, across all three years, the reform attitudes of Russians living in Lithuania are more similar to those of Russians in Russia than they are to those of titular Lithuanians.

The finding that Russians in Lithuania fail to share the attitudes of indigenous Lithuanians makes a good deal of sense. After all, Lithuanians on the whole were relatively negative toward Russia and Russians, sentiments that stem from their forced entry into the Soviet Union after World War II. Moreover, with independence came increased discussion about ex-

TABLE 5 **Regression Equation Predicting Support for**

Political Reform

INDEPENDENT	199)1	199	92
VARIABLES	b	BETA	b .	BETA
Life satisfaction ^a	298** (.029)	070	.161* (.030)	.043
Age	058** (.001)	276	~.053** (.002)	259
Education	.279** (.013)	.160	.180** (.014)	.106
Gender ^b	.415** (.047)	.061	.446* (.051)	.069
Rural residence	755** (.056)	096	.021 (.061)	.003
Income	.000 (000.)	.011	.000 (000.)	.071
Ethnicity				
Ukrainian	.124 (.055)	.016	.195 (.058)	.027
Lithuanian	2.74** (.168)	.112	.619* (.180)	.037
Constant	11.4	41	9.	57
Adjusted R ² Number of		15	•	11
cases	2,1	88	1,9	36

Source: University of Iowa's Post-Soviet Citizen Surveys.

Note: The pro-reform index ranges from 1 to 17, with high values indicating greater support for rapid reform. Standard errors are in parentheses.

"Satisfied = high.

clusionary policies that would make voting and the holding of public jobs dependent on the ability to speak Lithuanian—changes that could limit the opportunities for people who speak only Russian. Russians in Lithuania, therefore, would have good reasons to support slower change and to be concerned about possible social disorder, because they, as a group, would be disadvantaged by rapid change.

In order to assess the impact of nationality on support for reform relative to other potential social cleavages in post-Soviet society, we included nationality and a number of other demographic characteristics in a multiple regression predicting pro-reform attitudes. The particular demographic predictors included in the equations were selected so as to produce a multivariate analysis similar to the one presented by Finifter and Mickiewicz (1992, tbl. 8). Our analysis confirms the Finifter and Mickiewicz findings for a number of the predictor variables, particularly the effects for age, education, and gender on attitudes toward political change (see Table 5). The propensity of younger people to be relatively more supportive of political change is apparent in both 1991 and 1992. These age differences may reflect generational socialization effects, or they may indi-

^bMale = high.

^{*}p < .01.

cate that young people are better able to withstand the traumas of the times and are less negatively impinged on by dramatic changes in the system. Similarly, the more highly educated respondents were consistently more reform-oriented than their less-well-educated counterparts. We agree with the Finifter and Mickiewicz explanation that the principle beneficiaries of the recent political changes have more often been the better-educated sectors of Soviet and post-Soviet society (1992, 866). The results reported in Table 5 also confirm that women have been uncomfortable with the pace and direction of political change. Finifter and Mickiewicz interpret this finding as meaning that women are generally more conservative than men (p. 867). However, as we have previously argued, orientations toward reform are not necessarily a reflection of women being more conservative but rather of women perceiving the changes that are currently taking place as having a disproportionately negative impact on them (see Hesli and Miller 1993).

The rest of the multivariate analysis fails to support the Finifter and Mickiewicz findings. First, it is noteworthy that the power of the demographic variables to explain the variance in pro–reform attitudes drops over the period. The fact that these same variables could explain less variance in pro–reform attitudes in 1992 indicates that political change took on a different meaning after 1991. The decline in explained variance over this period may suggest that socialization and modernization effects were more important predictors of pro–reform attitudes in the early phases of the Gorbachev period but that other factors (presumably more current political and economic forces cutting across demographic cleavages) had greater impact at the end of the Gorbachev era.

Other contradictions involve the significant impact of Lithuanian nationality and the relationship with urban/rural residence in 1991. The Lithuanian case provides support for the hypothesis that even with controls for other demographic characteristics, Lithuanians expressed more support for political change than did Russians or Ukrainians. In addition, the magnitude of the coefficient associated with Lithuanian nationality is greater in Table 5 than in Table 3, indicating a more powerful impact of nationality differences on political rather than economic reform orientations. Place of residence (either urban or rural) also had an independent and significant impact on orientations toward reform in 1991. The direction of the relationship in 1991 indicates that people living in the rural areas were less comfortable with rapid reform than were their urban counterparts. It is noteworthy that the rural residence effect is not operative in 1992. If urban/rural differences can be explained by a simple reference to "greater conservatism in rural areas" (Finifter and Mickiewicz 1992, 868), then rural respondents in 1992 should also be more conservative. A full-fledged explanation of observed differences is beyond our present scope, but let it be said that the particular circumstances of the times must be taken into account when offering

explanations for anti-reform orientations. Rural people cannot in all cases be assumed to be conservative and traditional. (For more on this argument, see Turner et al. 1993).

RELATIONSHIP BETWEEN SUPPORT FOR POLITICAL AND ECONOMIC REFORM

The most telling discrepancy between the Finifter and Mickiewicz findings and our replication, however, does not involve these demographic relationships. Rather, the most important divergence in the two sets of findings involves the relationship between support for political change and attitudes toward the locus of responsibility for economic well-being. Finifter and Mickiewicz report that preferences on political change and broad economic orientations reflect two separate independent dimensions of Soviet public opinion. 6 The implication of this multidimensionality of attitudes, according to Finister and Mickiewicz, is that economic and political change could occur independently of each other and take very different directions, at least if public preferences have a strong impact on changes. Such a finding is not only unexpected but certainly contradicts much of the broader literature on political economy, as well as the suggestions of some analysts of economic change in communist states (e.g., Mason 1992).

To reexamine Finifter and Mickiewicz's finding of no significant relationship between political and economic orientations, we employ data from our 1991 and 1992 surveys. The results resoundingly disconfirm the Finifter and Mickiewicz conclusion that two independent dimensions underlie these attitudes. Our data reveal a strong positive correlation between support for political reform and preference for individualism and a market economy (the Pearson correlation coefficients are .431 for 1991 and .453 for 1992).

However, given that similar demographic variables influence both sets of attitudes, the correlations might be spurious. A more stringent test of the interrelationship of political and economic orientations would be to examine that interrelationship while controlling for demographic correlations. Because correlations do not reveal the direction of causality, we face the question whether the measure of political reformism or the measure of economic reformism should serve as the dependent variable. Others' recent analyses suggest, however, that political orientations have a stronger impact on economic outlooks than vice versa (Duch 1993; Gibson 1993). We therefore employ the locus-of-responsibility measure as the dependent variable in a multivariate regression. The results of this regression are shown in Table 6.

In both years, a strong positive association exists between support for political reform and a favorable view of individual initiative even after controlling for various demographic characteristics and the signifi-

TABLE 6

Regression Testing Hypothesized Relationship between Support for Political Reform and Locus of Responsibility

INDEPENDENT	19	91	19	92	
VARIABLES	b	BETA	ь	BETA	
Support for					
political reforma	.27** (.02)	.32	.29** (.02)	.33	
Personal finances ^b	` '		` '		
Past	.32** (.05)	.07	.61** (.05)	.15	
Future	.44** (.05)	.11	.42** (.05)	.11	
Life satisfaction ^o	.15* (.07)	.05	.10 (.07)	.03	
Age	04** (.00)	21	03** (.00)	−.18	
Education	.16** (.04)	.08	.08* (03)	.05	
Gender ^d	.53** (.10)	.10	.52** (.11)	.09	
Urban/rural	17 (.14)	02	09 (.14)	01	
Income	.00* (00.)	.05	.00** (00.)	.09	
Ethnicity					
Ukrainian	.12 (.13)	.02	20 (.13)	03	
Lithuanian	.67 (.39)	.03	.45 (.40)	.02	
Constant Adjusted R ² Number of	3.	.92 .28	3.83 .33		
cases	2,1	28	1,9	38	

Source: University of Iowa's Post-Soviet Citizen Surveys.

Note: Locus-of-responsibility and pro-reform indices are defined in Tables 3 and 5, respectively. Standard errors are in parentheses.

cant economic (retrospective and prospective personal financial situation) and life satisfaction variables employed in Table 3. Moreover, adding the measure of support for political reform improves the explanatory power of both years' models beyond that obtained in Table 3.

The results of testing this model are largely the same for the 1991 and the 1992 data. One difference between the two years deserves note. Even after adding the political reform measure to the equations, the question asking whether the economic situation of the respondent's family had improved, stayed the same, or became worse over the past year had twice as powerful an impact in 1992 as in 1991, whereas the impact of the prospective measure remains unchanged. The strong positive coefficient for assessments of one's financial situation, particularly the retrospective assessments in 1992, thus confirms the results reported earlier in Table 3.

The picture that our data present seems clear. Mass orientations toward economic and political reform are not independent of each other: they do not oppose but rather reinforce one another in the former Soviet Union. Those most accepting of the former Soviet regime's political institutions and norms look least favorably on marked norms and institutions, and vice versa. This relationship holds true for both years of our surveys and even when we control for such key societal cleavages as age and level of education.

CONCLUSION

How do we explain the discrepancies between our findings and those reported by Finister and Mickiewicz? In part Finifter and Mickiewicz themselves provide the answer, as they acknowledge that "single items are generally less valid than multi-item indicators" (1992, 861). Our analysis certainly does suggest that there were some problems with their Q22a as a single-item measure of attitudes toward the economic system. Whether these were problems of translation, question clarity, or data entry errors we do not know. To a great extent the discrepancies revealed by our analysis focus on that single item. Thus the validity of the entire data set is not necessarily called into question. (For more on concerns about the validity of the earliest surveys coming out of the Soviet Union, see Miller, Reisinger, and Hesli 1993.) Of course, the rapid change occurring in the Soviet Union during the Gorbachev era could also have contributed to the differences between our results and those reported by Finifter and Mickiewicz. The longitudinal data presented here, especially in Table 4, reveal the significant shifts in attitudes toward political reform that were taking place during the last days of the Soviet Union. It is possible, therefore, that some of the discrepancies could be accounted for by attitude volatility rather than limitations in the data.

Regardless of the discrepancies between the two sets of data, we should not lose track of the fact that on a number of points the various data are in agreement. Moreover, such data, particularly the longitudinal data, provide an empirical basis for verifying hypotheses regarding life and politics in the former Soviet societies. Clearly this is a tremendous improvement over what could be done in an earlier period when the collection of such data was not permitted.

The longitudinal data, as well as the regressions presented in Table 6, demonstrate that Soviet public attitudes on economic and political reform were more interconnected and consistent than Finifter and Mickiewicz suggest (1992, 869). Individuals who preferred political reforms of a democratic nature also favored a decreased role for the state as guarantor of economic well-being. Being a political reformist,

^{*}p < .01.
**p < .001.

[&]quot;Pro-reform = high.

Better off = high.
Satisfied = high.

 $^{^{}d}$ Male = high.

therefore, does imply an abandonment of the socialist system in favor of increased individual responsibility even when it comes to the social welfare of citizens. In short, *perestroika*, which included policies that presumably promoted both political democracy and a shift toward decentralization, was a reflection of public preferences.

In addition, the longitudinal data reveal significant variation, both cross-sectionally and across time, in support for democratic and economic reform in the Soviet and post-Soviet societies. The analysis demonstrates that the explanations for this variation in public support for reform are complex and multifaceted. The self-interested pursuit of material benefits suggested by Finifter and Mickiewicz (1992, 870), is no doubt one factor accounting for the variation in support of reform. However, socialization, political ideology, and the public's response to current economic and political change may be even more important for understanding variation in support or opposition to continued reform in the post-Soviet societies. All of this, of course, leaves us with a challenging agenda for future research.

APPENDIX

The goal of the University of Iowa's Post-Soviet Citizen Surveys (PSCS) conducted in 1990, 1991, and 1992 was to interview a representative sample of adult citizens living in three Soviet republics: Lithuania, Ukraine. and the portion of Russia west of the Ural Mountains. The particular republics were selected on the basis of size of population, economic importance within the Soviet Union, and political relevance. Russia and Ukraine are the republics with the largest populations and highest gross national products among the 15 republics of the former Soviet Union. Although Lithuania accounted for less than 2% of the Soviet population, it was politically very important because, being among the Baltics, it represented the early moves toward secession from the Soviet Union. Primary funding for the surveys was provided by National Science Foundation Grants SES 90-23974 and SGER 90-09698. Soviet support for the project was provided by the Soviet Peace Fund, the Soviet Peace Committee, and the Soviet Political Science Association.

The Sample

The sample size for the study was 1,800 completed interviews in 1990, 3,000 in 1991, and 2,700 in 1992. The study design called for proper representation of the larget population in each of the three republics. The total number of interviews in 1990 was divided equally among the republics, 600 in each. In 1991, the target sample was divided as follows: 1,400 in Russia, 1,000 in Ukraine, and 600 in Lithuania. The 1992 sample was comprised of 1,300 interviews in Russia, 900 in Jkraine, and 500 in Lithuania.

The sample approach used for selecting respondents was a four-stage stratified sample. At the initial stage, each republic was divided into strata reflecting the extent of urbanization, geographic region, the distribution of nationalities, and population density. These criteria produced eight strata for Ukraine and Russia and four strata for Lithuania. The second stage of the sample involved listing the places that fell into each strata and selecting, with probabilities proportionate to size, a number of primary sampling units from each strata. The 1990 sample, because of limited funds, employed 40 primary sampling units, whereas the 1991 and 1992 studies had 81 primary sampling units. The third stage of the sample involved enumerating all the voting districts for each of the primary sampling units. Between 4 and 23 voting districts were randomly selected for each primary sampling unit depending on

the size of the place. The final stage of sampling involved selecting specific individuals from the voter lists—cross-referenced with residence records—for each of the selected voting districts. The procedure involved the calculation of a sampling interval n for each particular list, selecting a random number between 1 and n as the starting point and then selecting every nth person from that starting point. No substitution of respondents was allowed. The overall response rates in the studies were high by Western standards: 89.4%, 83.7%, and 87.4% 1990, 1991, and 1992, respectively. The response rates were virtually the same for each republic.

Ouestionnaire Construction

The survey instrument was designed through a series of meetings between the United States research team and a group of Soviet scholars: Andrei Melville, Alexander Nikitin, and Elena Bashkirova. The questionnaire was translated from English into Russian, Lithuanian, and Ukrainian and back-translated in the Soviet Union and in the United States using native-speaking Russians, Lithuanians, and Ukrainians. This translation-back-translation process was carried out a number of times until an agreement was reached on the particular translation to be employed. Prior to the final revisions, a pretest was conducted in Moscow, Kiev, and Vilnius. Neither the questionnaire nor the interviewers gave any indication that the survey was part of a collaborative project involving American researchers. The average interview took 38 minutes in 1990 and 45 minutes in 1991 and 1992. All interviews were conducted from 18 May to 21 June 1990, from 20 May to 25 June 1991, and in late June 1992. All questionnaires were later transported to the United States, where they were used to code open-ended questions and to correct any data entry errors.

Interviewing

The interviewers were recruited and initially trained by staff from the USSR Academy of Sciences, Institute of Sociology in Moscow, under the supervision of Elena Bashkirova. In addition, the American investigators provided further interviewer training sessions prior to the start of each study period. During the interviewing period, the American scholars traveled to the various sample points to supervise the data collection and to be certain that appropriate interviewing procedures were being followed.

Our experience of having conducted three waves of interviewing in the republics of the former Soviet Union has served to reinforce the need to take standard precautions aimed at assuring valid and reliable data. In addition to the usual concerns about sampling, proper translation of questions, and the appropriate training of interviewers, we feel that any researcher conducting survey research in the former Soviet Union must spend time in the field supervising the data collection, as well as verifying the quality of the interviewing and the accuracy of the recorded data. In addition, it is incumbent on the researcher to retrieve and archive the completed questionnaires so they can be used for correcting data entry errors. Finally, as a contribution to the advancement of the profession and as a means of checking the veracity of published results, all data used in scholarly work should be made available to others for secondary analysis through the ICPSR. The authors may be contacted for a more detailed description of the surveys.

Questionnaire Items Used in the Locus-of-Responsibility Index

- 1. Some people say the government of Russia [Ukraine, Lithuania] should guarantee everyone work and a high standard of living, others argue that every person should look after himself. On this card is a scale from 1 to 5, where 1 signifies that the government guarantees everyone work and 5 that every person should look after himself. Which position corresponds to your view?
- There should be a mechanism regulating income such that no one earns very much more than others. (agree/disagree)
- 3. Speaking only of Russia [Ukraine, Lithuania], what type of property would be best: private property, collective property or only state property?

4. Some people think that certain groups in society have too much influence on life and politics in Russia [Ukraine, Lithuania] and that other groups don't have as much as they deserve. Do you think "Businessmen" have too much, too little or the right amount of influence?

Notes

We wish to thank Chia-hsing Lu and Andy Peebler for assistance in the data analysis and Peggy Swails for her secretarial assistance.

1. The data from the 1990 survey is now publicly available through the ICPSR. The 1991 and 1992 survey data will be available for reanalysis by the end of 1994.

2. The Cronbach alpha reliability coefficients for a four-item scale were .68 and .63 for 1991 and 1992 respectively.

3. For statistical significance, we are restricting the probability of type I error to less than .01. The item, however, had relatively little variance, so that some evidence of a relationship between evaluations of the national economy and locus of responsibility does exist.

4. Cronbach's alpha for the proreform index in each of the three former republics for 1991 and 1992 is as follows:

Year	Russia	Ukraine	Lithuania
1991	635	.635	.448
1992	.656	.478	.407

- 5. The regression analysis for 1990 is not presented in Table 5. All demographic variables, and the single attitudinal indicator have the same directional impact as in 1991 and 1992. The major difference is the fact that the set of variables explains a larger portion of the variance in 1990 as compared with the later years. We excluded 1990 from the regression analysis because the proreform index can be expanded to a 17-point scale if we rely only on the 1991 and 1992 data. In these years, the options given to respondents on the agree/disagree battery employed five options from strongly agree to strongly disagree. In 1990, the options were limited to agree, neutral, or disagree.
- 6. Finifter and Mickiewicz actually report a slight negative correlation between locus of responsibility and political reform (1992, 861). But on the very same page they also report that the results of a factor analysis demonstrate that Q22a, the single item measuring locus of responsibility "is orthogonal to the other items."

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BAYESIAN INFERENCE FOR COMPARATIVE RESEARCH

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Regression analysis in comparative research suffers from two distinct problems of statistical inference. First, because the data constitute all the available observations from a population, conventional inference based on the long-run behavior of a repeatable data mechanism is not appropriate. Second, the small and collinear data sets of comparative research yield imprecise estimates of the effects of explanatory variables. We describe a Bayesian approach to statistical inference that provides a unified solution to these two problems. This approach is illustrated in a comparative analysis of unionization.

widely used tool in quantitative comparative politics is the linear regression model, estimating relationships between institutional and economic variables observed for a set of countries constituting some population of interest. A brief survey of recent comparative research finds no shortage of work of this type. For instance, Remmer (1991) identifies economic crisis as a source of electoral volatility in Latin American countries in the 1980s; Robertson (1990) presents evidence that institutions moderating the transaction costs of negotiations between capital and labor reduce industrial conflict; and Golden (1993) finds that union wage moderation in industrialized democracies results from coordinated collective bargaining.

Although the methodological advantages are well documented (Jackman 1985), we identify two distinct problems of statistical inference endemic to this type of quantitative comparative research. First, because the data constitute all the available observations from a population, conventional inference based on the long-run behavior of some repeatable data mechanism is not appropriate. Second, because data sets tend to be small and collinear, the effects of explanatory variables are estimated imprecisely. In most applications the first problem is generally ignored and the second is solved by imposing restrictions on the regression model to obtain "sensible" results.

The Bayesian model of statistical inference provides a unified solution to these two distinct problems of quantitative comparative research. Two features of Bayesian inference are important in this context. First, probability is conceived subjectively as characterizing a researcher's uncertainty about the parameters of a statistical model. This subjective probability concept seems particularly useful in comparative settings, where the data are typically convenience samples not generated by a known probability mechanism such as random sampling. Second, Bayesian inference allows the inclusion of other information, in addition to the quantitative sample information. Again, this seems useful in the comparative context where rich historical material is commonly available, sparking ideas for researchers, but formally discarded in the final analysis. In the Bayesian approach, this historical material can be formally incorporated into the analysis.

We begin by detailing why the nonstochastic and weak data typical of comparative research are problematic for conventional linear regression analysis. We next introduce some ideas about Bayesian inference and describe Bayesian regression analysis. These ideas are applied to a comparative analysis of unionization—the subject of a recent exchange in the American Political Science Review between Michael Wallerstein and John Stephens (Stephens and Wallerstein 1991; Wallerstein 1989). The Bayesian regressions we present are supplemented with a sensitivity analysis that investigates how our conclusions depend upon the sample data and our prior beliefs.

Although we survey some ideas in Bayesian statistics, a full introduction to the Bayesian approach to statistical inference is beyond our scope here. (Good book-length treatments are Pollard 1986, Lee 1989 and, at a slightly higher level, Press 1989.) Our intention here is to spotlight problematic but ignored issues of statistical inference in a common area of application. The Bayesian alternative that we present represents just one way forward to address these inferential problems.

TWO FEATURES OF COMPARATIVE DATA ARE PROBLEMATIC

A common design in comparative research generates data with two important characteristics: (1) the data constitute all the available observations from a population; and (2) the data, because of small sample size and collinearity, tend not to be very informative about the statistical parameters being estimated. Although our focus on comparative research is stimulated by the preponderance of studies with data of this type, our arguments are generalizable to other areas using data with these characteristics. For example, studies comparing the American states could well be another area of application (e.g., Barrilleaux and Miller 1988; Erikson, McIver, and Wright 1987).

Nonstochastic Data

Unlike analysts of survey research data or experimenters who randomly assign subjects to treatments, comparativists often collect all the available observations from some population of interest. For example, populations including "advanced industrial societies" (Wallerstein 1989), "contemporary democratic political systems" (Powell 1982, 2), and the "affluent market-oriented democracies" (Swank 1988, 1121) have all been studied by comparative researchers. In contrast to experimental or social survey data, comparative data are not generated by a repeatable data mechanism. Repeated applications of survey or experimental designs, on the other hand, yield new data sets with more information about the process under study. In comparative research, once all the data are collected from a population, further applications of the data collection process do not yield more information. Data of this type, generated by a nonrepeatable and unknown probability process, are described by Freedman and Lane (1983) as "nonstochastic." We adopt their terminology here.

To see why nonstochastic data create problems for conventional statistical inference, we briefly review the concept of frequentist probability. This idea is the workhorse of conventional, or frequentist, statistics. In 1837, Denis Poisson defined probability as the limiting distribution of a long-run relative frequency. If, for example, a coin is tossed n (a very large number) times and shows m heads, we can write the prob(heads) $\equiv p = m/n$, assuming that m/n coverges. In this case, the probability of heads, p, is the proportion of heads that will be observed when n grows to infinity. The probability, p, like size or weight, describes an objective characteristic of the coin (Barnett 1982, chap. 3; Leamer 1978, chap. 2).

Frequentist statistical inference assumes that data are generated by a repeatable mechanism such as the coin flip. Von Mises provides the paradigmatic statement of this view: "In order to apply the theory of probability we must have a practically unlimited sequence of uniform observations" (quoted in Barnett 1982, 76). A sample observation is thus just one possible result from many possible draws from a probability distribution. In practice, these draws are implemented by probability sampling (as in a social survey) or random assignment (in an experiment). Frequentist inference makes conclusions about a parameter (perhaps a mean or a regression coefficient) obtained from a sampling or assignment process, as if that process was repeated a large number of times. For example, a random sample survey of American adults may indicate that mean income in the United States is \$35,000. Assuming (rather implausibly) that income is normally distributed, we could estimate a 90% confidence interval for our sample mean, perhaps [\$15,000, \$55,000] for a modestly sized sample. Using conventional frequentist inference we can conclude that intervals like the one calculated would cover the true (population) mean income 90% of the time for repeated applications of the sampling procedure. The "repeated sampling" inference tells us neither whether the population mean lies within the estimated interval nor even with what probability the mean lies in the interval. Specifically, the frequentist inference does not entitle us to claim there is a 90% chance that the true mean falls within the estimated interval. The only available conclusion concerns the long-run behavior of a statistic—in this case, the 90% confidence interval.

While frequentist inferences from survey data allow conclusions about probability sampling processes, nonstochastic data create problems for frequentist inference because they are not generated by a repeatable mechanism. Frequentist inference is simply unrealistic given the manner of data collection. Comparative researchers sometimes show an uneasy awareness of this problem. In their analysis of advanced industrial democracies, Lange and Garrett frankly report that they "adhere to traditional standards [i.e., significance tests] while remaining unsure of their applicability" (1987, 268). In a similar vein, Weede endorses the use of t-statistics for testing hypotheses in his study of welfare state size in industrial democracies, not only for methodological reasons but also because "it is almost universal practice in econometrics and the social sciences" (1986, 518). More confusion is revealed in Gorin's comparative study of inequality that "cannot utilize probability theory to ascertain the level of significance" for want of "random samples"; he goes on to note that asterisks indicate a ".05 level of significance" (1980,

Comparative researchers' discomfort with frequentist inference is well founded because frequentist inference is inapplicable to the nonstochastic setting. It is simply not relevant for the problem at hand to think of observations as draws from a random process when further realizations are impossible in practice and lack meaning even as abstract propositions. In short, frequentist inference answers a question that comparative researchers are not typically asking (see also Freedman and Lane 1983, 189).

Two objections might be raised at this point. One challenge might be that statistical inference is unnecessary in the nonstochastic setting because all the available information is collected. This position is certainly valid, but it still commits the researcher to a substantive theory of how the data were generated. In particular, if there is no uncertainty associated with the data and if statistical inference is expendable, the researcher is effectively claiming that things could not have been different, that the data were generated by a completely deterministic process. This suggests a thought experiment: If we could set in motion, once again, the historical conditions that gave rise to the data, would the ensuing process generate a data set identical to the one actually obtained? Commitment to this type of determinism is the cost of abandoning statistical inference (Berk, Western, and Weiss 1993).

The second objection holds that although the data mechanism is not repeatable, it can be treated as if it were. In this view, the social world randomly draws observations from a set of all possible observations, or a "superpopulation" (Cochran 1953, 169). The data are one realization of all possible data sets that might have been collected. Or as one reviewer put it, "each country's history is one draw of a distribution of possible histories." While this assumption avoids a deterministic theory of the data process, it is highly speculative compared to positive knowledge about a sampling procedure or, in Fisher's uncompromising phrase, "the physical act of randomization" (quoted in Freedman and Lane 1983, 197). Even more troubling, however, is the conclusion that conventional inference allows in this instance. Take the example of a confidence interval for a mean where we can conclude that under repeated realizations (which are acknowledged to be impossible), the interval would cover the true mean 90% of the time. We have no way of knowing whether the current interval is one of the fortunate 90% and no possibility of further replications. (Our focus here has been on the confidence interval because this is the inferential tool on which we shall rely in our application and the one that implicitly underlies classical hypothesis tests. Similar criticisms of classical hypothesis testing and other aspects of frequentists inference are reviewed in Barnett 1982, 180-88).

Weak Data

The problem of statistical inference in comparative research is exacerbated by the weakness of comparative data. These data are weak in the sense that they provide little information about parameters of statistical models. The uninformative character of the data is reflected in large standard errors and consequently large *p*-values.

Weak comparative data have two distinct causes. First, the sample sizes characteristic of comparative research are small in relation to the number of parameters being estimated. While comparativists frequently make do with less than a hundred data points, survey researchers frequently have more than a thousand. For example, comparative researchers have defined the population of advanced capitalist democracies to include between 15 and 21 countries (Lange and Garrett 1985; Wallerstein 1989). Sometimes, even where samples sizes are fairly large, a relatively large number of parameters must be estimated (e.g., Williams 1991). Ideally, if more data could be collected this would be one solution to the uninformative data problem. As the sample size increases in relation to the number of parameters, the estimated variances of the regression coefficients would tend to decline.

Collinearity is the second cause of weak comparative data. In a comparative setting, collinearity is common because the explanatory variables are themselves often causally related. When explanatory variables are collinear, they carry little independent information about the various regression coefficients. A

regression on collinear predictors can yield estimates with unexpected signs and large standard errors.

To foreshadow some Bayesian ideas, collinearity provides no statistical difficulties except in very extreme instances (of exact or near-exact linear dependencies) which are seldom encountered in practice. The least squares estimators retain their properties, and statistical inference can proceed as normal. Why, then, is collinearity regarded as problematic? The difficulties caused by collinearity—large standard errors and unexpected signs for regression coefficients—are only problems with respect to prior expectations about the signs and variances of the coefficients (Leamer 1978, 170). As Leamer argues, collinearity is thus not a statistical problem but a problem of the interpretation of multidimensional evidence (pp. 170–73). As a tool of interpretation, the least squares estimator in the presence of collinearity does not allow us to distinguish information about one coefficient from information about another. Instead, the least squares estimator is informative about linear combinations of coefficients. The introduction of nonsample information allows sample information to be allocated among the coefficients according to substantive criteria.

A more familiar approach to collinearity involves constraining coefficients to zero (excluding them from the analysis). This is an informal way of introducing information to obtain sharper estimates of other coefficients. The researcher is implicitly saying, "To obtain a sensible estimate for the effect of x_1 , I must estimate the model on the basis of the substantive claim that x_2 had no effect at all." The substantive claim that x_2 has no effect allows the weight of the sample evidence to be allocated in favor of x_1 . Here, of course, a "sensible" estimate of the regression coefficient for x_1 is based on (typically unstated) prior expectations. Dropping x_2 introduces known specification errors, and in part defeats the motivation for statistical control that led to multivariate techniques in the first place. (The papers in Granger 1990 on model specification provide an elaborate treatment of this issue from Bayesian and non-Bayesian perspectives in econometrics.)

In sum, the weak evidence of comparative research that results from small data sets and collinearity yields weak or fragile inferences that are highly sensitive to the model specification. This is the natural outcome of an analysis based on little evidence. The only solution to this problem is the introduction of more information. But as a practical matter, more information is frequently not available. Data have been collected from all the countries in the sample. The growing popularity of pooled cross-sectional times series designs suggest one remedy to the weak data problem (e.g., Alvarez, Lange, and Garrett 1991; Beck et al. 1993; Radcliff 1992; Swank 1992); but additional time points often provide little new information because the processes being studied show far more cross-national than longitudinal variation (e.g., Wallerstein 1989, 482). Institutions, for example, do not change much over time but show considerable

cross-national variation. Furthermore, the collinearity problem is distinct from small sample size and can afflict the pooled cross-sectional design irrespective of the number of observations. However, while extra quantitative information is typically unavailable, large and substantively rich stores of qualitative information from comparative and historical studies are often present but not available in a form suitable for analysis. Bayesian procedures enable the weak quantitative information of comparative research to be pooled with the qualitative information to obtain sharper estimates of regression coefficients. Although we shall focus on situations in which the data are weak, Bayesian methods can also be applied (generally with less consequence) where the data are highly informative.

BAYESIAN INFERENCE

The Bayesian approach to statistical inference involves pooling nonsample (or "prior") information with sample data to formulate posterior subjective probability statements about the parameters of a statistical model. This description of Bayesian inference signals two aspects of the Bayesian approach that distinguish it from conventional inference. First, Bayesian inference is built upon a subjective probability concept. Second, Bayesian inference allows the introduction of prior information in addition to the sample information to make statistical inferences.

Subjective Probability

In contrast to the frequentist concept that refers to objective probability features of the world, subjective probability refers to a person's *degree of belief* in an uncertain event. Subjective probability is thus a personal statement of certainty or confidence, rather than a fact characterizing an object in the external world. Subjective probabilities share the same axioms as objective probabilities and are consistent with formal rules for rational decision making (Barnett 1982, chap. 3). The difference between subjective and objective probability resides in whether the probability statement refers to an individual's personal feeling of confidence, or whether it characterizes an objective feature of the world.

Prior Information in Regression

In Bayesian inference, researchers' subjective probability assessments of the parameters of a statistical model are pooled with the sample data to arrive at posterior probability statements about those parameters. The prior information is represented in a probability distribution. In the normal regression case that we are concerned with, this prior probability distribution can be summarized by prior means and variances for the regression coefficients. (Tanner 1993 considers more complicated applications.) The posterior probability statements express the researchers'

degree of belief in the parameters, given the data and the prior, replacing conventional inference about the distribution of coefficient estimates in repeated sampling. We can begin to see how prior information is incorporated into a data analysis by first reviewing how two data sets might be pooled in a regression analysis.

Two data sets, (X_1, y_1) and (X_2, y_2) , can be pooled for a regression analysis as follows:

$$\begin{bmatrix} y_1 \\ y_2 \end{bmatrix} = \begin{bmatrix} X_1 \\ X_2 \end{bmatrix} \beta + \begin{bmatrix} u_1 \\ u_2 \end{bmatrix}.$$

Following Leamer, if we write σ_1^2 and σ_2^2 for the two residual variances, the least squares estimate of β from the pooled data is

$$b_{pooled} = (\sigma_1^{-2} X_1' X_1 + \sigma_2^{-2} X_2' X_2)^{-1} (\sigma_1^{-2} X_1' X_1 b_1 + \sigma_2^{-2} X_2' X_2 b_2), \quad (1)$$

where $b_1 = (X_1'X_1)^{-1}X_1'y_1$, and $b_2 = (X_2'X_2)^{-1}X_2'y_2$ (1978, 76).

Although equation 1 may look a bit cumbersome, the pooled estimate for β is just a (matrix) weighted average of the two sets of least squares estimates obtained from each of the data sets.

Equation 1 is useful from the Bayesian point of view, because it suggests how sample and prior information can be combined to obtain posterior distributions for regression coefficients. The regression coefficients estimated from the first data set, b_1 , can be replaced by a set of regression coefficients that the researcher believes a priori are most probable. These are the prior means, written b^* , of the prior probability distributions we have referred to. Note that the inverse of the first part of the first term in equation 1 is the estimated variance of the coefficients from the first data set. This variance can be substituted by the subjective Bayesian prior variance, V^* , for the coefficients. Uncertainty about the prior means is reflected in the specification of V^* . Larger variances for a prior mean imply greater uncertainty. Prior covariances—off-diagonal terms in the prior covariance matrix-can also be specified if beliefs about one coefficient depend on beliefs about others -say, if one is estimating main and interactive effects in a regression equation and if confidence in the interaction effects depends on confidence in the main effects (e.g., Lange and Garrett 1985). The prior covariances, expressing dependent beliefs about the regression parameters, thus do not directly relate to prior beliefs about relationships in the sample data. Because beliefs about the coefficients are generally independent, the prior covariance matrix for the regression coefficients is often specified to be diagonal.

Substituting the prior information into equation 1 provides an expression for the posterior mean based on a combination of the prior and sample information:

$$b = (V^{*-1} + \sigma^{-2}X'X)^{-1}(V^{*-1}b^* + \sigma^{-2}X'y)$$
 (2)

where X and y designate the sample information (the subscripts are now redundant) and the residual variance, σ^2 , is estimated from the sample data. (Note that when the expression for the sample parameter estimate is expanded in equation 1, the cross-product matrices in the second term cancel out, leaving X'y.) The variance of *b* is given by the first term of equation 2 and standard deviations for the posterior coefficients are the square roots of the diagonal elements of this matrix. An alternative route to these expressions for the posterior means and variances of the regression coefficients is to multiply a normal prior distribution by a normal likelihood for the sample data, treating the residual variance as known. This is an application of Bayes Theorem, and it yields a multivariate normal posterior distribution for the coefficients. The posterior means and variances of this multivariate distribution are identical to the expression we obtained in equation 2. The pooling exposition highlights, however, how subjective prior information can supplement the weak sample data common in comparative research.

Approaching the Bayesian regression problem in this way, the posterior is simply a pooled estimate based on the sample data and the "data" represented by the researcher's prior beliefs. Bringing prior information to bear on an inferential problem via Bayes Theorem involves the same procedure as if one were to collect more data: in both cases, the researcher pools the sample information with the prior distribution or additional data using equation 1 or equation 2.

The resolution of collinearity problems through the introduction of prior information is made clear by the expression for the posterior variance—the first term of equation 2. If prior means are set to zero, a diagonal prior covariance matrix will improve the conditioning of the explanatory variables, resulting in regression coefficients with smaller variances. This estimator is a special case of the generalized ridge estimator (Belsley 1991, 300), except that in the Bayesian case the ridge for the sample cross-products matrix is chosen according to substantive criteria rather than arbitrarily. Bayesian regression thus solves the problem of collinearity by using more information than ordinary least squares, generating smaller standard errors for the regression coefficients.

A convincing Bayesian analysis goes further than just calculating the posterior distributions. Because the two ingredients for Bayesian regression—the prior and the sample information—influence the final results, a thorough analysis should explore the sensitivity of these results to the prior and the data. Robust findings yield substantive conclusions that are insensitive to small changes in the sample and nonsample information. These ideas will be detailed below.

In sum, Bayesian regression analysis provides answers to the problems of comparative research that we have identified. First, subjective probability replaces the conventional concept, providing a more realistic basis for analyzing data collected without a repeatable data mechanism. Second, more informa-

tion is brought to the analysis through the prior distribution. Weak data resulting from small samples and collinearity (common in comparative research) are bolstered by the prior, increasing the precision of the estimated regression coefficients. Finally, the robustness of Bayesian results can be explored through an analysis of the sensitivity of the posterior to the sample data and the prior information.

BAYESIAN REGRESSION ANALYSIS IN A MODEL OF UNION DENSITY

We illustrate Bayesian regression analysis in the context of a recent exchange between Wallerstein and John Stephens. Wallerstein and Stephens are concerned to explain cross-national variation in union density—the percentage of a work force that are union members. Wallerstein argues that the size of the civilian labor force is a key determinant of union density. Stephens claims that density depends on industrial concentration. These two predictors correlate at -.92 and Stephens writes that "because of multicollinearity, economic concentration and the size of the labor force could not be entered in the same equation" (Stephens and Wallerstein 1991, 945– 46). Of course, as noted, no statistical argument prevents the inclusion of both predictors in a regression on union density. Stephens is simply observing that because of the correlation between labor force size and industrial concentration, the data will not be very informative about coefficients for either variable, and the results may be sharply at odds with prior expectations. The problem of collinearity results partly from incomplete data for economic concentration. Stephens uses information about logged gross domestic product to impute missing observations for 9 of the 20 countries in the sample, yet gross domestic product also correlates highly with labor-force size. For this analysis, we bracket the issue of missing data, although it should be noted, with Wallerstein, that Stephens's imputation strategy provides only weak information about economic concentration (pp. 949-50).

Two other features of the Stephens-Wallerstein data are also noteworthy. First, the sample is not generated by any known probability mechanism such as random sampling. The 20 countries comprising the sample were chosen because they experienced a continuous history of political democracy since World War II (Wallerstein 1989, 489). In the absence of some rather fanciful assumptions about probability features of the data-generating mechanism, the classical model of statistical inference is inappropriate. Second, the weak data problem that results from collinearity is compounded by the small sample that provides little statistical power. Stephens implicitly recognizes this problem in the analysis by using a relatively large level of statistical significance (.1). Here, then, is a classic example of weak data generated by an unknown probability mechanism in a

TABLE 1

Prior Means (Standard Deviations) and Their Substantive Interpretations for Bayesian Regression Analysis of Union Density

VARIABLE	PRIOR	SUBSTANTIVE INTERPRETATION
Wallerstein's priors		
Left government	.3 (.15)	One year of left-wing government increases union density by about 1 percentage point. A year of left-wing government may increase union density by as much as 2 percentage points of union density, but its effect is almost certainly not negative.
Logged labor-force size	-5 (2.5)	Doubling the size of the labor force would reduce union density about $\ln(2) \times 5 \approx 3.5$ percentage points. This increase in labor-force size may generate a union decline as big as 7 percentage points, but a growing labor force is unlikely to increase union density.
Economic concentration	0 (10 ⁶)	The diffuse prior indicates that the researcher has no strong prior beliefs about the sign or magnitude of an effect. When the explanatory variables are uncorrelated, the diffuse prior yields posteriors that are approximately given by the sample data.
Stephens's priors	, ••	
Left government	.3 (.15)	Like Wallerstein's prior, one year of left-wing government increases union density by 1 percentage point.
Logged labor-force size	0 (10 ⁶)	Diffuse prior.
Economic concentration	10 (5)	If economic concentration were to increase by 100% in relation to the United States, union density would increase by 10 percentage points. This increase in the concentration ratio may generate a density increase as large as 20 percentage points, but any increase in concentration is unlikely to decrease union density.

Note: Left government is measured by Wilensky's (1981) cumulative index of left-wing government; logged labor-force size is the natural log of the size (in thousands) of the dependent labor force in the year that union density is measured; and economic concentration is measured by the four-firm concentration ratio, in proportion to the United States.

comparative setting. Our data are taken from, and described by, Stephens (Stephens and Wallerstein 1991). Note that labor-force size is measured on the log scale, so that the coefficients express the changes in union density in response to percentage changes in labor-force size. Economic concentration is measured as a ratio to U.S. economic concentration, so that the coefficients express the change in union density as a result of a country's change in economic concentration in relation to the United States.

What light can Bayesian regression analysis shed on the Stephens-Wallerstein debate? From the Bayesian perspective, the source of the controversy between Stephens and Wallerstein is the data that are not strong enough to allow a convincing resolution one way or another. Both researchers informally enlist more information to support their arguments. In Wallerstein's case, this information is a formal theory about the relationship between labor-force size and union density. For Stephens, supplementary information comes from historical material and, in particular, the research of Kjellberg (1983). The Bayesian approach allows the information informally introduced into the analysis by Stephens and Wallerstein to enter formally through a prior distribution.

To begin this analysis, we specify two sets of prior information that represent the opinions of the two researchers. Unfortunately, neither theory is suffi-

ciently precise to unambiguously suggest a prior distribution. (We shall return to this issue.) However, it is clear that both Stephens and Wallerstein are confident that left governments assist union growth. We quantify this confidence with a prior mean of .3 and a prior standard deviation of .15 (Table 1). Because the prior 95% confidence interval— $.3 \pm (1.96)$ × .15)—does not overlap zero, both researchers are quite confident that left government is unlikely to have a negative effect on unionization. As Wallerstein notes, this effect probably suffers from simultaneity bias because unions increase the electoral success of left-wing parties (1989, 490). Because our substantive focus is on the effects of labor-force size and economic concentration, we treat the left-government variable chiefly as a control and simply note that the coefficient will tend to overestimate the impact of left parties on union density. (The problem of simultaneity can be addressed through the prior distribution, but we reserve this issue for another paper; see, e.g., Leamer 1991.)

Prior opinions of the two researchers differ on the labor-force size and economic concentration effects. Wallerstein has a strong belief in a negative labor-force size effect, although his theory does not suggest how large this effect might be. A few quasi-experimental comparisons using information from outside the sample suggests a plausible prior mean. In 1950,

Sweden and Norway shared similar union structures, ethnic homogeneity, and histories of social democracy. The Swedish labor force was about twice as large as the Norwegian, and the unionization gap was about 20 percentage points. If a fifth of the unionization gap (4 percentage points) were attributable to labor-force size, the size effect size would be $4/\ln(2) \approx 5.5$. We get a similar number if we think that about a fifth of postwar American union decline is due to the near-doubling of the U.S. labor force. Choosing a size effect to explain a fifth of the variability in union density reflects, rather arbitrarily, our belief in the quality of the model specification. Fourfifths of the variability in union density is left to the effects of other causes. Confidence in the sign of the effect is reflected in a prior standard deviation that yields a 95% confidence interval that excludes zero.

Stephens believes that economic concentration increases union density, although a prior mean is not obvious from his discussion. Again, we develop a prior based on a quasi-experimental comparison using nonsample information—this time, examining declining economic concentration and union density in the United Kingdom through the 1980s. Measuring economic concentration as the average size of British manufacturing establishments as a ratio of the size of American firms at a fixed point in time, economic concentration in Britain declined by about .3 from the late 1970s to the late 1980s. (See the United Nations Yearbook for Industrial Statistics for 1982 and 1991.) Union density in this period declined by about 15 percentage points. If economic concentration generated about a fifth, or 3 percentage points, of the decline, a plausible prior mean would be (3/.3) = 10percentage points. Confidence in this effect is again supplied through by a prior distribution whose 95% confidence interval does not overlap zero. Note that the style of reasoning behind the specification of these priors is only illustrative. More plausible or more confident specification of prior information would require a much more detailed substantive or theoretical discussion (e.g., Western 1994).

We must also specify a prior for the intercept term, Wallerstein's prior for Stephens's economic concentration variable, and Stephens's prior for Wallerstein's labor-force-size variable. Here, we introduce the idea of prior ignorance that characterizes highly uncertain beliefs about parameters. Prior ignorance about the coefficients can be defined by a zero prior mean and a very large prior variance (Leamer 1978, 62). When ignorance is defined in this way for all the coefficients of the model with a so-called diffuse prior, the posterior distribution converges on the conventional least squares result. Thus, the sample data alone will drive the analysis. If the predictors are uncorrelated and ignorance priors are placed on only a subset of the coefficients, the posteriors for those coefficients will also approach the conventional least squares estimates. In short, this ignorance prior does not so much express the belief that a regression coefficient is zero with great uncertainty: rather, the diffuse prior allows the sample information to domi-

TABLE 2

Posterior Distributions with Noninformative Prior Information in a Regression Analysis of Union Density

INDEPENDENT VARIABLES	MEAN (S.D.)	5TH PERCEN- TILE	95TH PERCEN- TILE
Intercept	97.59 (57.48)	3.04	192.14
Left government	.27 (.08)	.15	.39
Size	-6.46 (3. 79)	-12.70	22
Concentration	.35 (19.25)	-31.32	32.02

Note: These results are equivalent to the ordinary least squares estimates (N=20)

nate the prior information in the calculation of the posterior distribution. We place diffuse (or "ignorance") priors on the intercept term, the economic concentration coefficient for Wallerstein's prior, and the labor-force-size coefficient for Stephens' prior.

Results from conventional least squares estimation are in Table 2. Importantly, diagnostics show that the residuals from this least squares fit are approximately normal and the effects, approximately linear (see Fox 1990, 80–87; Hastie and Tibshirani 1990, chaps. 6–7). Thus the data conform to the structure assumed in the normal likelihood function. From a Bayesian perspective these results can be interpreted as the posterior distribution for a noninformative prior distribution. In addition to the mean and standard deviation of the posterior coefficients, we also report normal approximations for the fifth and ninety-fifth percentiles from the posterior distribution (\pm 1.64 \times standard deviation for the coefficient). These percentiles are simply an alternative way of describing the dispersion and location of the posterior distribution. The results suggest a large range of plausible and negative for values for the size effect. The effect of economic concentration is less certain. The posterior mean is close to zero and the posterior distribution stretches over a large range of positive and negative values. In short, the weight of the data favor the effect of size on union density, but neither parameter is estimated with great precision.

The introduction of prior information can sharpen these estimates. Results for Wallerstein's prior are shown in the top panel of Table 3. With the informative prior, the effect of labor-force size is now estimated with greater precision. Given the prior and the data, we can be 90% certain that the coefficient for labor-force size falls between -9 and -2. The improved conditioning of the data is also reflected in the posterior distribution for the economic concentration coefficient. Although Wallerstein's prior for the concentration effect was diffuse, the posterior mean has

TABLE 3

Posterior Distributions with Stephens's and Wallerstein's Informative Priors in a Regression Analysis of Union Density (N = 20)

INDEPENDENT VARIABLES	MEAN (S.D.)	5TH PERCEN- TILE	95TH PERCEN- TILE
Wallerstein's prior Intercept	82.43 (32.83)	28.42	136.43
Left government	.28 (.07)	.17	.39
Logged labor-force size	-5.44 (2.09)	-8.87	-2.00
Economic concentration	4.87 (12.41)	-15.54	25.28
Stephens's prior Intercept	70.82 (19.87)	38.13	103.51
Left government	.27 (.07)	.16	.38
Logged labor-force size	-4.79 (1.77)	-7.70	-1.88
Economic concentration	9.38 (4.84)	1.42	17.34

become larger and the standard deviation of the posterior has become about one-third smaller. The confidence region, however, still covers zero.

From Stephens's perspective, when prior information is allocated to the economic concentration coefficient, the posterior mean becomes large and positive and the standard deviation of the posterior distribution shrinks by about four-fifths compared to the least squares estimate. The posterior distribution is now completely in the positive range. Again, as prior information is introduced, inference about both coefficients is generally improved. Although the posterior mean for the size effect has shrunk under Stephens's prior, the confidence region is roughly the same as that obtained with Wallerstein's prior. Both sets of prior beliefs support the inference that laborforce size decreases union density. However, only Stephens's prior supports the conclusion that growing economic concentration increases union density.

Sensitivity Analysis

It might be objected here that the introduction of subjective prior information allows the researchers' prejudices into the analysis, corrupting the results that would be obtained from the sample data alone. This criticism has a long history, demonstrated by Fisher's charge that an experiment interpreted with prior information "would carry with it the serious disadvantage that it would no longer be self-contained, but would depend for its interpretation from experience previously gathered. It could no longer be

expected to carry conviction to others lacking this supplementary experience" (1935, 69). A contemporary champion of Fisher's position, Bradley Efron has similarly argued that Bayesian methods "fail to reassure oneself and others that the data have been interpreted fairly" (1986, 4; emphasis original). These comments of Fisher and Efron rests on the idea that a "fair data analysis"—a data analysis without prior information—is possible. In practice, however, prior information enters most analyses through coding decisions, transformations, and unreported searches over sets of explanatory variables to obtain results that look sensible in the sense of falling within an expected range of meaningful results. While all data analysts use prior beliefs, Bayesians go some way to making these priors explicit and integrating them systematically into the analysis. In the Bayesian approach then, acknowledged subjectivity is the route to objectivity (deFinetti 1974, 1:5-6).

Still, those objecting to Bayesian inference make a powerful point. Selecting a prior is subjective in the sense that two researchers will not necessarily agree on its specification. For example, at least part of Stephens's prior belief in the effect of economic concentration is based on his historical discussion of the growth of Swedish corporatist bargaining (Stephens and Wallerstein 1991, 944). Other researchers might believe the Swedish experience is unique in the sense of being uninformative about the more general relationship between economic concentration and unionization in comparative perspective. They might argue that Stephens's prior gives undue weight to the lessons of the Swedish case. What is more, priors are specified with a degree of whimsy in applied settings (Leamer 1983). For instance, although Wallerstein's argument led us to a prior labor-force-size effect of mean 5 and standard deviation 2.5, we could readily agree to an alternative prior, say one with the same mean but standard deviation 2.7. In short, neither several researchers nor even the same researcher, in practice, will prefer one prior probability distribution to the exclusion of all others.

Because the choice of prior is subjective in this sense of attracting little consensus, it is important to investigate how the posteriors depend on the priors. If the posteriors are highly sensitive to the priors, this suggests that the sample data add little to the prior information-indeed that inferences are driven by the priors alone. A parallel argument can be made about the relationship between the sample data and the posteriors. If a small number of observations from the sample data are highly influential for the posteriors, the results are similarly unstable, reflecting information about a few cases rather than the whole of the data set in combination with the priors. In sum, because of the joint influence of the prior information and the data on the analysis, a convincing analysis investigates the sensitivity of the posterior to the priors and the data.

The sensitivity of the posterior to the prior could be examined in several ways. One sensitivity analysis,

TABLE 4

Posterior Distributions for Stephens's and Wallerstein's Diffuse Priors in a Regression Analysis of Union Density

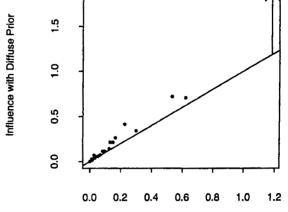
INDEPENDENT VARIABLES	MEAN (S.D.)	5TH PERCEN- TILE	95TH PERCEN- TILE
Wallerstein's prior Intercept	93.50 (52.02)	7.94	179.07
Left government	.27 (.07)	.15	.39
Logged labor-force size	-6.18 (3.42)	-11.81	56
Economic concentration	1.60 (17.68)	-27.49	30.68
Stephens's prior Intercept	80.88 (38.14)	18.15	143.62
Left government	.27 (.07)	.15	.39
Logged labor-force size	-5.42 (2.69)	-9.85	99
Economic concentration	6.11 (12.22)	-13.99	26.20

sometimes called "extreme bounds analysis," investigates variability in the posterior as prior variances are allowed to range from zero to infinity while the prior means are fixed at zero (Leamer 1983). In our application, with nonzero prior means, we simply describe the sensitivity of the posterior to a reduction in the prior information through an increase in the prior variances. An alternative approach might manipulate the prior means, although this should be pursued in the spirit of a sensitivity analysis, leaving a priori uncertainty about the regression coefficients to be expressed in their prior variances.

Note: The diffuse priors are the informative priors multiplied by 10 (N =

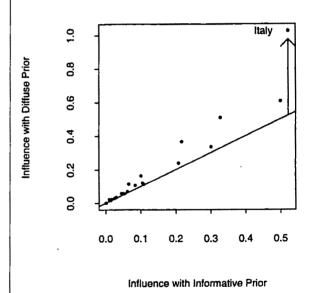
We report two more sets of posterior distributions for a diffuse prior with the prior variances multiplied by 10. Under the diffuse version of Wallerstein's prior, Table 4 shows that the labor-force-size coefficient becomes larger but substantially more imprecise. The 90% confidence region still excludes—but now borders on—zero. The posterior for economic concentration is now effectively centered over zero. For Stephens's diffuse prior, the posterior mean of the concentration coefficient shrinks by about a third, and the standard deviation of the coefficient increases by about three times compared to Stephens's informative prior. The 90% confidence interval now includes a large range of negative values expressing great uncertainty about the effect of economic concentration on union density. Finally, similar to Wallerstein's diffuse prior, the posterior for size gets slightly larger but substantially more imprecise com-

Sensitivity of Bayesian Regression Results to Prior Information and the Sample Data. (a) Wallerstein's prior



Influence with Informative Prior

(b) Stephens' prior



Note: The plot shows Bayesian influence statistics for informative priors (horizontal axis) and diffuse priors (vertical axis). If the sample data from a country have the same influence on the final results, regardless of prior, it will fall on the solid 45-degree line. Italy is an outlier, becoming highly influential as prior information is removed from the analysis.

pared to the posteriors based on the informative priors. To summarize, inferences under both priors become considerably more uncertain as nonsample information is removed from the analysis. A strong inference about the sign of the economic concentration effect depends crucially on the informative version of Stephens's prior.

The sensitivity of the posteriors to the data can be

TADEL 3		
Posterior Distributions with Informative and Diffuse	Priors in a Regression	Analysis of Union Density

	INFORMATIVE PRIOR		DIFFUSE PRIOR			
INDEPENDENT VARIABLES	MEAN (S.D.)	5TH PERCENTILE	95TH PERCENTILE	MEAN (S.D.)	5TH PERCENTILE	95TH PERCENTILE
Wallerstein's prior				w.		
Intercept	54.16 (34.61)	-2.76	111.09	31.19 (56.50)	-61.75	124.12
Left government	.29 (.06)	.19	.40	.29 (.07)	.18	.41
Logged labor-force size	-4.19 (2.12)	-7.67	70	-2.71 (3.57)	-8.58	3.16
Economic concentration	16.29 (13.02)	-5.12	37.70	23.72 (19.41)	-8.20	55.64
Stephens's prior						
Intercept	67.53 (19.01)	36.27	98.80	53.15 (38.93)	-10.88	117.18
Left government	.30 (.06)	.19	.40	.30 (.07)	.18	.41
Logged labor-force size	-4.88 (1.64)	-7.58	-2.17	-4.01 (2.64)	-8.34	.33
Economic concentration	10.87 (4.87)	2.86	18.87	15.88 (12.72)	-5.04	36.80

Note: In this analysis, the outlying case of Italy is omitted. The diffuse priors are the informative priors multiplied by 10 (N = 19). Least squares estimation with Italy excluded yields the following result (standard errors in parentheses): Union Density = 22.08 (63.11) + .30 (.07) × Left Government – 2.12 (4.00) × Size + 26.66 (21.41) × Concentration.

investigated with Bayesian regression diagnostics. Pettit and Smith (1986) discuss a statistic that indicates the influence of each sample observation (or set of observations) on the joint distribution of the posterior (see Appendix). This diagnostic indicates that Italy is highly influential under both informative priors. Both dimensions of the sensitivity analysis sensitivity to the prior and sensitivity to the data—are shown in Figure 1. Here, influence statistics from the informative prior are plotted against influence statistics from the diffuse prior. If the sample data from a country have the same influence on the final results, regardless of the level of prior information, the country will fall on the solid 45-degree line. However, as the prior information is reduced, the sample data generally have an increasing impact on the posterior, so that the quantitative information from most countries becomes more influential as the prior information recedes. Italy grows disproportionately in its influence on the posterior. When prior information is eliminated, this country is shifted upward on the plot and indeed drives a large part of the story.

Table 5 summarizes the posterior distribution of the coefficients when Italy is omitted. These results show that Italy is influential for the posterior means of the size and concentration coefficients but has little impact on the posterior variances. Thus our posterior uncertainty changes little as a result of deleting Italy, but the range of values over which we are uncertain changes considerably. The new posterior distributions for the reduced data set indicate that Italy was driving up the posterior mean for the size effect but suppressing the posterior mean for concentration. Under Wallerstein's prior, the bulk of the posterior for the size coefficient remains negative, although the magnitude of the posterior mean declines by about a fifth. By contrast, the posterior mean for economic concentration increases by four times when Italy is omitted. Still, uncertainty about the effect remains substantial and the 90% confidence interval still includes zero.

Interestingly, Stephens's prior produces stronger evidence for the effect of size than Wallerstein's prior when Italy is omitted. This can be explained by noting that when there is strong dependency between two variables (size and concentration in this case), prior information for one can result in a sharper estimate for the effects of the other. In effect, an informative prior for the concentration coefficient frees up independent information for the size coefficient. (For a discussion and further examples, see Belsley 1991, 311, 318). This finding illustrates an implication of our discussion of collinearity: when variables are correlated, inference about one coefficient depends on prior information about others (Leamer 1978, 176). Like the size effect under Stephens's prior, the concentration effect remains fairly robust to the exclusion of Italy. Sensitivity analysis on the reduced data set also resembles the full sample analysis, indicating the crucial importance

of prior information for inference about the signs of the size and concentration effects. With diffuse priors in the reduced sample, all posterior size and concentration effects overlap zero.

CONCLUSION

We have tried to do two things. First, we have tried to identify two problems with conventional statistical inference in comparative research: weak and nonstochastic data. Second, we have outlined a Bayesian approach to statistical inference that offers unified and principled solutions to these disparate problems. Regardless of the merits of the Bayesian approach to statistical inference, the problems with conventional frequentist inference in comparative research that we identify will not go away. Conventional inference, like Bayesian inference, is based on a positive theory of how data are generated. This is not commonly recognized. In practice, data analysis also involves the use of extensive prior information. Again, this is not widely acknowledged. A chief concern of ours, then, is to problematize conventional statistical practice in an area for which conventional statistical inference was not particularly designed.

The Bayesian alternative that we propose is one way of addressing the problems of statistical inference in comparative research. It is based on a subjective probability concept that does not rely on data generated by a repeatable mechanism. The theory of data generation behind Bayesian inference is thus more general than the theory of data generation in conventional inference. Sources of uncertainty in the Bayesian approach are not restricted to-but could include-repetitious social processes. The other advantage of the Bayesian approach in the context of comparative research is the formal incorporation of prior information. When sample data are weak, prior information provides a useful supplement to the analysis. Still, the subjective choice of prior is an important weakness of Bayesian practice. The consequences of this weakness can be limited by surveying the sensitivity of conclusions to a broad range of prior beliefs and to subsets of the sample.

To summarize the data analysis, inferences about the negative effect of labor-force size and the positive effect of economic concentration on unionization are sensitive to the priors and the data. Analysis of the full sample showed that evidence for the size effect is consistent with a large set of informative and diffuse priors. Stephens's argument for a positive concentration coefficient, on the other hand, depends decisively on a prior belief in this effect. Italy drives confidence in the size effect and contributes uncertainty to the impact of economic concentration. When Italy is excluded from the analysis, all the results are weakened substantially, and no sign inference about either the concentration or size effect is sustained under the diffuse priors.

On balance, the sample data alone are not sufficiently informative to decide whether Wallerstein or

Stephens is right. A resolution requires more information, which can be supplied through a prior distribution. The plausibility of the results is thus closely linked to the plausibility of the priors. Although this conclusion may appear unsatisfying, we would argue that it demands a lot from 20 data points to resolve a question of broad comparative scope concerning a complex process of institution building. More optimistically, if we allow information like Stephens's qualitative historical account of the impact of economic concentration on early Swedish unionization, a stronger set of results can be obtained. Thus, the data analysis underlines the importance of case studies and comparative histories to assist in the interpretation of quantitative evidence. While this is not news for comparative researchers, the Bayesian approach that we describe permits the transparent incorporation of this supplementary and influential information.

APPENDIX: BAYESIAN REGRESSION DIAGNOSTICS

Following the notation in the text, where s^2 is the sample estimate of the residual variance, Pettit and Smith's (1986) Bayesian influence statistic, I_i , expressing the influence of the ith observation on the joint posterior distribution of the regression coefficients, is

$$I_i = \frac{u_i}{2(1-u_i)} \left[u_i + \frac{e_i^2(2-u_i)}{s^2(1-u_i)} \right],$$

where $u = \text{diag}[s^{-2}(XV^*X')]$, and e is a vector of posterior residuals (the difference between the observed response and the fitted values from the observed data and posterior regression coefficients).

The *I*-statistic has parallels with conventional diagnostics. The matrix from which *u* is extracted can be understood as a "posterior hat-matrix" with the leverages of the sample observations on the posterior parameters along the main diagonal (Pettit 1985, 189; for discussion of leverage and the hat-matrix, see Cook and Weisberg 1982, 11–22). The scaled squared residuals in the expression for *I* can be thought of as squared "posterior studentized residuals" (Pettit and Smith 1986, 485).

S-Plus and GAUSS routines to calculate posterior regression parameters, confidence intervals, and Pettit and Smith's influence statistic are available on request. Press 1989 contains a useful software review.

Note

We thank David Gow for his comments on an earlier draft, and Richard Berk and Larry Bartels for their (seemingly unrelated) influences on our thinking about statistical inference. Chris Achen, Neal Beck, David Epstein, Don Green, Gary King, Sharyn O'Hallorhan, Marco Steenbergen and seminar participants at the University of California, Los Angeles, Department of Sociology provided helpful comments and criticisms. Earlier versions of this paper were presented at the annual

meetings of the Midwest Political Science Association, Chicago, 1993 and the Tenth Annual Political Methodology Conference, Tallahassee, 1993.

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CONTROVERSY

PUBLIC SPHERE, POSTMODERNISM AND POLEMIC

heories of the public sphere, as standardly formulated, aim to specify the minimal, necessary conditions for a discursive realm free of coercion or manipulation. In his article in this Review in September 1992, Dana Villa urged us to reconsider this standard account. He argued that when read in light of postmodernist theory, Hannah Arendt provides the basis for a revised conception of the public sphere that privileges plurality and difference over consensus. Jim Johnson suggests that Villa's analysis is a thinly veiled polemic against critical theory. Johnson argues that, as critique, Villa's argument is neither decisive nor encompassing, and that as polemic it blinds Villa to potentially fruitful disagreements with critical theorists. Villa replies that Johnson misses the synthetic thrust of the original article because he identified public realm theory too narrowly with Habermas. Thus, he misconstrues the dialogue Villa sought to facilitate between Arendt and postmodernism.

COMMENT

There are the sterilizing effects: Has anyone ever seen a new idea come out of a polemic?—Michel Foucault, Polemics, Politics, and Problemizations

In his article "Postmodernism and the Public Sphere," Dana Villa (1992a) disputes the way political theorists currently conceptualize the public sphere. In particular, he objects to those who identify Hannah Arendt's rendering of the public sphere as a model for "a dialogical, consensus based politics" (see Villa 1992b, 274). Jürgen Habermas is, if not the only, certainly the main culprit here. His "polemical stance," according to Villa, both exaggerates the opposition between critical theory and postmodernism and fosters common misperceptions of Arendt's relation to these competing theoretical projects (p. 713). Villa wants to rectify these misconceptions. He aims to answer Habermas' polemic and thereby to prompt us to reconsider the relation between postmodernism and Arendt's defense of publicity (pp. 713, 717). His strategy has three parts. First, Villa advances a set of postmodern objections intended to discredit Habermas' normative ideal of the public sphere. Second, he seeks to show how Arendt's defense of the public sphere—interpreted as the locus of "a politics that stresses plurality, difference, spontaneity, and initiation against the regularizing apparatus of consensus" (p. 719)—more successfully meets these objections. Third, on that basis, he seeks to establish, albeit with certain important reservations, a set of affinities between Arendt's work and postmodern theory.2

In short, Villa aims to deploy postmodern skepticism against the rationalist modernism embraced by Habermas. Then, having discredited critical theory, he seeks to distance himself from what he takes to be the unattractive positions of his erstwhile postmodern allies. In the course of his argument, however, Villa himself resorts to polemic. The most obvious

cost of his polemical stance is that it blinds him to possibilities for an enlarged and enhanced debate, one that might lead beyond the present impasse between postmodernism and critical theory. I try to convey the extent of this cost by indicating, without fully elaborating, several points of engagement that Villa overlooks. I present these as *questions*; the problem is not that Villa fails to answer these questions but that his polemic prevents him from raising them (or similar ones) in the first place.

The issue here is not whether one or another side gets Arendt "right." Villa himself concedes that Habermas captures a central theme of Arendt's thought (p. 713). And her work is clearly rich enough to sustain more than one defensible reading. So my purpose is not to challenge Villa's interpretation of Arendt. Similarly, the issue is not whether Habermas' portrait of the public sphere (or the larger theoretical project of which it is a part) is above criticism. It surely is not. And my intention is not uncritically to endorse his project. The issue, rather, can be captured in three related propositions: (1) the postmodern arguments that Villa advances against critical theory are not as decisive as he seems to think; consequently, (2) the range of theoretical alternatives that emerge from Villa's proposed rethinking of Arendt's relation to postmodern theory is unjustifiably truncated; and (3) (perhaps most importantly) polemic is not a fruitful mode for conducting theoretical disagreements. I shall elaborate on each of these propositions in turn.

RESPONSES TO POSTMODERN OBJECTIONS

Villa advances what he considers to be "the three most basic and forceful" postmodern objections to the normative ideal of the public sphere as a site of unconstrained political debate and deliberation (p. 712). The *power objection* challenges the ideal of a public sphere as the locus of a dialogical politics. He

disparages this ideal for "its naive reliance upon conditions of symmetry, nonhierarchy, and reciprocity as adequate guarantees of a 'coercion-free' space" (p. 715). He contends that it neglects the operation of what Foucault calls disciplinary power, which, because it functions through insidious, continuous, ubiquitous mechanisms of surveillance and normalization, demonstrates that "hierarchy and asymmetry (the two primary villains according to public realm theory) are not essential" to the operation of power in modern society (p. 715). Consequently, the normative ideal of a public sphere is a radically inappropriate remedy for disciplinary power.

The epistemological objection contends that the ideal of a public sphere is "parasitic on an Enlightenment metanarrative of emancipation" (p. 716). As such it is at odds with the defining feature of the "postmodern age," namely, a widespread "incredulity toward metanarratives" generally and toward that of political emancipation in particular (Lyotard 1984, 3, xxiv). From this perspective, Habermas' "attempt to delineate the public sphere in terms of a specific form of interaction and the rationality appropriate to it" appears especially suspect. This is because it threatens to regularize—and thereby suppress—not only "the irreducible heterogeneity of language games" but the plurality of voices that might participate in such games (p. 716).

Villa thinks that the third, ontological objection, is "in some ways the most telling" criticism that postmodern theorists level at the ideal of a public sphere (p. 712). It questions "the specific reality claimed for the public sphere and the objects within it" (pp. 712, 716). And, while Villa does not develop this theme, he intimates that it highlights the dubious (because abstract and procedural) characterization that Habermas gives to the public sphere.

I will now suggest how critical theorists might respond to these postmodern objections. In each case, they confront pressing and difficult issues. But in no instance are the objections Villa raises to critical theory as decisive as he would have us believe. Thus they provide no clear warrant for preferring Arendt's rendering of the public sphere (as Villa reinterprets it) to the normative ideal that critical theorists advance. I do not present these responses as decisive refutations. I simply wish to indicate that despite Villa's conclusive tone, room exists for potentially productive disagreement on the issues that he raises. Again, my aim is to highlight missed opportunities—to raise questions, not to provide definitive answers.

Power

Habermas (1989a) locates the rise of the bourgeois public sphere in the eighteenth century. Foucault locates the perfection of disciplinary power during the same historical period. He argues that "the development and generalization of disciplinary mechanisms constituted the other, dark side" of the processes that Habermas traces. More strongly, Foucault asserts that "those systems of micro-power that are

essentially nonegalitarian and asymmetrical that we call the disciplines" provided the necessary, unavoidable "foundation" for the emergence of a liberal democratic politics. And he goes on to explain that disciplinary institutions "have the precise role of introducing insuperable asymmetries and excluding reciprocities" (Foucault 1979, 222 emphasis mine). His most telling example of the asymmetrical, hierarchical, nonreciprocal nature of disciplinary power is Jeremy Bentham's panopticon. In this quintessential disciplinary institution, "each individual, in his place, is securely confined to a cell from which he is seen from the front by a supervisor; but the side walls prevent him from coming into contact with his companions. He is seen but he does not see; he is the object of information, never a subject of communication" (p. 200 emphasis mine).

These passages are neither obscure nor easy to reconcile with the conventional postmodern interpretation of Foucault that Villa accepts without hesitation. They also are difficult to reconcile with the categorical assertion Villa makes on the basis of that interpretation, namely, that hierarchy and asymmetry are not essential to the functioning of disciplinary power (p. 715). The issue here, once again, is not simply who gets Foucault "right." The issue, rather, is whether Villa's objection to the normative ideal of a public sphere is well specified. If, as the quoted passages suggest, hierarchical, asymmetric, nonreciprocal relations are central to even disciplinary forms of power, the project of specifying an idealized model of unconstrained political discourse, though difficult in ways that I shall indicate, is hardly as ill advised as Villa makes out.4

A less conventional and, I would argue, less polemical interpretation of Foucault opens possibilities for debate. For instance, if the perfection of disciplinary institutions represents the "dark side" of political modernity, do such institutions, as Foucault seems to suggest, find their inescapable normative counterpoint in claims to reciprocal, symmetric and equal communication? Conversely, how, if at all, could critical theorists articulate the possibility that uncoerced public communication might, as Habermas intimates (1992, 74–75), be extended from overseeing familiar political and economic institutions (e.g., state and market) to interrogating other decentralized, but nonetheless systematic operations of social power (e.g., social norms)?

Epistemology

As Villa reports, Lyotard "famously defines" postmodernity as consisting in a thoroughgoing suspicion toward modernist metanarratives generally and toward the metanarrative of political emancipation in particular (p. 716, emphasis mine). But this definition of the postmodern age seemingly neglects a quite plausible yet disconfirming interpretation of the most momentous political events of our time. As Habermas points out, the revolutions of 1989 in East Cen-

tral Europe were hardly made under the banner of postmodern skepticism (1990a, 7–8). They drew upon the well known repertoire of modern political protest. And they were animated—if not exclusively, at least conspicuously—by key ingredients in the modern narrative, namely the legitimating ideas of truth, justice, liberty, and popular sovereignty. Any account of so complex a set of events is necessarily incomplete, hence contestable. Postmodern theorists will no doubt advance rival interpretations. But is it not at least arguable that, as Habermas suggests, modernity is "extending its borders" rather than retreating in the face of postmodern skepticism?

Empirical evidence, however desperately needed in disagreements of this sort, is unlikely to resolve this question. But it surely provides warrant for questioning whether Lyotard's *definition* of the postmodern condition by itself embarrasses efforts to articulate a normative ideal of a public sphere. As events suggest, postmodernists seem greatly to exaggerate the demise of what they call the modern metanarrative of emancipation. What, then, prevents critical theorists from drawing upon that narrative, or its components, to ground criticisms of the homogenizing, exclusionary potential that postmodern theorists hold is inherent in any vision of a democratic public sphere?¹⁰

Ontology

Villa himself concedes that this third objection is less relevant to Habermas than to Arendt (p. 716). But he complains that "Habermas is able to evade it" only by grounding his defense of publicity in "an abstractly procedural account of discursive rationality" (p. 713). Villa resists this "evasion" without explicitly stating why. Yet he nowhere asks whether critical theorists are necessarily committed to Habermas' quite strong formulation of communicative reason (McCarthy 1992, 3, 181–99).

Critical theorists surely aspire to ground their ideal of the public sphere in a notion of communicative reason. But do they need to understand this as a regulative idea, something toward which practices in fact aim? Might they not understand it, more modestly, as a critical ideal—a counterfactual vantage point from which to assess and challenge extant political practices and institutions? Foucault for example, countenances the latter possibility. He concedes that "the idea of a consensual politics may indeed at a given moment serve either as a regulatory principle or better yet as a critical principle with respect to other political forms," subsequently settling on the second interpretation. Thus the ideal of a consensusbased politics is a critical idea to maintain at all times: first, one asks oneself "what proportion of nonconsensuality is implied in such a power relation, and whether that degree of nonconsensuality is necessary or not, and then one may question every power relation to that extent" (1984, 378-79).11

This defense of the public sphere raises pressing theoretical questions. Can critical theorists articulate the seemingly common normative criteria that both tacitly inform Foucault's critique of disciplinary power and explicitly undergird their own defense of the public sphere? Can they specify more persuasively the individual competences and social conditions necessary to make understanding, if still improbable, at least conceivable (Johnson 1991, 1993)? Are there reasons to think that critical theorists cannot address such questions? Villa surely does not offer any.

In the end, then, Villa's objections to critical theory are contestable in ways I have indicated. Given his confident tone, this conclusion is disturbing in itself. But the sorts of question I have raised highlight a further, especially baleful consequence of his argument. Villa unjustifiably circumscribes the range of viable theoretical alternatives precisely because he never even entertains the possibility that critical theorists might advance a more supple, modernist defense of the public sphere.

NIETZSCHE OR ARISTOTLE?

MacIntyre (1981) posed this dilemma more than a decade ago. Villa concludes that we face an updated but similarly glaring antimodernist predicament: postmodern skepticism *or* premodern nostalgia (p. 719). Simply because he arrives via an alternate route neither makes this choice appealing nor renders it inevitable. Indeed, it is ironic to find Villa calling on postmodern theory in order to recapitulate existing dichotomies. This, however, is what his proposed rethinking of the relation between postmodernism and the public sphere leads him to do.

There is one condition under which the impoverished range of theoretical alternatives that Villa offers is justifiable. Neither critical theorists nor others must be able to advance a cogent modernist defense of the public sphere, where one minimal criterion of cogency is that any such defense address postmodernist concerns. Does this condition hold? Villa never asks. Instead, he actively suppresses opportunities to actually engage critical theory and thereby to enhance rather than stifle theoretical discussion. ¹⁴ I have already implicitly identified several such missed chances. Here I will very briefly suggest two more.

First, Villa might push critical theorists to develop the *remarkable* rather than the predictable themes in Habermas' essay on Arendt. ¹⁵ I refer to Habermas' firm reminder that "strategic action has taken place also within the city walls" and that therefore any adequate conception of politics must incorporate a robust, nonresidual role for strategic interaction (Habermas 1983, 180, 183). Might critical theorists take this observation as an opening from which to explore how (or whether) to address what postmodern theorists call the "agonistic" dimensions of social and political interaction? ¹⁶

Second, Villa might push critical theorists to confront an apparent, especially relevant tension, internal to Habermas' theory of communicative action. On the one hand, Habermas presumes that actors are

communicatively competent—that they not only can, but necessarily do, embed their utterances in a system of contestable validity claims. On the other hand, he also insists that actors are endowed with "linguistic competence" and that this mastery of the grammatical rules of a natural language grounds a capacity not simply to formulate "comprehensible" utterances but to do so in innovative ways and under novel circumstances (Habermas 1979, 20, 26–27). Does this tension between constraint and creativity provide an opening from which critical theorists could more adequately address postmodern qualms about the potential homogenizing effects of communicative reason?

These are hard questions. Critical theorists need to pursue answers to this sort of question in order plausibly to defend the normative ideal of the public sphere and thereby avoid Villa's stark antimodernist predicament. Villa offers no reason to think that they cannot do so. Indeed, because he is engaged in a polemic Villa never even acknowledges the opening these sorts of question provide. Consequently, despite his professed concern for the exaggerated sense of opposition between postmodernism and critical theory, Villa's argument has the effect of exacerbating rather than easing existing theoretical divisions.

POLEMIC: HOW NOT TO PURSUE POLITICAL THEORY

Villa complains that Habermas adopts a "polemical stance" toward postmodern theorists. His complaint seems warranted. Habermas, who is well known for openly engaging theoretical rivals and critics, is perhaps uncharacteristically harsh when he discusses postmodern theory. ¹⁷ Villa's complaint also is ironic, for it tacitly invokes an ideal of free and open debate that Villa subverts in his own practice.

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Consider in this respect the "essential" distinction Foucault (1984, 381–82) draws between entering discussion and engaging in polemic. Discussion is "a game that is at once pleasant and difficult." In it participants implicitly undertake commitments entailed by "the acceptance of dialogue" and avail themselves only of rights that "are in some sense immanent" in "the dialogue situation" itself. In polemic, by contrast, the aim is not simply "to wage war" but to regard "that struggle as just undertaking." The polemicist proceeds unconstrained by mutual rights and commitments, and treats each interlocutor not as "a partner in the search for truth, but an adversary, an enemy who is wrong."

Villa is waging a just war in Foucault's sense. To that end he deploys an arsenal of polemical weapons. He offers as conclusive readings of texts that, however conventional they may be, can at best be held tentatively. He argues by definition. He presents as inevitable a theoretical predicament that in actuality is an artifact of his own argument. And even if we grant that Habermas adopts a polemical stance

toward postmodern theory, Villa never acknowledges—let alone addresses—other critical theorists who adopt less contentious approaches. 18 By proceeding in this way Villa encounters performative contradiction: he endorses postmodern impertinence, parody, and skepticism against a suppressed normative background that alone gives credence to his complaints about polemic. But more importantly, the rigors of the campaign blind Villa to the possibility of the sort of principled and productive discussion that Foucault professed to enjoy. 19 His polemical stance prevents him from asking-let alone resolving—the sorts of question I have raised in this comment. With the possible exception of Arendt, Villa leaves all parties in their original places. The primary difference is that their positions are now entrenched, rigidified, less susceptible to reformulation. By his example, then, Villa also regrettably answers no to the question Foucault poses in my epigram. Critical theorists, postmodernists, and others should take note.

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RESPONSE

Toward the end of his response to my essay on public realm theory and postmodernism, James Johnson complains that "with the possible exception of Arendt, Villa leaves all parties in their original places." This is, for reasons I shall detail below, a revealing and remarkable claim, one that implicitly dismisses the public realm theory of Hannah Arendt and endows Habermas' theory with an unquestionable hegemonic status. Johnson finds no "new ideas" in my essay because he identifies public realm theory solely with Habermasian theory. Hence, he misses the synthetic thrust of my essay, which attempts to overcome the hardened opposition between public realm theory and postmodernism specifically by opting for Arendt's theorization of an agonistic public sphere over Habermasian proceduralism.

Let us look, first, at the charges. Johnson accuses me of eschewing productive discussion in favor of polemic, a choice that dooms any attempt to rethink positively the map of contemporary theory. According to Johnson, my polemical intent guarantees that no genuinely new configuration between modern public realm theorists and their postmodern opponents will appear; rather, old battle lines will simply be redrawn, old oppositions sharpened, and the potential for fruitful dialogue dismissed. Johnson asserts that the reader of my essay who expects an aufhebung of the modern-postmodern debate (or, at the very least, a step forward) will be sorely disappointed. Instead of overcoming the baneful either/ors of contemporary theory, Johnson thinks my essay merely shifts them, concluding with a dichotomy

(Nietzsche or Aristotle) every bit as exhaustive and undialectical as the Habermasian one I attack.

What is interesting about this charge is the way it surreptiously transfers Habermas-size ambitions to me, such as the desire to provide an exhaustive mapping of the world of contemporary theory. This desire clearly animates The Philosophical Discourse of Modernity, with the binary of "subject-centered reason" and its critics on the one hand and the "paradigm of mutual understanding" on the other. Johnson agrees with me that the thick black line that Habermas draws between the two paradigms is reductionist but thinks that I am trying to refute Habermas simply by drawing another, equally reductionist, boundary. Johnson sees such boundary drawing as perpetuating a theoretical Punch and Judy show. He wants us to put down our paint brushes and get on with the serious business of working out the kinks of critical theory in general and public realm theory in particular.

My essay does not substitute my global map of theory of Habermas'; rather, it questions the adequacy of Habermas' mapping and suggests that the rigidity of his dichotomy has underscored a largely false opposition between public realm theory and postmodernism/poststucturalism. As I point out, it is to the discredit of many postmodern theorists that they largely accept this opposition as fact. My essay suggests that this opposition has been reified; that the politics of public realm theory are more complex, ambiguous, and subtle than many Habermasians or postmoderns assume; and that the unique public realm theory of Hannah Arendt opens channels of communication between public realm theory and postmodernism in a way foreclosed by Habermas.

My goal in presenting an overview of public realm theory as construed by the Habermasians and various postmodern objections to such theory was not to resolve this difference but to show the way the terms of the debate had been set. The strategy of the essay is first to focus on the opposition and then to show how Arendt's public realm theory straddles it. It is because Arendt theorizes an agonistic public sphere that many of the more typical postmodern objections to public realm theory have little purchase on her. Thus the heart of my essay is found in its distinction between public realm theory as practiced by Habermas and public realm theory as practiced by Arendt (a distinction that has been largely obscured by the Habermasian appropriation of Arendt). With this distinction in mind, new possibilities and configurations become possible between postmodernism and (a more Arendtian conception of) public realm theory—hence my emphasis on how Arendt's theory can answer many of the objections raised by postmodernism/poststructuralism to public realm theory of the Habermasian sort. Of course, there are differences between Arendt and Foucault, Lyotard and others-differences I tried to highlight with my final contrast between Arendt's nostalgia for a public reality and the postmodern affirmation of the blurring of boundaries (a decidedly un-Arendtian ambition).

My essay, then, does not offer up the choice between "postmodern skepticism" and "nostalgia for the premodern" as a substitute for the Habermasian dichotomy between public realm theory and the concerns of the postmoderns. Rather, it suggests an unexplored avenue of dialogue or synthesis, while reminding the reader that Arendt's agonism retains its specificity. In other words, thinking the public realm with Arendt rather than Habermas enables us to overcome many of the roadblocks to the kind of theoretical dialogue or synthesis Johnson thinks so salutary. Why, then, the fuss about polemic and the shutting down of theoretical dialogue? Why does Johnson read my essay as repudiating, rather than furthering, a potential dialogue between postmodernism and public realm theory?

The obvious answer is that Johnson identifies "public realm theory" with the Habermasian project, broadly construed. If one begins with the assumption that Habermas has a monopoly on public realm theory, that he has established a normative paradigm that englobes public realm theory tout court, then it becomes possible to say, with Johnson, that "with the possible exception of Arendt, Villa leaves all parties in their original places." Unfortunately, nothing of the sort takes place. I have, on the contrary, shown that the "abyss" that separates postmodernism and public realm theory is much narrower than supposed. What irks Johnson (and leads him to miss the synthetic character of my essay) is that I do not share his presumption that public realm theory is Habermasian critical theory or that Habermas establishes a theoretical horizon within which all talk of the public sphere must invariably occur. Following Arendt, I think there are other, more fruitful ways of talking about the public sphere—ways that keep plurality, debate, and difference alive rather than seeking to shut them down via a formalistic decision procedure. Because Johnson believes that the only relevant dialogue is between Habermas and his critics, he dismisses the dialogue I am attempting to facilitate between Arendt and postmodernism. Needless to say, I think this stacks the dialogical deck a little too much: there are other, equally important conversations to be had in contemporary theory.

Once we acknowledge that the modern-postmodern debate opens up a number of conversational avenues worth exploring, not just one, Johnson's lengthy list of complaints is put into perspective. Those who, like myself, find postmodern objections to public realm theory à la Habermas persuasive are apt to seek out different, less global, models of the public sphere. Those who, like Johnson, find these objections either unconvincing or answerable may well remain committed to Habermas' discursive model. The conversation one pursues largely depends upon one's reaction to the various criticisms voiced by Foucault, Lyotard, and others. My essay assumes that these criticisms have some bite and that public realm theory would do well to take account of them, specifically by abandoning a strong normative conception of the public sphere for a more flexible,

phenomenological approach. Johnson, given his allegiance to Habermas' discursive paradigm, proposes a different sort of "taking account"—one that continues to treat the discursive model of the public realm as normative. Here we can agree to disagree. What I find peculiar is Johnson's insistence that I write the essay he would have written on the topic of postmodernism and the public sphere. Having failed in this regard, my essay is branded mere polemic.

Johnson accuses me of "waging war" rather than engaging in "the search for truth" because the conversation I wish to promote is not the one he deems important. His deployment of Foucault's comment about polemic is designed as a kind of coup de grace—a damning at the hands of the "master" that places my essay outside the bounds of civilized theoretical discourse. Even Foucault, Johnson triumphantly declares, believed in the Socratic virtues. It may come as a surprise to Johnson, but so do I. However, like Foucault—and, indeed, like Arendt—I am all too aware of how the Socratic virtues can be codified as an exclusionary and silencing discursive rationality. Johnson's "nonpolemical" comments end by banishing me from the dialogue of political theory. The irony, I presume, is intended.

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Notes

Thanks to Jack Knight and Stephen White for very helpful comments on earlier drafts.

- 1. Habermas is not Villa's only culprit. Other critical theorists, such as Richard Bernstein and Seyla Benhabib, are also at fault (p. 714). And many postmodernists are complicit insofar as they accept Habermas' interpretation (p. 719, n. 6).
- 2. Specifically, Villa claims that Arendt "avoids the relativist consequences of postmodern pluralism" by appealing to a shared understanding of the world and that she adopts a different "attitude" than do postmodernists to the subversion of "a distinctly political realm" in the modern world (p. 719). These reservations about Arendt's relation to postmodern theory come out more clearly elsewhere (see Villa 1992b).
- 3. Thus, Foucault explains that as it emerged, disciplinary power had to "solve a number of problems" that other forms of power were ill equipped to address: specifically, it had to "master all the forces that are formed from the very constitution of an organized multiplicity; it must neutralize the effects of counter-power that spring from them and which form a resistance to the power that wishes to dominate it: agitations, revolts, spontaneous organizations, coalitions—anything that may establish horizontal conjunctions" (Foucault 1979, 219, emphasis mine). And since, as Foucault insists, "there are no relations of power without resistances" (1980, 142), disciplinary mechanisms must continuously perform this task of introducing asymmetries and subverting reciprocities.
- 4. This is especially so because, as Foucault explicitly claims, disciplinary mechanisms do not exhaust the entire range of modern power relations (1980, 106; 1984, 380).
- 5. Jacques Derrida once explained to an interviewer, "I cannot conceive of a radical critique which would not be ultimately motivated by some sort of affirmation, acknowledged or not" (Kearney 1984, 118). Foucault, despite conventional readings of his work, implicitly seems to exemplify this reminder.
 - 6. This statement might seem miscast to those who see

Lyotard as making a normative rather than an empirical argument. But this objection is difficult to sustain. While Lyotard clearly advances a normative argument, that argument just as clearly presupposes empirical—and hence fallible—claims regarding the nature of the "postmodern world" (1984, 41). Consider, for example, his claim that "the word postmodern . . . designates the state of our culture" and his explicitly offering his views on the altered status of knowledge in our postmodern culture as a "working hypothesis" (Lyotard, 1984, xxiii, 3). Or consider that he himself invokes a litany of Western and Eastern European events as "evidence" that the modern narrative of emancipation has not simply "been forsaken or forgotten, but destroyed, 'liquidated'" (Lyotard 1992, 28–29, 18).

7. Similarly, Western responses to 1989 and its aftermath, where they have not been purely opportunistic, have arguably been animated by modernist impulses.

8. Postmodern theorists will no doubt distrust Habermas' reading of events. Before too quickly dismissing his account, however, they might consider Timothy Garton Ash's first-hand assessment of how distinctly modernist ideas influenced Central European events in 1989 (1990, 105, 154).

9. In Central Europe today, if modernity is besieged, it is by the sort of ethnic hostility, xenophobia, and so on that transforms the "difference" and "otherness" that postmodern theorists celebrate into violent enmity (Tismanaenu 1992, 3, 279–88). Nevertheless, besieged does not mean overwhelmed: Do either postmodern theorists or Arendt have a superior, coherent response to this trend that does not derive its force from the modern narrative?

10. I have in mind here the task of elaborating on the possibility of what, in his recent political writings and interviews, Habermas repeatedly refers to as "autonomous public spheres" (Habermas 1989 b; 67; 1990a, 18; 1992, 183; emphasis mine). Nancy Fraser (1992) has initiated this task in a fair but pointed criticism of Habermas. Villa views her efforts as "quasi-Lyotardian" (p. 720, n. 10). Yet if Fraser draws conclusions about the proliferation of public spheres that post-modern theorists might find congenial, Villa neglects to note that her comments are unmistakably motivated by the modernist agenda of publicity, equality, pluralism, and democratic self-determination. For a similar argument, see Young 1989 on the notion of a "heterogeneous public."

11. This comports with Habermas' view that the idealized conditions of discourse are "counterfactual" and his claims about the limits of social theory (1984, 42; 1992, 60, 92–93, 160–61, 168, 180). In fairness, Villa seems at times to countenance precisely this reading of the public sphere (pp. 712–13); but he never fully articulates his views. Moreover, he offers a partial reading of this passage from Foucault that belies such an interpretation of his position (p. 719, n. 2).

12. This is not quite so easy as critical theorists might initially suppose, especially if they hope to confront squarely postmodern suspicions that communicative reason is irremediably "regularizing." Minimally, it would require that they disentangle a set of issues that Habermas' own language tends to obscure. For example, reaching understanding (according to Habermas, the implicit telos of language use) does not in any straightforward way entail consent (according to Habermas, the mechanism by which speech acts coordinate social interaction). Nor need it issue in consensus or agreement. Shared understandings may, in fact, ground profound disagreement.

13. Villa's predicament is even starker than MacIntyre's insofar as he provides no criteria for determining which political "attitude" one ought to adopt when confronted by the "choice" he offers.

14. This is so even in Villa's lengthy final footnote. Referring to Fraser (1992), he admits—but does not explore—the possibility that our predicament might not be quite so dire as he makes out, that there may be alternatives other than "the purely mournful or subversive attitudes" he acknowledges in the text. But he also asserts without argument that the notion of the public sphere upon which such an alternative rests "ceases to function in an essentially normative capacity" (p.

720, n. 10). This view reflects Villa's failure, noted earlier, to recognize the essentially modernist impulses that inform Fraser's criticism of Habermas.

15. Here, as with Foucault, Villa is ill served by conven-

tional interpretations of texts.

16. See Johnson 1991 on the difficulties Habermas has in treating strategic action. In any case, insofar as postmodern theorists include expressive and aesthetic as well as strategic dimensions in "agonistic" relations, what I depict as an opening is just that—a potential beginning, an invitation. Here, Habermas' cryptic discussion of "dramaturgical action" with its roots in Goffman's (1959) analysis of "the presentation of self" might afford still another point of departure for discussion between postmodernists and critical theorists (Habermas 1984, 90-94). For Lyotard specifically cites Goffman among the sociologists who contribute to our understanding of the agonistic dimensions of social relations (1984, 16-17, n. 60). This conversation might explore such issues as whether (and when) expressive actions really are, as Villa claims (p. 718) and Goffman denies, nonteleological.

17. Habermas views the debate between postmodernists and critical theorists in terms that mirror those that Villa poses. As Habermas remarks: "The contrast between rationalist approaches in Germany and post-structuralist approaches in France is being quite unnecessarily exaggerated at the moment. . . . I can scarcely recognize myself in the caricatured image of my theory that is sometimes drawn in

this dispute" (1990b, 127).

18. For instance, he fails to acknowledge White (1991) and McCarthy (1991); but see Villa 1993. In any case, from reading Villa, one would never guess that Habermas does not fully and unambivalently embrace modernity, that he considers it a precarious achievement and that he is critical of the one-sided (hence incomplete) way in which it has developed.

19. This is not simply an idiosyncratic predilection: "A whole morality is at stake, the morality that concerns the search for truth and the relation to the other" (Foucault 1984, 381). It nevertheless goes without saying that discussion can be frank, pointed, even heated and that it can be enlightening without issuing in agreement.

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FORTHCOMING IN SEPTEMBER, 1994

The following articles, research notes, and controversies have been scheduled for publication in a forthcoming issue.

William James Booth, "On the Idea of the Moral Economy."

Gary W. Cox. "Strategic Voting Equilibria under the Single Non-transferable Vote."

Mary G. Dietz. "The Slow Boring of Hard Boards: Methodical Thinking and the Work of Politics."

Darrell Dobbs. "The Piety of Thought in Plato's Republic, Book I."

James D. Fearon. "Domestic Political Audiences and the Escalation of International Disputes."

Andrew Gelman and Gary King. "Enhancing Democracy through Legislative Redistricting."

Lawrence R. Jacobs and Robert Y. Shapiro. "Issues, Candidate Image, and Priming: The Use of Private Polls in Kennedy's 1960 Presidential Campaign."

Lyn Kathlene. "Power and Influence in State Legislative Policy Making: The Interaction of Gender and Position in Committee Hearing Debates." David D. Laitin. "The Tower of Babel as a Coordination Game: Political Linguistics in Ghana."

James H. Lebovic. "Riding Waves or Making Waves? The Services and the U.S. Defense Budget, 1981–1993."

Edward H. Muller and Mitchell A. Seligson. "Civic Culture and Democracy: The Question of Causal Relationships."

Melissa Å. Orlie. "Thoughtless Assertion and Political Deliberation."

John T. Scott and Vickie B. Sullivan. "Parracide and the Plot of *The Prince*: Cesare Borgia and Machiavelli's Italy."

R. Harrison Wagner. "Peace, War, and the Balance of Power."

Ross E. Burkhart and Michael S. Lewis-Beck. "Comparative Democracy: The Economic Development Thesis." A research note.

Christopher Kenny and Michael McBurnett. "An Individual-Level Multiequation Model of Expenditure Effects in Contested House Elections." A research note.

Helmut Norpoth and Jeffrey A. Segal; William Mishler and Reginald S. Sheehan. "Popular Influence on Supreme Court Decisions." A controversy.

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POLITICAL THEORY

Democracy and Decision: The Pure Theory of Electoral Preference. By Geoffrey Brennan and Loren Lomasky. New York: Cambridge University Press, 1993. 237p. \$44.95.

Geoffrey Brennan has coauthored, with James Buchanan, a seminal work in constitutional political economy, *The Reason of Rules*, and *The Power To Tax*. Loren Lomasky is a philosopher interested in questions of authority and rights. After previously coediting a book, *Politics and Process*, together, they have now jointly written an important new book, *Democracy and Decision*, which is a critique of public choice theory as it applies to the electoral arena.

At first blush, this is the best of times for public choice theory and its political science alter ego, positive political theory. With a Nobel Prize in economics to leading practitioner James Buchanan, public choice gained added legitimacy in economics, while in political science, theorem-laden articles appear in the discipline's leading journals with regularity. In the Twenty-Fifth Anniversary edition of *Public Choice*, Dennis Mueller, author of the leading public choice textbook, opines, "It is difficult to believe that political science will not be taken over by formal modeling of rational actors' behavior in the same way that economics has been. Inductive theorizing invariably loses out to deductive; less formal modeling invariably loses out to more formal modeling" (77[1993]: 146).

But all is not roses for rational choice modelers. In political science, the rise of rational choice modeling has led to a reaction of epic proportions, similar in tone to the antibehavioral movement of the late fifties and early sixties and directly comparable to the (unsuccessful) rearguard fight of older economists against the increased use of mathematics in that profession after World War II. According to Mueller, "While [that] takeover was occurring, complaints could be heard that the new techniques 'were not economics', were being used 'to reinvent the wheel', were 'divorced from reality', and the like" (ibid.): by simply substituting political science for economics in this quote, we obtain the basic argument of the current rational-choice bashing repertoire in political science. All that we need to add is the common accusation that public choice is just a cover for right-wing ideas.

If anti-public-choice sentiment were confined to political scientists of a humanist (or left-wing) persuasion, we might be able to dismiss it as simply old-fogeyism or leftist paranoia; but concern about the lack of realism and lack of explanatory power of rational choice models is widespread and is found even among practitioners of public choice and positive political theory. In particular, there has arisen a concern that public choice models in their purest form (i.e., ones based on a narrow definition of self-interest or a monomaniacal focus on a single goal such as electoral victory) simply miss too many important features of politics. Thus, we have Morris Fiorina titling a 1991 paper "Is Turnout the Paradox That Ate Rational Choice Theory?," while William Niskanen goes even further, suggesting that although public choice has become a recognized field of academic study, "it is not clear that we have much to tell the rest of the world." Niskanen then goes on to note that public choice theorists not only cannot answer such questions as "Why do people vote?" but "have no satisfactory explanations of the major political developments of our lifetime: the massive expansion of government budgets and regulation, the erosion of the economic constitution, and the progressive spread and recent collapse of communism" (Public Choice 77(1993):155).

Democracy and Decision is reflective of this critique of public choice from within the subfield. While critical of public choice theories of voter behavior, it does not in any way reject the standard tools of microeconomics and applied decision theory, and, like Howard Margolis' work on altruism, it offers new models to help account for findings that appear largely inexplicable within a framework of pure self-interest narrowly defined. The central contribution of Democracy and Decision is the working out of a theory of expressive voting, whereby Brennan and Lomasky mean voting behavior that is not connected to an impact on outcome—like cheering for a team while watching them on television, even when you are alone at home.

My economist colleague Ami Glazer, also an exponent of an expressive model of voting, has suggested that "the ballot box is the last place where it is still legal to be a racist." Brennan and Lomasky argue that the ballot box is also a place where it is not particularly costly to be an altruist; that is, they turn the "selfish voter" argument on its head, by pointing out that because a voter is unlikely to be decisive, voters are much freer to vote in a way that makes themselves feel good by enhancing their self-image as ethical, principled beings. The argument is made nontautological by the assertion "All other things being equal, the relative significance of expressive elements increases by a factor equal to the inverse of the probability of being decisive" (p. 24). Thus, Brennan and Lomasky are able to say that homo economicus is generally a reasonable model for decisions about personal consumption but is, nonetheless, a bad model for choices in the public arena. Just as recent work by Donald Wittman (on competition for voters) or Norman Schofield (on cabinet formation) models parties as caring about both ideology and about winning, the Brennan and Lomasky book shows that allowing for expressive forms of voter preference and for multiple goals need not mean rejecting the value of formal modeling or of a rational choice approach, broadly speaking.

Brennan and Lomasky are able to trace both empirical and normative implications of viewing politics in terms of expressive choices. Normatively, they note that preferences revealed at the ballot box will not have the same claims to authenticity in terms of what economists call "demand revelation" as do preferences about private goods expressed through market choice. Empirically, they suggest how the expressive voting model can help account for a wide range of phenomena—from why facts and allegations about the private lives of public figures weigh into voter decision making to why public attitudes toward Social Security hinge more on perceptions of fairness than on the incidence of costs and benefits

across generations.

There is no such thing as the rational choice model of any given phenomena, only a rational choice model. For example, even though the minimal winning coalition model does not work well in most countries in explaining cabinet coalition formation, this is no reason to give up on rational choice models of coalition building. The usefulness of any given model-rational choice or other -must be in terms of the insights it provides us to the particular phenomena we are seeking to understand and explain. Informed by common sense and focused on the substance of electoral politics, Democracy and Decision carefully steers between the Scylla of abstract rationalchoice bashing without the offering of superior alternatives and the Charybdis of confusing degree of technical elegance or mathematical sophistication with likely social science importance.

Whatever your methodological stance, if you are interested in electoral politics, this is a book worth reading. Unlike much positive political theory, sometimes seemingly intentionally written *up* so as to remain incomprehensible to most political scientists, *Democracy and Decision* is lively and well written. Because the ideas in it suggest potentially testable alternative models in so many substantive domains, it is also a great book to give to graduate students looking for thesis topics.

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BERNARD GROFMAN

Hannah Arendt: A Reinterpretation of Her Political Thought. By Margaret Canovan. New York: Cambridge University Press, 1992. 298p. \$54.95.

Arendt, Camus, and Modern Rebellion. By Jeffrey C. Issac. New Haven: Yale University Press, 1992. 320p. \$30.00

Arendt was a theorist of the extraordinary, a philosophical poet of the preternatural: Holocaust and Achilles, concentration camps and ancient spaces of appearance, imperialism and revolutions in search of freedom. In these jarring historical and aesthetic phenomena, she located the massive dangers, as well as the possibilities, in the contemporary human condition. Future politics, in all of its possible senses, would only rise from the ashes of these multiple pasts: the brilliantly human and the hideously inhuman. No other future was an offer.

Arendt's political theory is thus not only intrinsically intriguing but precisely pertinent to many of our concerns and dilemmas. Fully cognizant of this, Canovan and Issac strive hard to unravel her complex theory and connect it to our politics. Yet their distinct readings of Arendt, interesting and suggestive, are too flawed substantively to be fully persuasive. In this sense, neither is cogent as an argument, but Canovan's study is far less compelling than Issac's in its analytic reach and accomplishments.

Long and detailed beyond the call of scholarly duty, Canovan's inquiry is not an argument about Arendt or a reinterpretation of her thought: it is an extended commentary on nearly all of her writings based on her assumption that Arendt has been "little understood" (p. 2). Two further claims buttress this one: that the "proper context" of her corpus is totalitarianism (p. 7) and that her real political theory lies hidden in her unpublished writings. No one, according to the intrepid Ms. Canovan, has really got Arendt right.

That Arendt's political theory is rooted in her encoun-

ter and engagement with the totalitarian Holocaust is not a novel theme. Kateb and many other scholars have not only insisted on this for over a decade, they have also analyzed the nuanced pathways that connect her theory and that terrible event. Canovan's stress on this point is puzzling and disingenuous. Far more troubling, however, is Canovan's doting reliance on Arendt's unpublished papers and her surprisingly uncritical attitude to them.

To Arendt, the private was usually a synonym for fantasy and falsity, a marker for those things not worth revealing to the public. Hence, a scholar who resorts to—and structures a "reinterpretation" (in fact, a commentary) on—Arendt's private, unpublished papers is under some obligation to justify her project. Canovan, however, does not, although she offers the feeble rationale that Arendt's thought is actually "inward-looking" and "esoteric" (pp. 11–12)—contrary to what Arendt herself has said and many interpreters have supposed. No evidence exists to support this outlandish claim, and none is adduced.

Nevertheless, the result of this strategy is ironic: the "insights" that she elicits from the private papers fail to shed any new light on Arendt's political theory. For example, Homer's pivotal role in Arendt's articulation of the nobility of action on which Canovan insists, is plain and patent in the *Human Condition* and in the extant published literature on Arendt. In fact, many of Canovan's breathless, analogous discoveries, based on her reading of the unpublished papers, fall into the familiar category. At best, they signal slight shifts in emphasis and occasionally add clarity, but they do not warrant a radical revision of her thought.

No novel interpretation or substantive reinterpretation of Arendt's political theory is visible in this study. Tracing the "sources" of a range of Arendt's ideas and commenting at length on them is apparently her forté. Canovan is instructive and often illuminating in her discussion of totalitarianism, barbarism and civilization, republicanism, citizenship, human plurality, and the relationship between philosophy and politics, although her tone is frequently cloying and defensive.

This peculiar style of intellectual empathy, in fact, accounts for a number of unusual but untenable claims. In her unlimited passion to "understand," Canovan zealously overreads and misconstrues Arendt's critical reflections. Two morally troubling examples should suffice. First, Canovan resuscitates the hoary canard that Arendt blamed the Jews for the terrible destruction they suffered at the hands of the Nazis (p. 44). (The only interesting question here is why she has revived this accusation.) Canovan asserts that in Arendt's analysis, "the Holocaust could never have happened" if only the European Jews "had a clear grasp of political principles" (p. 55). Not only did Arendt say nothing of the sort, but such a claim would contradict her unceasing theoretical emphasis on the radical contingency of life and politics. No credible textual evidence is produced by Canovan to support either indictment.

No less troubling is her rush to defend Arendt's solicitous analysis of white racist conduct in Africa and characterization of black natives as "savages." Canovan neither objects to, nor avoids, this label in discussing black suffering: on the contrary, she fluently explains it away as a corollary of their natural inferiority, their barbaric "worldlessness" (quoting Arendt). Canovan is not deterred, by the racist undertow in this analysis,

from depicting Arendt as a ranking theorist of universal human rights.

Habitually uncritical commentary of this kind is not an accidental facet of Canovan's study: it has its source in her lack of theoretical savvy—in her unwillingness to fashion a standpoint from which critically to assess Arendt. Canovan casually assumes what she should be explaining in many cases—for instance, that Arendt is an antifoundationalist and an existentialist. In *Hannah Arendt*, Canovan succumbs to Arendt's considerable intellectual powers precisely because of this lacuna in Canovan's scholarly template, and the result is a study that is continually struggling to justify itself.

In contrast, Jeffrey Issac has a rough thesis in mind—that Arendt and Camus are theorists of "rebellious politics" but are rebels of a peculiar kind fitted for our postmodern time, suspicious of absolutes and final answers. Neither is duped by the boundless idealism of the Left, and both are protagonists of limits and moral realism. Hence, they are rebels who offer no "solace" and no lasting solutions. As Issac reads them, these "rebels" are not conspicuously or overly rebellious: they are, in fact, pliant liberals with a taste for the kind of sly pragmatism espoused by "leftists" like Rorty and Walzer.

Not only that, but they are both committed to "the project of democratic empowerment" and universal human rights (p. 242). Camus, possibly, fits this image of the prosaic modernist fairly well, but to cast Arendt into this mold is to stretch credulity. Fecusing primarily on Camus, Issac has fashioned an interpretive net that can accommodate Arendt, but at her expense. To stuff Arendt into the Enlightenment straightfacket is badly to misread her, and Issac implicitly recognizes this: he frequently retreats from his sanguine assessments of Arendt as an American liberal.

But there is a deeper problem with this interpretation—the presumption that Camus and Arendt were intellectual equals. His literary acumen notwithstanding, Camus was hardly "an exemplary theorist" or even a "political theorist" in any precise, substantive sense (see pp. 11–12). An interest in similar themes was the legacy of a shared historical and philosophical milieu, not a sign of intellectual parity. Camus and Arendt were substantively incompatible figures: he was intellectually light, a parasitic man of letters who said a few interesting things; she was an extraordinary, original theorist of the post-Holocaust human condition, an epic thinker who uncovered new trails of thought.

This reductive strategy not only trivializes Arendt, it robs her analysis of its radicalism and reduces it to the level of Camus's trite bourgeois formulas. For instance, Arendt condemned the racism and unwillingness of Zionist leaders to make peace with the Palestinian natives. In a typically Camusian move, Issac equates this critique with Camus's staunch opposition to independence for Algeria. Camus's insolent dismissal of the colonized is a far cry from Arendt's strikingly unbiased judgment against the oppression of the Palestinians by the incoming European Jews.

Walzer's tutalege is very much in evidence in Issac's analysis. Issac's adulation of Camus and praise for his "compelling" reasons for rejecting Algerian claims (p. 205) and Issac's staging of Arendt as a theorist of limits, a pragmatist wary of utopianism, and an enthusiast of "civil society" are variations on the Walzerian passion for lukewarm liberal politics, "connected crit-

ics," and ethnic affiliation. That Arendt can be read selectively in this way is not in dispute. The crucial point, rather, is that this Walzerian reading traduces her brilliance and the unique radicalism of her political theory. For compromising politics of this sort, especially the Camusian variety, Arendt had no sympathy. To say that she "misunderstood his work" is an untenable assertion (see p. 16). On the contrary, Arendt understood precisely the bourgeois temper of Camus's mind.

Still, Issac's study is a provocative, interesting interpretation, even though he speculates well beyond the spacious bounds of Arendt's political theory. Canovan's book, on the other hand, is a profound intellectual disappointment, an immense effort that goes nowhere. Had Canovan attempted to do what, in the final pages of her faltering conclusion, she recognized she should have, she would have produced a commanding text—an interpretation of Arendt based on a "thorough assessment of the kind that cannot be attempted here" (p. 280).

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SHIRAZ DOSSA

The Idea of Democracy. Edited by David Copp, Jean Hampton, and John E. Roemer. New York: Cambridge University Press, 1993. 449p. \$59.95.

This collection consists of five parts. Part 1 concerns the "point" of democracy; parts 2–4 relate democracy to "preferences," "public reason," and "economics"; and part 5 consists of cases studies. Each part contains main papers plus commentaries, with contributors from a number of different disciplines. However, the editors more or less frankly acknowledge that these divisions are procrustean and arbitrary, calling attention to the overlap between discussions in different parts. If anything, the parts correspond to different theoretical approaches, rather than dealing with separate issues. For example, the appropriateness of industrial democracy arises largely as a moral issue in part 1 (Arneson) and largely as an economic issue in part 4 (Bowles and Gintis).

Two implicit foci of debate are consequentialist-versus-nonconsequentialist justifications of democracy and the role of preferences. In relation to the first focus, Estlund considers, in a challenging but rather obscure piece, the idea that if democracy is justified by its consequences and if there are political experts, we should defer to them and so undermine democracy. His preferred response is to question whether such experts could ever be recognized and therefore whether such deference could ever be rational. Copp, in his commentary, argues that a form of government has to be judged not merely on how well it deals with social decisions but also on what sort of system it is. But this, he insists, is not to depart from the terrain of consequentialism. However, he fails to consider a familiar point that does involve such a departure and does create problems. Democracy may be valued not because it fosters autonomy (consequentialist) but because, being a system of self-rule, it necessarily instantiates it (intrinsic). The instantiation of autonomy may then be accompanied by a very poor performance in terms of the outcome of its social decisions. When Copp says, "We value democracy because we think democracy will have the best consequences for society, all things considered, and because of the kind of governance it is" (p. 115), we have

to recognize that these two things may fall apart, forcing us to decide which we regard as more important.

Preferences get discussed in a number of ways. In a general discussion, Hardin rehearses the problems in aggregating preferences recognized by Arrow and motivational difficulties connected with the free-rider problem. He argues that the same assumptions that have done wonders in explicating economic activity have exposed grievous flaws in democratic thought and practice. Christiano argues that there is an intrinsic justification for democratic control of public-collective properties, as opposed to mere preferences, which are not necessarily expressed in the act of voting anyway—thus attempting to short-circuit the Arrow problem. As for the free-rider problem, Hardin does not consider the possibility that while the requisite assumptions may be true of human beings, they may not be all that is true of them: if human beings are not exclusively self-interested agents, it may be that there are other features of their makeup that can explain and justify other forms of behavior than the economic. Sunstein enters a robust case for democratic action against people's preferences if they have resulted from unjust background conditions. He gives a long list of possible instances where state intervention might be justified but leaves matters at the level of moral intuition, rather than going on to provide reasoned justifications for them.

Where actual preferences are in some substantive sense not arrived at autonomously, the so-called paradox of emancipation arises: if you think people express preferences at odds with their own interests but you also wish to abide by democratic procedures, do you allow actual preferences to determine a suboptimal outcome or override their preferences and therefore apparently abandon democratic procedures? That is a question usefully raised by a number of these papers.

Answering it might involve raising further questions about what most of them take for granted—broadly, the fabric of existing, representative democracy. There has been interesting recent work laying down a challenge to this fabric and mooting the possibility of more participatory institutions (e.g., Fishkin's Democracy and Deliberation, 1991), but it is not discussed here. In addition, although the editors refer to the move to democracy in Eastern Europe, there is no indication that anything of theoretical importance can be learnt from that ghastly failed experiment. Roemer, it is true, urges that we should separate out the factors responsible for that failure and not just assume that one-party rule and a command economy and state ownership are equally hopeless; but his paper is concerned with the feasibility of state control of the commanding heights of the economy, rather than with the political issues. So far as democracy is concerned, it is the political component in that failure—the vanguardism with which Leninist doctrine answers the paradox of emancipation—that requires examination. Even more recent events in Eastern Europe should warn us against the thought that once indefensible doctrines have been seen off, they are never heard of again.

The collection, then, contains many interesting discussions but possesses little unity. Rawls's paper, "The Domain of the Political and Overlapping Consensus," for example, is reprinted and elicits an eloquent response from Joshua Cohen, who attempts to show that Rawls's later thinking does not lead to an abandonment of his earlier egalitarianism, as well as a penetrating

critique from Hampton designed to show that defensible liberalism cannot really be neutral. But Rawle's project presupposes a democratic political culture, rather than being centrally concerned with its justification.

Finally, any reviewer should cavil at one aspect of the production of this volume. Where articles have been published before, commentators generally refer to the pagination of the original (and in one case to the pagination of a presumably unavailable conference typescript), rather than that in the present volume. That does not facilitate scholarly enquiry.

University of Bristol

KEITH GRAHAM

The Other Heidegger. By Fred Dallmayr. Ithaca: Cornell University Press, 1993. 232p. \$30.75.

Heidegger's Crisis: Philosophy and Politics in Nazi Germany. By Hans Sluga. Cambridge: Harvard University Press, 1993. 285p. \$29.95.

The common thread connecting these two useful books is the vexed problem of the relation between Heidegger's philosophy and his nazism. Heidegger joined the Nazi party and served as the Nazi rector of the University of Freiburg briefly in 1933–34 before resigning. The relation between his philosophical theory and his political turning is the subject of much debate, particularly since the publication of Victor Farías' book, Heidegger and Nazism, in French in 1987.

Since that time, there has been a steady stream of books in French and English devoted to aspects of the link between Heidegger's theory and his nazism. The present volumes respond to the continuing controversy. În 'The Rectorate 1933/34: Facts and Thoughts''—an article written in 1945 that only appeared in 1983-Heidegger contended that his brush with nazism was essentially meaningless and certainly so for an evaluation of his thought. Taking a similar line, Dallmayr and Sluga both acknowledge Heidegger's nazism but seek to relativize it in a way that leaves the wider theory untouched (or at least relatively so) by his political turning. Dallmayr identifies another Heidegger who is a radical critic of just what he seems to have been identified with, and Sluga shows that Heidegger's comportment was rather typical of what passed for responsible academic behavior during a rather difficult period.

I am impressed by Sluga's careful account of the philosophical background in which Heidegger turned to nazism and Dallmayr's equally careful interpretation of Heideggerian texts. Both of these books are important contributions to the continuing effort to comprehend the link between Heidegger's nazism and his philosophy. Yet I find their respective efforts to explain away (or at least severely to weaken) the importance of Heidegger's link to nazism for a grasp of his thought to be unpersuasive. As a player in the game, I believe that Heidegger's nazism is finally inseparable from his theory. Accordingly, my remarks on specific points in Sluga's and Dallmayr's books are meant to reflect my unease about their respective interpretative strategies.

Sluga's contextualist approach to Heidegger continues a tendency that has recently been featured in publications by Ott, Nolte, Farías, Bernd Martin, Thomas Laugstien, George Leaman, Claudia Schorcht, Rainer Marten, and others. He presents the most detailed view we currently possess of German philosophy in general during this terrible period. Whereas others look to the historical context to reveal the depths of the problem, he relies on a grasp of the philosophical context to show how typical Heidegger was. To make his case, Sluga talks comparatively little about Heidegger but a great deal about the surrounding view of well-known and comparatively obscure German thinkers of the period. Even if few think that talk of the good actually leads to doing the good, the extent to which German philosophers rushed to cooperate with nazism-in effect, to conduct business as usual in a changing environment—is clearly disturbing. Sluga's discussion is fascinating and sobering. He largely succeeds in his effort "to embed the story of Heidegger's personal crisis in an account of the crisis that enveloped German philosophy in general" (pp. viii-ix).

Both Sluga and Dallmayr prefer not to engage alternative ways of reading the link between Heidegger's thought and his nazism. Sluga regards the prior debate as simply factional, moralistic, and individualistic and psychologizing. Bourdieu receives special discussion early in the book for a simplified account of the philosophical context and later on for a supposedly reduction-

sophical context and later on for a supposedly reductionist approach (pp. 9–10, 247–48). Sluga argues in detail that Heidegger was not unique, since many other German philosophers turned to Nazi politics. His accounts of Heyse and Bauch are especially useful. He further innovates in two other ways. One is his careful discussion of Fichte, whose theory was appropriated by the Nazis as part of the intellectual basis of national socialism. The other is his insistance on the Nazi period as a time of crisis and the perceived crisis as a motivating

factor in the widespread philosophical turning to nazism.

The effort to incriminate Fichte as a kind of proto-nazi has been made by others, recently by Glucksmann (*Les maîtres penseurs*), although perhaps never through such detailed comparison. Yet it is unclear that Fichte's situation in 1807 and that of German nazism in the twentieth century is even roughly similar. The view of the Weimar Republic as a time of crisis is prominent in Heidegger's 1929/30 winter lecture series (*Die Grundbegriffe der Metaphysik: Welt, Endlichkeit, Einsamkeit*). Despite Nolte's urging, the fact nazism occurred in a time of crisis seems to me to be insufficient either to excuse the general academic turning to nazism or to excuse or otherwise minimize Heidegger's political turning.

Sluga's Heidegger is too much like the other thinkers of the period. We need to recall that Heidegger was the only philosopher of the first rank to rally to the Nazi cause. Among German philosophers, he was committed unusually early and to an unusual extent to national socialism. Sluga insists (correctly, in my view) that Heidegger was engaged in self-promotion (see p. 173), yet this is an instance of the psychologizing tendency that Sluga rejects. So, too, is the claim that Heidegger's nazi turning is due to his desire for his theory to have practical effects (see p. 231). For Sluga, political ideas reflect political conditions (p. 246). What about philosophical ideas in general? Is this the case for Heidegger? Did Heidegger's theory lead him to nazism?

Dallmayr's book, which consists of a series of essays (some previously published) is rightly seen as part of the current effort by Derrida, Lacoue-Labarthe, Caputo, and others to acknowledge Heidegger's nazism while recovering the still-valid part of the theory. For Dallmayr, the fascist element in Heidegger's thought is "one (easily the most deplorable) strand, but not the only and not even

the dominant one" (p. ix). To put the point bluntly but not inaccurately, breaking with the view of Heidegger as a good philosopher but a bad politician, for Dallmayr, Heidegger is a good philosopher and a good politician as well, although in a theoretical, not a practical, sense.

Dallmayr's discussion is invariably based on sensitive, generous, and occasionally overgenerous interpretation of the full range of Heidegger's writings, with special attention to the later thought. The frequent remarks on the Beiträge zur Philosophie, a still little known but central work of Heidegger's later thought, are particularly useful. Four chapters (roughly half the volume) are devoted to aspects of Heidegger and politics; the remainder ranges widely over such topics as ethics and justice, otherness, friendship, and Nishitani's version of zen Buddhism.

The effort to reveal another possible non-fascist interpretation of Heidegger's theory is plausible, since few commentators, other than Adorno, want to "reduce" Heidegger's theory to his nazi proclivities. Yet Dallmayr, who confesses to being awed by Heidegger (see p. ix), could have strengthened his case for another Heidegger by taking on the views of those who disagree with him. In some instances, such as the *Letter on Humanism*, his readings are vastly undetermined by the facts—as if Dallmayr regarded as obviously true Heidegger's own, unsupported claim that he later turned against national socialism.

The four chapters on Heidegger and politics depict a Heidegger as subversive of the established order, which is perhaps less at odds with his nazism than Dallmayr pretends. He holds that after 1933, Heidegger's corpus reflects a profound struggle against nazism without returning to traditional orthodoxy (see p. 19). But Heidegger did not even resign as rector until the middle of 1934. Dallmayr, who discusses the views of Pöggeler and Derrida, does not bolster his conviction through textual evidence. An important example is the reference to Heidegger's rejection of the nazi race theories, which is routinely used by his defenders to argue that he was not a Nazi. Dallmayr's true remark that Heidegger was unable to support "naturalism" or "biologism", does not justify his surprising conclusion that "the critique of modern ideologies (deriving from metaphysics) cannot possibly entail support for Hitlerism or National Socialism" (p. 47). Many German philosophers, including Heidegger, who were unenthusiastic about (and even clearly opposed to) such doctrines, supported nazism. Although Dallmayr reads the Letter on Humanism as opposing nazism (see p. 63), it can more plausibly be read as an effort at political rehabilitation at a time when Heidegger still held to his nazi convictions. It is correct that Heidegger was opposed to popular sovereignty (p. 104), although one misses an analysis of the Platonic nature of this claim. Dallmayr reads the first series of Hölderlin lectures as moving away from nazism, although he fails to consider the residue of folk (völkische) ideology in this document.

I share Dallmayr's conviction that Heidegger's theory is "suffused with ethical preoccupations" (p. 109). He regards Heidegger's resistance to formalized ethical theory as related to a rejection of nazism as a racial-biological theory (pp. 125, 130). He further regards Heidegger's rejection of "racial-biological chauvinism" as tantamount to the effort to choose an alternative course (p. 152), yet the texts lend insufficient support to this hypothesis. In the same way, Heidegger's traditional German philosophical emphasis on the renewal of

ancient Greece in the first series of Hölderlin lectures (p. 176) can also be read as linking his longing for Hellas with his nazism. The discussion of friendship is devoted to an uncontroversial account of Hebel. The salute to Nishitani emphasizes his concern with Western philosophy but fails to consider the link between his views and the ambiguous link of his teacher, Nishida, to fascism.

Duquesne University

Tom Rockmore

American Democracy: Aspects of Practical Liberalism. By Gottfried Dietze. John Hopkins University. Baltimore: Johns Hopkins University Press, 1993. 290p. \$39.95.

Self-consciously, this book is in the tradition of Tocqueville, Bryce, and Laski. Its author, Gottfried Dietze, an erudite scholar and renowned teacher, is a native of the land of Beethoven, Hegel, and Goethe. He is also a long-time American resident and observer of the American people, especially their politics. Unfortunately, this book is less than we might have hoped.

It is not a closely argued book: the style is soft and remarkably, (sometimes almost randomly) discursive. For example, he writes: "Freedom does not, as Spinoza believed, produce just good. Following the Libson earthquake of 1755, an event that considerably muted the optimism of the time, the dangers of liberty were pointed out by the authors of the Federalist Papers and by Kant, who followed thoughts expressed earlier by Hobbes, Locke, and Blackstone" (p.19). The oddity of this sentence lies not only in the connection that it suggests should be made between a natural event and the political views of a wide array of philosophers, two of whom lived before it occurred. Even more strange is the implication that the authors of the Federalist in some significant way sided with Kant against Spinoza. Or is all this just rhetorical flourish?

Whatever it is, there is a lot of it in this book and mostly with uncertain effect. The author has a powerful tendency to scatter not only a wide array of little-explained references through his pages but also quotations, some extended. The general impression left certainly includes a burnish of imaginative erudition. But it also leaves one wondering once again whether arguments are just being loosely illustrated or specifically and authoritatively documented.

These difficulties carry over into the crucial problem of definitions. Liberalism, although much discussed (documented? illustrated?) is let go essentially as the desire for freedom, built into human nature and as old as man himself. Classical liberalism, a tradition with which the author identifies himself, is the same but restrained by law and principles of morality. And democracy, of which America is said to be the premier example, is passed off more than once as simply government by the people (or, at least, majority rule). The principal effect of this looseness in definition is to render the book's argument imprecise, its chapters unfocused, and even many of its paragraphs poorly structured.

Another effect is make many of the books remarks appear to be not just asides but the opinionated views of an unrepentant "conservative," especially on subjects as diverse as modern art, the morality of American youth and popular culture, and the influence on modern times of Freud, Nietzsche, and Marx. It is important to men-

tion this aspect of the book because of the possibility that other readers, including especially those who share most of Dietze's opinions, might find this book wideranging, insightful, and articulate. Even in the face of that possibility, I found Dietze's central thesis unsatisfying. That thesis, as repeatedly stated, is simply that modern-day Americans, driven by primordial needs, history, and the unrivaled characteristics of their current circumstances, have dangerously strayed far from their classical liberal roots toward a pure liberalism that preaches an absolute freedom to do whatever one wants. We have become a consumer-oriented society in the broadest sense, free to make money, move, change religions, and accumulate both things and experiences just as we please and without limit.

In general terms, this is an unexceptional thesis that few on either the Right or the Left would dispute. But unless pinned down in terms of specific propositions and supported by both carefully sifted data and rigorous analysis, it leaves major questions hanging. In the context of this book and the possible perspectives of its author, two questions are especially significant.

First, for a book whose subtitle is Aspects of Practical Liberalism, is it enough to suggest that the liberalism of America's most practical document, its Constitution, lies in its being such a bundle of compromises that people (and politicians) are free to make of it as they please? Or would it be wiser to see the provisions of that document, (e.g., those establishing the Congress as an independent body) as embodying specific conceptions of the liberal ideology? Putting the matter another way, does our liberal constitution give us a government that can govern? By what means and to what standards? In the Germanic scholarly tradition, there are powerful voices that speak to questions of this order—most importantly, Max Weber's. It is our loss that Dietze did not use his advantages to give us a Weberian analysis of the American constitutional system.

Second, much of Dietze's analysis depends on a Hartzian perspective. Although he knows that perspective through and through, he gives us no critical analysis of it, much less an appraisal of its merits. But Hartz was notably deficient in not giving us much understanding of the content of the liberalism he found consensual in America, beyond saying it was an English liberalism. But is English liberalism different from German? More importantly, are there not German voices (most notably Hegel's in his treatment of civil society in *The Philosophy of Rights*) that can be profitably interpreted as supremely revealing and critical of English (American) liberalism? Again, it is to be regretted that Dietze did not make more use of his advantages.

New York University

H. MARK ROELOFS

Life's Dominion: An Argument about Abortion, Euthanasia, and Individual Freedom. By Ronald Dworkin. New York: Alfred A. Knopf, 1993. 273 p. \$23.00.

That women have a "right to abortion" is a dogma of contemporary liberalism; that people have a "right to die" is well on its way to achieving dogmatic status. One is hardly surprised, therefore, to find Ronald Dworkin, a liberal moralist of the most impeccable orthodoxy, defending these putative rights in his new book on abortion and euthanasia.

What is surprising is Dworkin's claim that the debate about abortion and euthanasia reflects competing "interpretations" of a shared basic commitment. He suggests that those who favor abortion and euthanasia, no less than those who oppose them, believe in the intrinsic value—and, indeed, the sacredness or inviolability—of human life. Debate breaks out because the contending parties "interpret" life's value, sacredness, and inviolability differently.

For example, proponents of "abortion rights," far from denying the sanctity of human life, may, according to Dworkin, reasonably conclude "that abortion is sometimes necessary in order truly to respect life's inherent value, which depends on the intrinsic importance of human creative investment [in it]" (p. 157). For such people, as for proponents of euthanasia, the "life" that is sacred and inviolable is not "biological life" but the "human life [that] is created not just by divine and natural forces but also . . . by personal choice, training, commitment, and decision" (p. 93). They hold that "it may be more frustrating of life's miracle when an adult's ambitions, talents, training, and expectations are wasted because of an unforeseen and unwanted pregnancy than when a fetus dies before any significant investment of that kind has been made" (ibid.).

Dworkin devotes about two-thirds of *Life's Dominion* to a defense of the regime of abortion law put into place by the Supreme Court in *Roe* v. *Wade*. In certain respects, however, his account of the constitutional law of abortion is questionable. Apparently overlooking the implications of *Doe* v. *Bolton*, the companion case handed down with *Roe* on January 22, 1973, he implies that states may meaningfully protect the unborn against abortion in the final trimester of gestation. As Mary Ann Glendon has observed, however, that proposition is subject to the gravest doubt in view of the Court's holding, in *Doe*, that there is a constitutional right to abortion through the third trimester to protect maternal health and its expansive definition of *health* as including all factors, physical, emotional, and even "familial."

Dworkin maintains that the controversy over abortion, which continues to rage 20 years after the Supreme Court purported to settle the matter, "is based on a widespread intellectual confusion we can identify and dispel" (p. 10). He assures us that "once the confusion has been identified, we will see that a responsible legal settlement of the controversy, one that will not insult or demean any group, one that everyone can accept with full self-respect, is indeed available" (pp. 10–11).

What is the alleged confusion? Dworkin maintains that public debate largely fails to recognize a "crucial distinction" between objecting to abortion because it violates the right of unborn human beings not to be killed (the derivative objection) and objecting to abortion "because it disregards and insults the intrinsic value, the sacred character, of any stage or form of human life" (the detached objection) (p. 11). The derivative objection, he maintains, is unsound because human fetuses, nearly until birth, lack all "interests," including "an interest in surviving," and therefore cannot have a right not to be killed (pp. 18-19). The detached objection, by contrast, is not unreasonable. However, it merely provides grounds for "personal opposition" to abortion and, perhaps, efforts to make people who are considering abortions pause to reflect carefully on what is, after all, a serious moral decision; it does not warrant outlawing deliberate feticide.

Dworkin argues that most people who oppose abortion do so on reasonable (i.e., detached) grounds. The problem is that many of them fail to recognize that the detached objection does not justify laws protecting the unborn against abortion. Hence, they wrongly seek to impose their view on others who reasonably interpret the intrinsic and inviolable value of life as permittingand, indeed, sometimes even requiring-abortion. If advocates of the pro-life cause would only recognize their confusion, Dworkin suggests, they could abandon the political struggle while retaining their commitment to the sanctity of life. That, it seems, is the "reasonable settlement" that Dworkin believes "all parties" can accept while retaining their self-respect. In effect, Dworkin offers the prolife movement what he takes to be honorable terms of surrender.

Anticipating a possible objection, Dworkin considers the fact that our law protects many nonhuman realities whose destruction or violation we object to on detached grounds. We protect the environment, for example, or endangered species not on the ground that they have rights but, rather, because we consider them inherently valuable. It is here that his redescription of the proabortion and proeuthanasia positions as expressing a competing "interpretation" of a shared basic commitment to the sanctity of life has its payoff: it enables Dworkin to cast the debate about abortion and euthanasia as essentially "religious" in character, that is, as a debate about "the meaning of life." Hence, according to Dworkin, the First Amendment's protection of the free exercise of religion forbids government from prohibiting what he bluntly concedes is the book's opening sentence are forms of "deliberate killing" and "choices for death" (p. 3).

of "deliberate killing" and "choices for death" (p. 3).

The success of Dworkin's argument hinges on his distinction between detached and derivative objections to these death-dealing choices and on his claim that derivative objections to the killing of unborn and seriously demented human beings are unreasonable. These, in turn, depend on the validity of the person-body dualism implicit in Dworkin's argument.

Opponents of abortion and euthanasia reject the proposition that some human bodies are "inhabited" by persons, while others, such as bodies of unborn and gravely impaired human beings, are not. They do not believe that incorporeal persons inhabit nonpersonal bodies. Thus, they view all human beings, including the unborn and the demented, as members of the community of subjects to whom duties in justice are owed—that is to say, as *persons*, or (if you will), "rights-bearers."

Dworkin's case against the view actually held by sophisticated supporters of the pro-life position strikes me as woefully inadequate. His argument that fetuses cannot have "interests" and therefore have no right not to be killed, straightforwardly begs the question; and he makes no effort to respond to the critique of personbody dualism pressed against his position by, among others, his fellow Oxford professor of legal philosophy, John Finnis.

Moreover, his talk about the contending parties in the debates over abortion and euthanasia sharing a basic commitment to the sanctity of life and only interpreting this commitment differently, is, I believe, practically insignificant. The real issue of principle between supporters of abortion and euthanasia and opponents of these forms of killing has to do with the question of who are subjects of justice.

The challenge to the orthodox liberal view of abortion

and euthanasia is to identify nonarbitrary grounds for holding that the unborn and seriously demented do not qualify as subjects of justice. Frankly, I doubt that this challenge can be met. In any event, Dworkin here fails to make much progress toward meeting it.

Princeton University

ROBERT P. GEORGE

A New Constitutionalism: Designing Political Institutions for a Good Society. Edited by Stephen L. Elkin and Karol Edward Soltan. Chicago: University of Chicago Press, 1993. 240p. \$45.00 cloth, \$15.95 paper.

The essayists in this collection believe "that something systematic can be said about how political institutions work and can be made to work ... and why some regimes are better than others' (p. viii). In the current intellectual climate, this seems an almost reckless statement, since postmodern analysis has quite powerfully suggested how tightly bound all political communities are to their particular and contingent rhetorical conditions. The subtitle might lead one to expect a comparative analysis of constitutional regimes rigorous enough to combat postmodernism's claim and to show that political research can escape the gravitational pull of particular cultures, find the philosopher's stone by which to show the superiority of one system's political values over another's, and thereby demonstrate the political correctness of particular constitutional designs.

There is, in fact, no serious cross-national analysis here, nor do we find a new constitutionalism through an analysis of different ways of constituting complex political regimes in general (e.g., Minnesota-Louisiana, University of Chicago-University of Georgia, IBM-Microsoft). Instead, readers will find here another critique (warranted, in my judgment) of contemporary political science. Readers will confront exhortations to abandon analyzing bits of political data, rhetorical styles, and analytic philosophy and, instead, to get on with the task of designing good national regimes.

These pages do contain many important ideas. Much postmodern thought does encourage the "hermeneutics of suspicion," and that attitude may plausibly corrode government. An ancient version of the same corroded the Roman Empire, according to Gibbon. Karol Soltan urges that designing good government means designing better citizen education to achieve greater sophistication about politics. Borrowing on Chomsky, Soltan wants the new constitutionalism to describe standards by which the ideal citizen could become an effective political speaker-hearer. Stephen Elkin draws from Kenneth Arrow the goal of designing a state that achieves the "proper aggregation of voter preferences" (p. 29). James Ceaser argues that rational choice analysis cannot respond effectively to value-laden cries for justice. Charles Anderson, restating the main themes of his book, shows pragmatism's compatibility with liberal values and goals. Theodore Lowi and Cass Sunstein deliver their familiar pitches elegantly: Lowi decries the delegation of legislative power to the executive, while Sunstein, less polemical than Lowi, points out that the founders's original hope that national government would play the role of public-spirited deliberation survives in the role played by the judiciary.

The book thus presents a theme: We must develop confidence in our capacity to define problems and their solutions through conversations that are "practical" and "conditionally normative" (p. 133). But experiencing this worthy theme-and-variations finally resembles nothing so much as the familiar pattern into which romantic relationships are said to fall. One begins with hope for something new and challenging and in due course finds oneself in the same, quite-familiar place with the same familiar comforts and the same unresolved tensions and unmet yearnings. Thus, in Edwin Haefele's concluding essay, we learn that the American republic is constituted by self-government, civic virtue, a regulated private economy, and separation of church and state.

Hence, the constitutionalism proposed here is hardly new. The authors urge us to return to the thrilling days of yesteryear, when people actually could talk confidently about designing good constitutions for good societies: it is a romantic book. In its romantic yearning for a kind of perfection, it does not convincingly demonstrate the existence of a constitutional design problem serious enough to call for major changes in the first place. In comparison with other regimes, our design does seem to have avoided police states. Our political system has not drastically limited human mobility. We are reasonably free of social-caste distinctions. The political system seems not to obstruct adapting business practices to the new global economy. Of course, public education is a mess, but the authors do not explain why we need a new constitutional design to clean that mess up

It would also have helped if the collection somewhere addressed more aggressively the assumption that academic theorizing of this sort can ever translate into major political change. Civil wars and economic depressions may redesign constitutions, and in these crisis times, great thinkers help redesign, but this book does not coincide with a crisis.

Still, two things should be said on this book's behalf. First, Justice Holmes was quite right to point out that we need reeducation in the obvious more than elucidation of the obscure. Political scientists (because they are citizens, too) do have a responsibility for seeking knowledge that may better our political conditions. Precisely because postmodernism's criticisms are on target, conventional positivist/empirical political science must take stock of its political obligations. There is no justification for what we do beyond the expectation that we will contribute to betterment of life on the planet, and the authors rightly force us to examine whether our practices do so.

Second and more important, political change requires rhetorical change. This may be the subtextual agenda of the Committee on the Political Economy of the Good Society, from which these essays flow. Unfortunately, these essays admittedly do not offer the one thing rhetorical change requires, namely, a coherent doctrine of what the new constitutionalism should look like (p. 13). But by trying—and failing—to articulate a coherent theory of constitutional design among the classical and modern theories of political economy, the book helps invigorate the postmodern hope that truly new constitutional theories are worth creating. And these theories will (as this collection implies) address how we talk.

University of Georgia

LEIF H. CARTER

The Dialogue of Justice: Toward a Self-Reflective Society. By James S. Fishkin, New Haven: Yale University Press, 1992. 243p. \$27.50.

In *The Dialogue of Justice*, James Fishkin has produced a readable and stimulating approach to liberalism. Metalevel engagement with a range of contemporary liberal theorists leads up to a reworking and development of something close to the "dialogic" themes in Mill's *On Liberty* and an exploration of liberal society as "self-reflexive."

The book has three main parts. First, Fishkin argues that attempts at a systematic theory of the basis of a liberal political regime fail, because plausible ideas about what constitutes a just society pull irresolvably in conflicting directions. Second, he advances various plausible criteria, in light of which he criticizes other substantive viewpoints. Approaches to liberalism (1) must discriminate but not rule out everything, (2) should legitimize the state to all but not obligate citizens of other states, (3) should not provide other theories with a basis upon which to develop incompatible prescriptions, and (4) should not manipulate people's preferences. These ideas are brought alive in the critical discussion of other approaches, leading Fishkin to develop some useful typologies-for example, "brute," versus refined views of individuals' preferences, actual versus hypothetical situations, and neutrality versus universality. Fishkin's approach here is both interesting and powerful and displays the immense fruitfulness of general and metalevel argument, as contrasted with the pursuit of detailed points internal to the work of specific writers.

In the end, Fishkin finds the combination of these criteria too strong for any substantive theory. He then develops his own approach, by way of an exploration of the idea that preferences should not be coercively molded. He argues that political practices can justifiably claim support from all members of a society if (1) they are consensual in the sense of having wide support across major social cleavages, (2) are minimally voluntary (i.e., there is unimpeded exit, (3) essential benefits are supplied to all members of society, and (4) the society's practices are self-reflective, that is, subject to "continuing critical examination through unmediated debate" (p.). Fishkin develops these ideas in interesting ways, concluding with some practical suggestions about political institutions. Rather than paraphrasing his argument, which deserves to be read in detail, I shall move straight to some criticism.

First, nonmanipulation introduces more problems than Fishkin seems to allow for (see, e.g., problems raised in the community power debate and by the work of Foucault). Second, Fishkin appeals implicitly to dialogue-based ideas about epistemology. This is contentious both qua epistemology and also because it is incompatible with the implicit epistemological ideas of some of those who would be involved in his dialogue (e.g., fundamentalists). It is also not clear to what extent substantive ideas about benefits are being drawn from the epistemology as prerequisites to citizens' participation or are introduced extraneously. The latter would obviously be problematic; the former, to work, would depend on Fishkin's being able to argue that everyone can really make a potentially useful input to the argument.

Third, what one might call empowerment for the sake of participation is sometimes developed in ways that look more like the enhancement of pluralistic interest articulation, but this is by no means the same thing as dialogue. Dialogue itself introduces particular problems: it is not clear that people will be interested in listening to the arguments of others. Even today, serious discussion of political issues seems all too often to fall victim to the television remote-control zapper.

Fourth, the right of exit introduces a problem and highlights an ambiguity concerning the relation of dialogue and political community. Fishkin is working with the nation–state, but it is not clear why dialogue should generate or respect national boundaries. Why, say, should the views of those over the Mexican border (or in Australia) not count as much as those on this side? Is there a right of *entry*? Could the rich all "exit" and reconstitute their society free from the troublesome demands of the poor?

Fifth, there are also some interesting problems relating to criticism and political culture. Fishkin is himself ambiguous concerning of the position of religion at one point, arguing that it may underlie political culture (thus receiving protection but becoming subject to critical scrutiny) yet at another writing explicitly that it should be excluded from public dialogue (pp. 145, 202). (Clearly, exclusion means that in the case of conflict, what is public and subject to dialogue wins out, while religious views are treated as if they are matters of private taste; but it is not clear that believers should really want them treated in this way, not least because they will typically think that the content of their religious views is itself rationally compelling. Yet insofar as religious toleration and protection is the product of practical comprise, rather than of the principles that underlie the public sphere, this may itself mean that what is in the public sphere does not have the purely rational character that

In my view, one cannot insulate political culture from other fields; and given that political ideas will be strongly affected by other ideas (and vice versa), it is not clear that one should exclude them even if one could. All this, however, raises a more general problem concerning whether critical rationalism in the form of such openness to public scrutiny can sustain a popular political culture (see Ernest Gellner's *Postmodernism, Reason, and Religion*). Just as, in the earlier parts of Fishkin's book, one might well imagine that critical dialogue could take apart all positive ideas on the basis of which we might conduct our public lives, leaving nothing viable in their place. Or—to put it the other way round—is it not possible that the conduct of public life may require a certain measure of myth, ambiguity, and wishful thinking?

Fishkin's approach would require.)

There also seem to me possible problems with Fishkin's concern for popular and unmediated, rather than merely public, self-reflection. Some of those who, like Hayek, defend a liberal economic order as the best for mankind also argue plausibly that it has certain unavoidable defects. The problem here is that *if* Hayek is right, the connections between the defects and the advantages (and hence the limitations upon remedying prima facie defects) are not easy to explain, and it is difficult to see how they could easily be legitimated to a society's non-theoretically-minded members.

That it raises such problems seems to me an indication of the interest and fruitfulness of Fishkin's approach, rather than a point against it. The book as a whole is readable, interesting, and very thought-provoking. Are there any defects? It does have a somewhat unfinished

feel to it, and Fishkin's initial material could have done with more extensive introduction. His treatment of substantive issues could sometimes have done with a good deal more polishing; and Fishkin might usefully have returned, at the end of the volume, to discuss how his own approach related to the more general themes with which the book commenced. I suspect, however, that Fishkin will not be slow to address these and other issues that emerge from the present book, in a subsequent volume.

Australian National University

JEREMY SCHEARMUR

Institutional Realism: Social and Political Constraints on Rational Actors. By Robert Grafstein. New Haven: Yale University Press, 1992. 244p. \$30.00.

Robert Grafstein has made a valuable effort to confront fundamental questions about the ontological status of political institutions and their effects on political behavior. Grafstein's theory of institutional realism holds that institutions are "real things" that both limit and structure political behavior. He argues that institutions should be understood quite differently from conventions, roughly described as behavioral regularities due to common understanding. Institutions bring order and create patterns even when many people are not aware of the purposes that are being served. In short, the argument is that institutions can be treated as if they are exogenous when studying a good deal of political behavior. Institutions were not necessarily created for the purpose that they currently serve and participants may not be aware of the totality of the institution that constrains them.

Grafstein reached his position after a study of the conventional view, which is perhaps most widely known as it appears in social contract theory. In the state of nature, people recognize the dangers of anarchy and coalesce to create institutions under which they can live. He points out plenty of examples where scholars have interpreted regularized patterns of behavior (that is the way Grafstein defines institutions) as if they were conventions. For example, sociologist Talcott Parsons created an "overindividualized conception of society, one in which stable social structure must meaningfully mirror the standards and patterns of choice ascribed to individuals" (p. 43). Buchanan and Tullock are taken to task because they argue (in The Calculus of Consent) that constitutions are arrangements that are freely entered into as a refuge from the state of nature. Grafstein attacks the functionalist notions that institutions survive because they serve the desires of their participants or that institutions are a result of conscious agreements among people. Instead, "the institution in which agents are embedded establishes certain constraints on their behavior regardless of whether they are understood as such" (p. 82). The bulk of his argument against current spokespeople for rational choice theory (the "new institutionalists") is that they have not escaped the simplistic conceptions of the older conventionalism.

Two parts of Grafstein's argument are the most persuasive. First, if we rely simply on conventions, we do not have enough power to understand some institutions. We need to account for the fact that some institutions reproduce themselves without the conscious thought of their participants. Second, the conventional-

ist's tendency to interpret institutions as optimal communal adaptations should probably be abandoned. Some institutions persist despite their unpopularity. Institutions may be created and continue to exist even though the functions they serve are neither part of the intention of some of their original supporters nor of current participants. This anti-functionalist argument fits well within the current mainstream of political science thinking about institutions. Jack Knight's recent book, Institutions and Social Conflict (1992), advances the same argument. In his recent study of Congressional institutions (Information and Legislative Organization, 1991), Keith Krehbiel takes steps to avoid the functionalist tendencies in interpreting institutions, and attributes institutional design to human intention only when there is solid evidence for doing so.

Another, more dubious theme runs through Grafstein's book. He maintains that since humans are unaware of the institutions around them, our psychological models should be scaled back to reflect the limited nature of knowledge. While this claim probably does apply to some people (e.g., one-time visitors to a local school board meeting), it is hard to see how it can be consistently applied to fulltime politicians (e.g., members of the U.S. House of Representatives). Furthermore, by maintaining the argument that people are generally oblivious to institutions, Grafstein blocks himself from studying some of the really important ques-tions about institutions such as "where do institutions come from?" and "why are some institutions more able to survive challenges than others?" One is reminded of the stance taken by Edward Sait who argued that "New social forms originate and old social forms die without any clear perception, by contemporaries, of what is happening" [Political Institutions—A Preface (1938, p.15)]. Sait maintained that institutions "seem to have been erected, almost like coral reefs, without conscious design." The extremely limiting nature of this theory of institutional creation and maintenance was a major part of the inspiration for the more modern theories of institutions that Grafstein is arguing against.

Instead of developing a theory of institution-building, Grafstein explores some ideas about the effects that institutions might have when they do exist. The most thoroughly developed example of this concerns voter turnout. Rational choice scholars have wrestled with the paradox of not voting for some time, but Grafstein finds their analysis wanting. He invokes evidential decision theory to assert that because of their institutional roles, people might perceive "an unavoidable stochastic relation between their behavior and the behavior of others" (p. 102). For some reason, people believe that if they vote, then others are more likely to do so. The mathematical model indicates that a rational person, guided by evidential decision theory, might choose to participate. Empirical details on the extent to which people actually do perceive interdependencies will need to be demonstrated in future research. In addition, the role of electoral institutions in causing these interdependencies to develop (and strengthen or weaken) will need to be explored.

As is true of any book, there are passages during which one might stop to quibble with interpretations of previous research. A more sympathetic reading of the new institutionalism literature mitigates much of Grafstein's attack. Shepsle's articles, for example, contrast "structure-induced equilibria" with "equilibrium institu-

tions" in a way that accommodates most of Grafstein's criticism. Shepsle does not treat an institution simply as a "settled pattern of behavior," as Grafstein asserts (p. 85). Institutions are rules and procedures—things that exist apart from convention—that may or may not induce behavior patterns. A configuration of rules lasts (the equilibrium institution continues to exist) until a coalition forms that has the power to change it. This theory does not necessarily carry along with it the functionalist baggage of the older sociological or political conventionalism. One need not suppose that institutions are maintained by a consensus; nor is widespread agreement on the reasons for change necessary for change to occur. Institutions have distributive consequences, and when they are aware of this fact some people strive to change them and others resist.

University of Kansas

PAUL EDWARD JOHNSON

The Interpretable Constitution. By William F. Harris II. Baltimore: Johns Hopkins University Press, 1993. 225p. \$38.50.

In the best tradition of political philosophy, Will Harris aims to make us puzzle over things taken for granted and to awaken a sense of wonder about the whole enterprise of written constitutionalism. His book often succeeds in provoking the uneasy feeling that time-honored categories of constitutional analysis are radically inadequate. This alone is a profound achievement.

Harris's ambition is no less than the founding of a new paradigm of constitutional studies, one that would leave behind what he regards as the stale dualisms of standard approaches: strict versus loose constructionism, activism versus restraint, substance versus procedure, interpretivism versus noninterpretivism, and positivism versus natural law (pp. 124–31). These unhelpful dichotomies would be case aside to accommodate Harris's complex "theoretics of thirdness" (p. 29).

Harris's third way is not easily described. It is non-foundational in that it eschews reliance on "external" sources of meaning and authority, including natural law, philosophical theories of justice, and the intentions of the framers where these are not discernible in the Constitution itself. In terms strikingly reminiscent of John Rawls's *Political Liberalism* (1993), Harris argues, "Whatever may be the ultimate 'truth' of the constitutional model from the external perspective, I am not concerned about it. . . . 'Truth' is an inapposite category in political theory" (p. 14).

Harris would have us reconceive the whole genre of constitutionalism as a self-contained practice, "justified not in terms of some higher or firmer independent ground, but owing to its internal sure-fittingness" (p. 11). Written constitutionalism's ambition is, for Harris, comparable to that of Hobbes's sovereign—to ward off chaos by regulating language and reconstituting our lives deeply and pervasively. In the beginning, the sovereign people "speak the Constitution," thereby channeling, bounding, and defining political power and calling themselves into being as a constitutional people. The challenge of the constitutional interpreter is to preserve the existence of this people by moving back and forth between the written document and the con-

stituted political order, interpreting the one in light of the other.

The two orders of constitutional word and policy (or Constitution and constitution) are related not simply analogically but "homologically," as mutually constitutive parallel structures (pp. 90–91). The absence of any external grounding "brings on the need to make the internal enterprise very solid" (p. 11). Political wholeness, order, and legitimacy rest on nothing more than "the continual process in which the words of the document and the activity of the polity are aligned" (p. 13). But not any sort of alignment will do. The original sovereignty of the people lies not in their collective willfulness but in the fact that they will reasonably and as a whole. Interpreters should reconfirm these elevated qualities by striving to render political power comprehensible and reasonable.

Harris rightly disparages the view (so dear to the law schools) that constitutional interpretation is the special preserve of a professional elite. Authority resides not simply in an original act of ratification embedded in the past but in the continual ratification of conscientious interpreters living under the Constitution's authority. Thus, the Constitution's authority depends on its "legibility" and "interpretability"—meaning that we should strive for a politics in which public officials and citizens take their roles as conscientious interpreters seriously, giving and demanding constitutional reasons in public and insisting on real coherence between interpreted document and political practice.

If Harris owes a debt to Hobbes, he clearly owes one to the ancients, as well: he strives to recover the ancient insight that the political regime pervasively constitutes the character of citizens. Harris's constitution is not merely a form of political power but a "regime," or way of life. The terms and categories of a successful constitution become second nature, providing the political language and self-image of the people as a whole.

Harris casts new light on many constitutional issues. He provides, for example, a useful if sketchy account of popular sovereignty, a conception of which he finds adumbrated in the Constitution's provisions for its own ratification. By identifying a notion of sovereign people implicit in the Constitution, Harris makes it clear that the Constitution conceives of (and indeed derives from) a form of authority higher than those required to amend the Constitution. This has the effect of putting the amending power in its place, contained within, and limited by, the Constitution in various ways. Indeed, Harris interestingly argues that popular sovereignty provides a standard for ranking the authoritativeness of Article V's different modes of proposing and ratifying amendments (by legislature and popular convention).

There is much to admire in *The Interpretable Constitution*, but many will be put off by the book's frustrating elusiveness. Much of this is due, no doubt, to the intrinsic complexity of the argument. Yet Harris asserts at one point that clarity "is a controversial virtue. . . . It has a political program. Its technique is to naturalize its assumptions, to make them seem neutral, transparent, inarguable" (p. 37). Harris means to warn us, I believe, that the demand for clarity may discourage the deployment of difficult abstractions even when they are required by the subject matter. This is a fair point, I suppose, but an author bears the burden of showing that difficult prose is necessary. Some will doubt that Harris has met the burden.

One genuinely interesting feature of this book is that

Harris attempts to combine a moral seriousness akin to Sotirios Barber's (*The Constitution of Judicial Power*, 1993) with a resolute dismissal of moral foundations. The constitutional interpreter seeks, says Harris, reasonableness, coherence, viability, and wholeness but no grounding in external standards or universal moral principles. Harris's Constitution is a constructed and self-contained order—a "public system of signs sheltering a widely available lucidity about public matters," a "stipulated public universe" grounded in nothing but its interpretability (pp. 12, 14, 102). A community of conscientious interpreters, Harris asserts, is self-sustaining and in need of no foundational account of the nature of moral reality.

Harris thus appears to join Rawls in claiming that one can put aside foundational questions without embracing the sort of moral flipness found in much of Richard Rorty's work. Despite their similarities, the postures of Harris and Rawls also seem decidedly at odds. (It is too bad that Harris never engages with Rawls's current project). Rawls would have us put aside foundational issues subject to reasonable disagreement without asserting the truth of a particular reasonable view (the range of reasonable foundational views being limited). Rawls does not deny that there is a "true" account of the trust as a whole, he only asserts that reasonable people can disagree (within a certain range) about what the truth might be.

Harris's constitutionalism, on the other hand, sometimes appears to rest on a very definite and particular foundational view-a morally skeptical conventionalism. Harris relies, after all, upon a distinctively Hobbesian rendering of the problem of political order. Hobbes emphasized the radical nature of prepolitical disorder in order to justify political absolutism. Harris also views constitutions as hedges against primal chaos and "the nothingness of disorder and darkness" (p. 49; see also pp. 51, 73, 107, 174, 176). Harris is not out to justify absolutism; rather, he deploys the trope of the darkness and chaos outside the constitution's bounds to emphasize the absence of any external grounding for constitutional meaning. All this also serves to dramatize the Constitution's radically constitutive consequences for social life and citizen character.

It is odd, however, to portray the Constitution as independent of a variety of preexisting sources of order and meaning: the British constitutional tradition, shared beliefs about natural rights, the family, economic markets, the state constitutions, and the English language. Even the Declaration of Independence is completely missing from Harris's account. Harris's point seems to be that none of these sources provides an external standard for settling constitutional meaning, any more than do natural law or the unwritten intentions of the framers. To posit such an external source of meaning would make a mockery of the form of words that were solemnly ratified, that claim to be supreme law, and that we take (or should take) so much pains to interpret; it would deny the fundamentality of this form of words. Talk of darkness and chaos beyond the constitution's well-lit space may not, then, be intended to disparage the importance of history, rights theory, and other matters. It may only imply that all claims to constitutional meaning must be cashed out from within the (at least somewhat) autonomous enterprise of constitutional interpretation.

Harris is acutely aware that the success of the Consti-

tution depends upon its ability to fundamentally reconstitute the polity as a whole in its own image. The radicalism of Harris's constitutionalism is a useful corrective to the bland superficiality of the dominant legalistic models and some liberal theory. Even so, it is far from clear that the need for such a corrective justifies Harris's claim (recurring and melodramatic) that only void and darkness lie beyond the Constitution's orderly and well-lit space. The Constitution constitutes radically—transforming families, churches, and even language in its own image—but it does not create order out of sheer chaos. The Constitution constitutes, but only to a degree. If Harris's view were to take hold, his own corrective would need correcting.

The Constitution is one of our shared practices, but not the only one. Philosophy, history, comparative politics, and other disciplines allow us to imagine alternative constitutions and to keep some critical distance on our own. The very radicalism of Harris's constitutionalism puts that distance in jeopardy. That radicalism and its implicit flirtation with nihilism also jeopardize the faith in public reason that elevates Harris's conception of political authority above mere willfulness.

Perhaps, however, Harris's point is that within a successful constitutional order such as ours, real critical distance is far more difficult to achieve than is usually imagined. The Constitution constructs its ideal readers as its citizens. One might always suspect, therefore, that claims to critical distance are a conceit—that the only standpoints available are hopelessly in thrall to the deep practices and presuppositions of the regime that simply make us what we are. Such reflections can indeed lead one to entertain nihilistic possibilities, and here, again, Harris has a disquieting point.

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STEPHEN MACEDO

Algernon Sidney and the Republican Heritage in England and America. By Alan Craig Houston. Princeton: Princeton University Press, 1991. 335p. \$39.50.

Algernon Sidney was executed in 1683. He was suspected of plotting to assassinate Charles II, king of England, and his brother James, Duke of York, the future James II. In the absence of the two witnesses required by the English law of treason, the court allowed an unpublished theoretical treatise, the *Discourses Concerning Government*, to be admitted as evidence against him. When the *Discourses* were published in 1698, edited by the pantheist and freethinker John Toland, they established Sidney's enduring reputation as a martyr for liberty. Throughout the eighteenth century Americans referred to Sidney and Locke as the two great theorists of political rights, and it is possible that Sidney was more influential than Locke.

There are two reasons now for turning to Sidney's *Discourses*. First, Sidney was one of three authors to write a book-length refutation of James Filmer's defense of absolute monarchy, *Patriarcha*, shortly after it was published in 1680. The other two were James Tyrrell and John Locke, and the similarities, differences, and possible relations of influence between the three works are matters of continuing debate.

Second, Sidney (who fought for Parliament in the Civil War and spent years in exile after the Restoration) was widely regarded as an obdurate republican. Ever since the publication of John Pocock's Machiavellian Moment (1975) scholars have been studying the links between seventeenth-century English republicanism and the American Revolution, and debating the claim made by Pocock, Gordon S. Wood, and others, that John Locke, liberalism, and the natural law tradition of argument were relatively insignificant in the making of a revolutionary consciousness, while a contrasting republican tradition, concerned with virtue, corruption, and citizenship, rather than rights and obligations, was all-important.

Modern Sidney scholarship may be said to begin in 1978, when Blair Worden announced the discovery of the Court Maxims, a previously unknown (and still unpublished) political treatise by Sidney, apparently written in 1666. Since then, four volumes, apart from Houston's, have been devoted to Sidney: Paulette Carrive (1989); John Carswell (1989); and Jonathan Scott (1988 and 1991). Of these authors Houston is undoubtedly the best. He is learned and accurate; moreover he writes well, and argues clearly. (Some of the best arguments and liveliest quotations are concealed, in the manner of Gibbon, in the footnotes.) As a thesis this study received the Leo Strauss Award, and the book has gone on to win the Foundations of Political Theory Book Award.

Houston begins by providing an admirable summary of what is known of Sidney's life and political activities, and a sketch of the royalist theories he opposed. It is particularly helpful to have an explanation of why some scholars have argued that Filmer's views were typical of Tory thinking, while others have insisted they were exceptional. At the heart of the book stand three chapters on the political theory of the *Discourses*. These stress that Sidney believed in popular sovereignty and the separation of powers, not in "mixed government" as that term is usually understood. They are followed by an important final chapter on Sidney's influence in America.

The book's central argument is a simple one: Sidney writes in the natural law tradition of Grotius as much as he does in any republican tradition. Eighteenth-century readers usually coupled Locke and Sidney together, and felt under no obligation to choose between them. Scholars who follow Pocock and Wood in contrasting republicanism to natural rights theory obscure the presumption of contemporaries that the two complemented each other. (A similar argument has been made with regard to Cato's Letters by Ronald Hamowy).

On this question I find Houston entirely convincing. I am not persuaded, though, by his further claim that Machiavelli was not an important influence on Sidney (pp. 164-5, and other places listed in the admirable index.) I find it hard to read Sidney on the need to foster a warlike people (2.15, 2.21, 2.22, 2.23: "That is the best government which best prepares for war") or on civil disorders (2.24) without sensing the influence of Machiavelli. When, for example, Sidney defends "extrajudicial proceedings" and "extraordinary ways" I suspect he is translating Machiavelli's modi straordinari. It is true that there are numerous Machiavellian themes which do not interest Sidney (p. 165), but it is not true that for Machiavelli corruption and change are synonymous (p. 218): when Sidney criticizes those who think they are it is presumably Harrington, not Machiavelli, he has in mind.

If Houston underestimates Machiavelli's influence, he

is right to insist that republicanism and Machiavellism should not be treated as synonymous, and that Machiavellism should be regarded as a range of preoccupations, not a single theory. He correctly notes that Sidney's favorite author is not Machiavelli but Tacitus (p. 154). Of course seventeenth-century readers often turned to Tacitus to find there the arguments of Machiavelli, an approach which had been particularly encouraged by Traiano Boccalini, whom Sidney twice cites with approval (2.21, 2.30). But, like Sidney, Boccalini was interested only in a few specific aspects of Machiavelli's thought. It would have been interesting to compare Sidney with Boccalini, for both played a crucial role in adapting the republican thought of the Renaissance to make it serviceable in nation rather than city states. Such a comparison might have provided the basis for an alternative account of the republican tradition to that proposed by Pocock.

It is a sign of a good book that one wishes the author had written more not less. Houston describes the argument of Sidney's *Discourses* as "a sunken ship: though a few promontories are visible above the surface, the great hulk remains submerged in the depths" (p. 180). He has done an admirable job of salvaging what can be recovered from the wreck, and is persuasive in his claim that its design was less antique than has previously been believed. Sidney died so that his beliefs might live: thanks to Houston we now have a better understanding of what those beliefs were.

University of Victoria

DAVID WOOTON

Justice between Age Groups and Generations. Edited by Peter Laslett and James S. Fishkin. New Haven: Yale University Press, 1992. 243p. \$30.00.

Choice over Time. Edited by George Lowenstein and Jon Elster. New York: Russell Sage Foundation, 1992. 352p. \$39.95.

The issues of maintaining the quality of the ecosphere, the sustaining of the modern welfare state, and the restructuring of our national debt are each of immediate interest and of concern for those who examine intergenerational equity issues. More generally, those who look at intergenerational issues must concern themselves with how humans make decisions regarding outcomes that occur in future time periods. Similarly, those who must analyze how we choose from among alternatives with payoffs in the future will want to consider applications that go beyond one person's lifetime (i.e., intergenerational problems).

The Laslett and Fishkin volume is a collection of 11 essays that can serve as a serious introduction to some of the philosophical issues regarding intergenerational justice. The Lowenstein and Elster volume explores some of the experimental and other empirical findings and technical theoretical issues involved in explaining choice behavior involving delays and time. Although both books are of considerable depth and quality, neither really integrates the two concerns of intergenerational choices and choice with a time dimension. Thus, the student of either subject may wish to look at the two together for a rounded grasp of the subject. The pair of essays that Tyler Cowen helped author in the Laslett and Fishkin volume, analyzing the issues surrounding

the discounting of future payoffs in choice, comes closest to dealing with both issues at the same time.

But the lack of integration of the subjects of the two should not be taken as a harsh criticism of either. The Laslett and Fishkin volume contains a number of very interesting essays for those of us concerned with public policy or intergenerational equity. Although many may be aware of the longer history of welfare policies in the West, the historically oriented essay by David Thomson, "Generations, Justice, and the Future of Collective Action" is still worthwhile to jog our memories. Thomson summarizes some of his other research to show that the current insurance type of welfare state is historically not likely to be considered stable. Large transfer programs of the sort currently offered in our modern welfare state also were around in the nineteenth century and earlier. They all went through cycles of lack of popular support. Thomson then argues that such instability threatens the insurance value of the state programs and hence their value in the eyes of the citizens. He also makes the somewhat more controversial and original argument that these social (or entitlement) programs really are a redistribution to a particular generation. All in all, this is a controversial and enlightening piece.

Most of the other papers are substantially more philosophical in nature, and some of these are very strong indeed. Especially helpful were the pieces by George Sher ("Ancient Wrongs and Modern Rights") and James Fishkin ("The Limits of Intergenerational Justice"). Sher's piece is a very useful extension of Nozick's argument regarding compensation over time. Sher investigates the conditions under which the justification for compensation decreases as the event is further in the past, and he does this in a highly sophisticated and interesting fashion, dealing with such issues as the cross-generational consequences of behavior.

Fishkin, on the other hand, indicates how we might have to consider new foundational concerns if we wish to extend ethics across intergenerational issues. For example, traditional theories of harm and torts maintain individual identities as specific arguments in the welfare arguments (e.g., if you do X, it would hurt that particular person, i, and thus you should offer to i compensation). Fishkin shows that these sorts of argument are problematic for many (if not most) intergenerational equity issues. Juxtaposing this class of theories with "individual-identity-independent" theories of ethics, he shows that the theorist has no strong options. Fishkin then sketches how some of his earlier published ideas can be extended to perhaps give us some leverage in these situations.

Some of the other pieces (e.g. those by Braybrooke and Epstein, and the introduction were a bit less interesting. Braybrooke and Epstein restate quite classical and opposing ideological positions that are not really worth reviewing. And Laslett and Fishkin's introduction was less a guide to the skeletal structure of the volume than it might have been.

As indicated, the two pieces by Cowen can be seen to serve as a bridge between the two books, especially "Consequentialism Implies a Zero Rate of Intergenerational Discount." This essay is in the style of social choice proofs and includes a demonstration that (1) Pareto indifference, (2) transitivity of indifference, (3) neutrality (between persons within generations), and (4) well-defined preferences across living in different eras imply that one have no discounting of future states. Although

there were some problems with the analysis, the argument was valid and interesting. Validity, of course, does not imply soundness; and among my objections to the soundness of his claims, I would include his assertion that the four conditions are equivalent to consequentialism, that is, are needed to evaluate the qualities of an outcome (e.g., in cost-benefit terms). Also, Cowen does not properly deal with some of the difficulties in the four conditions. For example, the axiom of Pareto indifference has numerous limitations, which are not properly fleshed out. Indeed, its problems are similar here to its problems in the case of noncomparative utilities in Sen's proof that we cannot make judgements about redistribution. But the chapter is still a very interesting point of departure.

Choice over Time is concerned with individual choice when payoffs associated with alternatives are best understood as varying in their time of receipt. The concerns addressed here are primarily those of the modeler and theoretician in the psychology, economics, and public choice traditions. Within this book, one finds considerable information about the problems, findings, and theories of individual choice with delayed payoffs. The difficulties and models outlined by the authors here are quite analogous to the conundrums and anomalies developed by Kahneman, Tversky, Grether, and others in the areas of choice under risk. The researchers interested in prospect theory and other such models will find this volume quite enlightening. The very useful introductory essay weaves a demanding collection of essays into a manageable intellectual tapestry. The utility of the introduction is greatly enhanced by a second essay by Lowenstein, giving a sketch of the intellectual history of this field. But getting into the remainder of the volume in depth is a daunting affair, unless one has previously invested heavily in this area. In too many of the essays, the technical terms are not defined, and the models are not carefully explicated for the new comer. This failed task leaves the volume of serious interest almost exclusively to those who are already literate in research in the area of time-discounted choice. The essays contain reports and critiques of virtually all the important experiments. The major controversies and models are aired and even, in some places, extended.

The foundational conceptualization of choice concerning delays serves as a solid foundation for a number of alternative conceptualizations of the problem, including concerns with the shape and contents of the discounts. Essays by Mischel, Shoda, and Rodriguez; Ainslie and Haslam; Rachlin and Raineri; and Lowenstein and Prelec investigate these alternative foundations. They further lead to a common theme, or unity of perspective, that persuasively paints the area's major anomalies (e.g., predictable patterns of preference reversals over time as generalizable models of discounted choice over time. Indeed, this is done with considerable panache and intellectual sweep. Ainslie and Haslam, for example, move from the problems of general personality instability (e.g., as manifested in extreme cases via multiple personalities) to more ubiquitous preference instabilities.

Even though some of the findings are often found more compellingly developed elsewhere, this collection shines as a serious dialogue over the issues and extensions that are at its heart. For example, the essay by Schelling on self-control is an echo of his pieces in the American Economics Review and Public Interest. But here we find Shelling's argument "formalized" and placed in

a tradition of hyperbolic discount functions in a second essay by Ainslie and Haslam. This generalization enables us to see the general problem of addiction and disfunctionality (as well as self-control) as special problems in the area of discounted choice. As a general problem, self-control is further developed in three other essays by Shefrin and Thaler; Becker, Grossman, and Murphy; and Herrnstein and Prelec. With such juxtapositions and selections, Elster and Lowenstein have put together quite an intriguing volume.

In sum, those nonneophytes interested in micro issues of choice involving payoffs through time will find the book of value. And those interested in the macro policy concerns of the subject (e.g., as dealt with in the Laslett and Fishkin volume) should find the theoretical and empirical contents of the book of considerable use in their positive analysis of the choices over the policy alternatives.

University of Maryland

JOE OPPENHEIMER

The Social Cage: Human Nature and the Evolution of Society. By Alexandra Maryanski and Jonathan H. Turner. Stanford: Stanford University Press, 1992. 213p. \$39.50 cloth, \$12.95 paper.

Unlike the vast majority of theory works in social science, Maryanski and Turner embrace natural science as a relevant source of theory, data, and methods that social scientists can employ the better to understand human social behavior. In particular, this book deems evolutionary theory and primatology to be indispensable to realistic analysis of the social biological adaptation of contemporary human societies. The Social Cage is authored by sociologist biosocial-theorists. It was evidently written for sociologists, but it is certainly readable by political scientists. Members of the APSA's politics and life sciences section agree with Maryanski and Turner about extending historical time by using evolutionary theory in a serious way; insisting upon conceptualizing humans and primates, whose big brains are responsible for their unique cultural constructions; and insisting also upon transcending the usual social science cultural determinism by substituting a transactional biosocial approach to any study of human social organization, communication, and behavior generally. There are, of course, hundreds of such works by physical anthropologists, as exemplified by Adrienne Zihlman's The Human Evolution Coloring Book (1982).

Maryanski and Turner's discussion of human animality is exceedingly brief and relies much more than necessary on pop ethology, instead of the standard works by zoologists who teach and do research in the field of animal behavior. The authors' selection of sources for, and coverage of, primate systematics is much better done, both qualitatively and quantitatively. However, they do represent the organizational (and even more important, the aggressive) behavior of common chimpanzees by giving major credence to the later, rather than the earlier, work of Jane Goodall. Of course, the earliest work from 1960 to 1963 was done by Jane herself, observing the Gombe chimpanzees in a relatively much more natural setting than later with dozens of collaborators, after the human political, economic, and social environment caged her research enclave in. Anthropologist Margaret Power has pointed out that Maryanski and Turner "do rely wholly on reported behaviour of the Gome (and Mahali) studies of abnormally stressed chimpanzees, in drawing up Table 1. This results in a picture of the natural behaviour of chimps generally which is the opposite to what they would have found, had they drawn on the naturalistic studies of undisturbed (not artificially fed) apes. The result is that they propose an extremely misleading understanding of the probable behavior and relationships of our hominid ancestors" (personal communication). Moreover, as nonexperts in the subject they summarize, they give the impression of being perhaps overly impressed with such methodological innovations as cladistics and its imputed power to determine what pre-World War II paleoanthropologists called the "Missing link," which Maryanski and Turner denote as the last common ancestor among humans and other great apes. On the other hand, their detailed cross-species analysis of the species-specific social network characteristics among both hominoids and Old World monkeys, proffers a provocative approach for generalizing about the social organization differences among and between apes and monkeys.

They take quite a bullish stance on the question of the language/mental abilities of chimpanzees and gorillas, which they manage to do at least in part by overlooking such skeptical works as Thomas Sebeok and Jean Umiker-Sebeok's Speaking of Apes (1980). Much better is their discussion of psychobiology in relation to the transactional development of brain size and language among hominids over the last four million years and especially the last million years, together with a good summary of the complex relationships between primate/ human perceptive sensory organs and cognitive phylogenetic development. Then they relate sensation-cognition-language to hominid/human behavior—in response to ecological changes through evolutionary time—as determinants of the social communication basis for hominid social organization and behavior. That, in turn, resulted in the extraordinarily stable forager, gatherer, and hunter niche that characterized Homo erectus, Archaic homo sapiens, Homo sapiens Neanderthalis, and Homo sapiens sapiens for 1.5 million years, in contrast to the maladaptations of the past 12 thousand years (i.e., less than 1% of the gathering-hunting base time).

Maryanski and Turner assert that when the huntinggathering niche was universal (initially, in Africa then later through the Middle East to Europe in the West and Asia in the East), humans all lived in a correspondingly universal Garden of Eden—as compared to the niches that followed during and after the transitions to herding, gardening, agriculture, industrialism, and postindustrialism. In this regard, it is evident Maryanski and Turner, in their discussion of preindustrial societies (chaps 4–5) follow closely the theory and terminology advanced in the sixth edition of a more than 20-year-old leading text in biosocial sociology, which anticipates the same argument about the relative similarity of industrial societies to gathering and hunting groups, in regard to the putative isomorphism between the evolved species genome and the benefits of the adaptation in terms of such positive values as egalitarianism, leisure, freedom, and so on (Lenski, Lenski, and Nolan's Human Societies [1991], 403-37). Also, includes in his earlier introductory text in general sociology a briefer but theoretically identical statement of the present discussion of preindustrial and industrial societies, except for a less extensive development of social organization network theory (Sociology, 1985. chap. 4).

Gathering and hunting was an Eden because population sizes and levels remained sufficiently low that adequate food (both plant and animal) could be acquired with relatively little time and effort by small groups of humans who moved about opportunistically, continuously resettling their habitat. No one could amass material wealth; leadership was multiple-role-oriented; males and females were deemed to be equally important; and the group was also egalitarian in its economics, society, and politics. This, in turn, promoted a high degree of individualism. Over the period of 1.5 million years, the human species genome was shaped by selection for phenotypes that could behave in ways appropriate to the gathering-and-hunting niche; and that adaptation become genetically fixed by perhaps a hundred thousand years ago, by which time both brain size (weight) and language capacity had stabilized. (This interpretation of human phylogenetic development is quite conventional among paleoanthropologists and paleo-human biologists. It may be novelty for most political scientists but not, of course, sociologists.)

The social cage consists of the increasing constraints on human individual and social freedom imposed by the categorical imperatives of humans' adapting to "civilized" life as the caretakers of domesticated plants and animals (as Maryanski and Turner explicate in great detail in their chapters on horticultural and agrarian societies). Indeed, by the time one reaches their concluding chapter, cage becomes a much-overworked metaphor for human self-domestication.

An especially novel—and to me perplexing—section occupies the latter part of chapter 7 (on industrialized societies) and chapter 8. Thus, "industrial societies . . . [are] far better than their predecessors because of their capacity to free people from much physical drudgery and to give them wide arrays of options and opportunities and much more say in how they run their lives" (p. 140); yet they also assert that "kinship in industrial and post-industrial systems completes its odyssey back to its original nuclear profile in hunting and gathering societies" (p. 157). They had previously characterized kinship in hunting and gathering societies as involving "neolo-cal, bilocal, and patrilocal" residence and a "clear division of labor; males hunt, and females gather and do domestic chores" (p. 86) which seems difficult to reconcile with contemporary yuppie family life in postindustrial societies. Margaret Power has emphasized that the impact of kinship throughout human evolution has been to establish bonding, not caging; and she shows that the patrilocal band model, as distinguished from the forager model that characterized human organization throughout the initial 99% of human organization throughout the initial 99% of human existence, reflects pre-1960s anthropology (The Egalitarians-Human and Chimpanzee,

Maryanski and Turner further state that "the state is transformed with industrialization, becoming much more democratic and larger" (p. 157). Again, "The changes associated with post-industrial systems are more compatible with humans' biological nature than those occurring in earlier ones" (p. 161). Again "[Postindustrialism is] a system comparatively well attuned to [humans'] ape ancestry . . . [and] in truth ordinary people embrace the chance to live and participate in a system relatively well attuned to their primate heritage" (p. 162). In other words,

industrial society, technology, postindustrialism, and our contemporary nuclear-computer economy and society have "liberated" humans from their agrarian bondage, so that individualism is on the rise again; and, through the blessings of industrialism and technology (as exemplified by electronic kitchens that free their possessors to see professional employment meet to their high education and skills), they are living more like their gathererand-hunter forebears than any of their immediate foremothers-and-fathers could do during the 10 thousand years or so preceding the nineteenth century in England, Europe, and North America.

I have no doubts that industry and technology have given Jane Fonda and Ted Turner a relatively greater freedom that the average gardener or farmer-or even gatherer-hunters might once have enjoyed. Maryanski and Turner might be more persuasive if they attempted to demonstrate empirically that most people in modern industrial/postindustrial societies are upper-middleclass or better and are politically equal in fact and that they enjoy the leisure of a four-hour (i.e., a gathererhunter) working day. But the facts are that yuppies and libertarians and Perotistas are a small minority even in such great capitalistic societies as the United States and Japan and few of them are on four-hour workdays. I do assert-and on empirical grounds-that the social cage of postmodernist postindustrial technological life is the opposite, in its effects, to what Maryanski and Turner claim and hence that their concluding two chapters have got it backward. The social cage today is exponentially worse than that of agrarianism with regard to the economic security and the political and social freedoms that for most people make their lives worth living.

Nevertheless, and irrespective of my caveat on the empirical question whether contemporary humans are now occupying a niche that resembles that of gatheringand-hunting peoples, this book will be informative and enlightening to the many political scientists who attempt to teach and research politics while at the same time ignoring evolutionary theory, human evolution, and the biosocial aspects of human social organization and political behavior. I recommend The Social Cage to political scientists as a novel, intriguing, and exciting approach to understanding the relevance and importance of a great deal of "non-social science," that is, biosocial research findings relating to human communication, social organization, and political behavior today. Political behavior has been a reality since Homo sapiens began some 50 thousand years ago and, to some extent, even before then in the social life of our hominid and, earlier, hominoid forebears.

University of Hawaii, Manoa

GLENDON SCHUBERT

Foundations of Liberalism. By Margaret Moore. New York: Oxford University Press, 1993. 222p. \$45.00.

This is a very good book. Margaret Moore critically discusses, with admirable clarity and fairness, the work of a number of leading contemporary liberal thinkers, including Gewirth, Rawls, Gauthier, Larmore, Kymlicka, and Raz. Informed by a sympathetic appreciation of contemporary communitarian thought, her judgments regarding liberal theory, though often critical are not marred by the strident, polemical tone too often found in similar exercises. The analysis is detailed and

careful, with far more insights generated than axes ground. Along with Will Kymlicka's Contemporary Political Philosophy (1990), this book provides one with a sound and sophisticated overview of the "main positions" staked out in contemporary liberal theory. But it will also be of interest and benefit to those already familiar with these theorists and their critics. I have read a great deal of material dealing with liberalism and communitarianism over the past five years, and this ranks with the best.

The analysis is organized around the contrast between the personal point of view and that of the impersonal or impartial perspective of morality. The various liberal theories are evaluated in terms of the facility (or lack thereof) with which they articulate a compelling solution to the "integrity problem" (explaining how the personal and impartial perspectives are related) and the "motivation problem" (explaining why the person has reasons to act in accordance with the demands of impartial morality). In part 1, Moore applies this analytical framework to the theories of Gewirth, Gauthier, and the Rawls of A Theory of Justice, treating each as an attempt to resolve these problems. While seeing somewhat less merit in Gauthier's version of instrumental morality than Moore does, I nevertheless found her treatment of all three to be enlightening. My guess is that of the thinkers treated in the book, Gewirth's views are the least well known to political scientists; Moore's chapter on him will serve as an excellent introduction and critical overview. The chapter on Rawls is also quite good. I was especially intrigued by Moore's comments on the relationship between the structure of the family as an institution and the development of moral sentiments; they make for an interesting comparison with those of Susan Okin expressed in Justice, Gender, and the Family

In part 2, Moore considers contextual (Larmore, the later Rawls) and perfectionist (Raz, Kymlicka) arguments for liberalism. Moore's evaluative stance toward these views is somewhat more equivocal than the straightforwardly negative conclusions advanced in regard to the theories treated in part 1. This is because the views treated here (especially those of the later Rawls and Raz) are in important respects, similar to the views Moore herself advances in part 3 as an "alternative foundation for political and ethical principles." Moore acknowledges these similarities openly, while nevertheless rejecting the theories of (the later) Rawls and Raz in their present formulations. This difficult balancing act is well executed, and the resulting discussion of possible strategies of articulating and defending liberal principles (or political principles generally) will be thought-provoking and illuminating to anyone concerned with such

The similarities are, roughly, as follows. Moore's alternative foundational views include (1) a conception of the person that accepts the notion of a constitutive tie between personal identity and the social relations characteristic of a particular community; (2) a conception of justification (of ethical and political principles) sufficiently modes, local, and historically bounded not to aspire to universal or timeless validity; and (3) an affirmation of the perfectionist view that the state can be "justified in acting to protect and promote valuable ways of life. State action in support of certain ways of life may be necessary to ensure a flourishing and valuable culture, in which meaningful choice is possible" (p. 193). Rawls now

seems to endorse view 2 and arguably view 1, while Raz endorses views 1 and 3 and arguably view 2. Moore nevertheless criticizes Rawls and Raz on the grounds that the liberal political principles each endorses are ultimately inconsistent with these foundational philosophical claims.

There is, in my view, a good deal to be said for versions of all three views and hence for the foundational views that Moore skillfully defends in part 3. I was not as convinced by her arguments in support of the claim that liberal political principles are, of necessity, in a contradictory relation to these foundational viewswhich is another way of saying that I think Rawls' and Raz's liberalisms more defensible than does Moore. Readers will have to judge this and much else for themselves, of course, but they may be assured that reading Moore's book will provide much stimulation toward doing so and a fine example of how to do so thoughtfully.

University of Vermont

PATRICK NEAL

On the Edge of Anarchy: Locke, Consent, and the Limits of Society. By A. John Simmons. Princeton: Princeton University Press, 1993. 293p. \$39.50.

In this work, the author seeks "to present, analyze, and to a certain extent defend . . . John Locke's own theory in his Two Treatises of Government." In addition, On the Edge of Anarchy tries to present "the best possible defense" of Locke's political philosophy, although this may involve "excising or dramatically revising" Locke's thought, as well as subjecting it to "a contemporary critique" (pp. 3, 7). What we are promised, then, is that "the new Lockean view that will emerge at the end of this work is a better, clearer, more helpful view than Locke's own" (p. 9). This approach was adopted by Simmons in The Lockean Theory of Rights, and, as a comparative assessment, it has to be said that the objectives of Simmons's methodology were much more successfully realized in the earlier work. Whereas it was concerned only with Locke's theory of rights and not his whole political philosophy, the present book reverses this emphasis. One of its defects lies in Simmons's truncated version of what constitutes the "whole" of Locke's political theory.

On the Edge of Anarchy is structured around a discussion of Locke's account of the state of nature, his theory of consent, and his theory of resistance-important elements of Locke's political thought, to be sure. While these elements are considered in relation to each other, much of what gives meaning to "Locke's own theory" is omitted from the discussion altogether. This is because whereas Locke's meaning is often embedded in the political conflicts of seventeenth-century England, Simmons's meaning is often derived from contemporary

philosophical definitions.

For example, in discussing Locke's definition of the state of nature as a condition lacking "a common judge with authority," Simmons argues that Locke cannot really mean that want of a common judge is a definitional feature of the state of nature. This argument is premised upon a rather sharp distinction between a 'judge" and a lawmaking body, so that (Simmons concludes) a "common judge would appear to lack the right to make law for those he judges between"

(pp. 18-19). But this enforces a modern distinction that Locke and his readers would not have felt compelled to make, since they regarded Parliament as both a lawmaking and a judicial body. So when Locke writes that what "puts men out of a state of nature into that of a commonwealth, by setting up a judge on Earth with authority to determine all the controversies, and redress the injuries, that may happen to any member of the commonwealth; which judge is the Legislative, or magistrates appointed by it," it is clear that for him, the 'judge" and the lawmaking body are the same (Second Treatise 89). Moreover, this perspective is part of another argument that Locke is making here, namely, that no one could "think themselves in civil society, till the Legislature was placed in collective bodies of men, call them Senate, Parliament, or what you please"; and this civil society, with the legislature as the common judge with authority, is again contrasted with the state of nature by mutually exclusive definitions (ibid. 94). What is obscured in Simmons's discussion is the political importance of this argument against absolute monarchy, which (as Locke defines it) is simply the combining of executive and legislative power in the hands of a single individual (ibid. 91). Locke's conception of a limited monarchy in terms of institutional limitations (i.e., a division of executive and legislative power) is thus important for his defense of "civil society" as a particular kind of institutional arrangement or distribution of political power, contrary to Simmons's reading of the text, which seems to imagine that Locke has an interest in defending a notion of "limited monarchy" as a legiti-mate form of government apart from the institutional division of powers just described above (ibid.).

In discussing natural rights, Simmons argues that there is "a distinct lack of evidence" that Locke "took himself to be defending a thesis of inalienable rights" (p. 119). Much of this discussion is concerned with arguing that since we do not possess certain rights (e.g., the right to consent to kill ourselves), we cannot alienate what we do not have (pp. 120-21). Armed with this philosophical pronouncement, we may then dismiss virtually all of the Lockean candidates for inalienable rights, including the right to religious toleration (pp. 108-46). This seems an example of the kind of word manipulation that frequently passes for philosophizing. If I am entrusted (by God and natural law) with a right to choose my religious beliefs and, Locke argues, this right can neither be delegated to another nor taken away from me by another, this right to choose would normally be understood by individuals living in the seventeenth century as an inalienable right. Moreover, as part of a political debate, the claim for an inalienable right plays an important role in undermining the claims for authority advanced by Locke's opponents.

Simmons maintains that while Locke defends a right to resistance to tyranny, he does not argue that we have a duty to resist. According to Simmons, Locke believes that "we have the right to aid the oppressed, but not a duty to do so," although Simmons believes that Locke should have defended the duty of the people to resist injustice (pp. 179–81). Again, this seems to me to be a play on words at Locke's expense. For, in the seventh of the Essays on the Law of Nature, Locke elaborates upon the duties and obligations placed upon individuals by natural law, which include "the relief of one in trouble," as well as "the consoling of a distressed neighbor" and "the feeding of the hungry." It seems to me that "the

relief of one in trouble" covers the case of individuals being wrongly imprisoned and/or tortured because of their religious or political beliefs, which was certainly relevant to the argument for the right to resistance Locke was making in the *Two Treatises*. The fact that these duties (including charity to the poor) depend upon individuals' having the resources to implement them (which, in the case of resistance, may, as a prudential matter, require at least the tacit support of the majority) does not, in Locke's view, remove them from the status of being duties, as Simmons suggests.

The major portion of On the Edge of Anarchy is concerned with Locke's theory of consent and various suggestions for improving that theory. "Locke's real mistake," Simmons argues, "was in saying that one could only become a member [of political society] by express consent" (pp. 89, 194). Since Locke means actual personal consent (p. 60), we are faced with either an empirically implausible condition for establishing the legitimacy of government or with the attempts of Locke (and other consent theorists) to conflate express and tacit consent to the point where the very meaning of express consent is undermined (pp. 86-87, 197-269). Moreover, Simmons follows John Plamenatz and others in arguing that Locke's usage of the term consent has no reference to any particular form of government (p. 91). Hence, as a general characterization, "the problem for consent theory . . . [is] one of realism" (p. 199). It is hardly possible here to discuss (let alone resolve) the various problems arising from consent theory; but instead of condemning poor Locke for his lack of realism, it might be more profitable to recognize the existence of at least three linguistic conventions in seventeenthcentury England that did associate consent with the existence of elections and democratic practices, so that the historical plausibility that Locke's discussion of consent in the Two Treatises is grounded in a realistic attempt to associate political legitimacy with the existence of such practices can at least gain a fair interpretive hearing, instead of being dismissed out of hand as an expression of Locke's "own meaning" (as it generally is by philosophically oriented interpreters of that work).

None of my comments are meant to suggest that On the Edge of Anarchy is a bad book. On the contrary, Simmons has many intelligent and thought-provoking things to say about Locke, and he is both painstaking and comprehensive in his efforts to relate his own position to the existing secondary literature on Locke. Even so, On the Edge of Anarchy does demonstrate that there is still a gap between those who believe, with Plamenatz, that history and politics simply provide the background information for philosophical analysis and those of us who argue that what the philosophical problems are, for a thinker like Locke, cannot be stated independently of a contextual (political/historical) knowledge relating to the formulation of these problems.

University of California, Los Angeles RICHARD ASHCRAFT

Liberal Nationalism. By Yael Tamir Princeton: Princeton University Press, 1993. 194p. \$24.95.

Yael Tamir's Liberal Nationalism is a philosophical essay on a subject of central importance in today's

politics. Inspired by her involvement in the Israeli peace and civil rights movements, it attempts to reconcile two sets of seemingly contradictory values on which both movements are based: the values of liberalism and of nationalism. The purpose of the book is to redefine "the national vision" in the light of liberal values, to place "national thinking within the boundaries of liberalism" (pp. 3-4, 12). Tamir believes in the possibility of "an ideal marriage" between the two, in that "the liberal tradition with its respect for personal autonomy, reflection, and choice, and the national tradition, with its emphasis on belonging, loyalty, and solidarity, although generally seen as mutually exclusive, can indeed accommodate each other" (pp. 6, 140). Such accommodation, she claims, will benefit both traditions: liberalism will be rendered more attuned to people's deep affective needs, as well as more honest (aware of its own hidden nationalist agenda) and less egoistic, while nationalism will thereby be enabled to meet "the challenges of the next century" (p. 77).

Liberalism, in Tamir's discussion, is defined essentially as "a theory about the eminence of individual liberties and personal autonomy"; nationalism is defined as a theory "about the eminence of nationalcultural membership and historical continuity, and the importance of perceiving one's present life and one's future development as an experience shared with others" (p. 79). Speaking about nationalism, Tamir employs the terms of communitarian discourse, which, she claims, is "akin to the nationalist one in its content" (p. 14). The "nation," on which the definition of nationalism is predicated, is seen as a form of cultural community "imagined" by its members as distinct from others. Tamir is aware of the "illusive" quality of this definition, which does not allow to distinguish between nations and other cultural communities, but she believes that a more satisfactory definition is impossible (pp. 9, 66). The model of liberal nationalism that Tamir offers presup-poses the "contextual individual," for whom personal autonomy is as essential as embeddedness-the two needs liberal nationalism satisfies (pp. 33, 104-7). Liberal nationalism is polycentric, it "is predicated on the idea that all nations should enjoy equal rights, and in fact derives its universal structure from the theory of individual rights found at its core. If national rights rest on the value that individuals attach to their membership in a nation, then all nations are entitled to equal respect" (p. 81; see also p. 9). Liberal nationalism is best realized in the framework not of nation-states but of multicultural regional entities—political, military, and economic unions of separate but equal nations—each assured the conditions for cultural self-determination, which enable individuals to choose their national identities for the only relevant (i.e., "national-cultural") reasons (pp. 151-57, 163).

Tamir is aware that "recent versions of nationalism seem to lend little credence to the liberal nationalist position" she offers. In fact, she says, "a cursory glance at the surrounding reality could easily lead to the conclusion that liberal nationalism is a rather esoteric approach" (p. 117). Why should this be so? Why should nationalism, in reality, so often take illiberal forms and inspire political systems not even remotely resembling Tamir's ideal vision? The book does not address these questions and, taken on its own terms, cannot be criticized for that. It is an essay in philosophy, a "normative [and] programmatic" statement, rather than em-

pirical investigation; Tamir's goal is to suggest a model of nationalism compatible with liberal tradition, not to explain nationalisms as they are (pp. 6, 81). But in this day and age, questions raised by such nationalisms force themselves upon us and one can hardly resist the temptation to ponder them.

One central fact about nationalism as it exists is that it is not a uniform phenomenon. Rather than being a theory comparable to liberalism, it represents a class of theories, all of which share the definition of the political community as a "nation" (implying popular sovereignty and essential equality between members) and can differ dramatically in all other respects. All nationalisms satisfy and recreate a need for dignity, or (as Berlin, quoted by Tamir, defines it) status and recognition-which, in the absence of alternative ways to satisfy it, assures the continued appeal of nationalism. Different nationalisms, however, satisfy this need in very different ways and, in doing so, generate additional needs peculiar to them. In concrete experience, the abstract value of a "membership in a nation" is eclipsed by the value of a membership in a particular nation, with the particular satisfactions and comforts it affords. Being a member in a nation does not define one's identity: one is an American, a German, or a Russian—a member in the nation, defined as a community of free individuals, a superior race, a military superpower, a cultural giant, or the like. Very often, the satisfaction afforded by membership in the nation is predicated on the assumed inferiority of other nations, deemed to be undeserving of equal respect with one's own. Membership in a nation implies participation in the national ideals. These ideals differ: a supreme good for one nation may be an object of contempt for another. The nature of the national ideals defines the type and the specific character of any given nationalism. Liberal nationalisms are nationalisms based on liberal ideals. They are rare because illiberal nationalisms (of which there are many) are more effective in satisfying their members' need for dignity—perhaps because they relieve individuals of personal responsibility and define dignity as a collective attribute.

"Placing national thinking within the boundaries of liberalism" may, therefore, be equivalent to the attempt to combine different types of nationalism, subsuming an illiberal nationalism (e.g., the ethnic and collectivistic variety) under a liberal one and transforming the former into the latter, thereby changing people's ideals and aspirations. "Can this be done?" is a question of a great practical importance. Such a transformation of national identity, for example, is a necessary condition for the democratization of post-Soviet societies. Tamir constructs a philosophical ideal of nationalism, but in leading the reader to questions such as this, she also performs a valuable service for those who try to understand its reality.

Harvard University

LIAH GREENFELD

Inequality. By Larry S. Temkin. New York: Oxford University Press, 1993. 352p. \$49.95.

Larry Temkin, an associate professor of philosophy at Rice University, has undertaken a daunting task. Observing that philosophers have written scores of books and journal articles in recent years in defense of egalitarianism, what he finds missing from their collective enterprise is any discussion of how one might go about measuring the degree of inequality between alternative "worlds." While economists have discussed various ways in which inequality might be measured, philosophers have remained largely unaware of, or impervious to, their efforts. Temkin examines the work of the economists, but that is not his essential mission; rather, it is to apply a "Sidgwickian methodology" and the modus operandi of analytic philosophy to "explain, and ultimately guide, our egalitarian judgments," our "egalitarian intuitions" (pp. 5–6).

This mission—of determining when one situation is worse than another in respect to inequality—is a tricky, "complicated" business, one that requires scrutiny of competing principles of equality that have been promulgated by various schools of moral philosophy. These principles are often in conflict and thus lead to divergent assessments of how two competing "worlds" compare to each other. Temkin plays off these theories in myriad combinations, hoping to refine "our intuitions" about equality into "considered judgments." By distilling out equality from other desirable moral values, he hopes to understand it better. That understanding, then, can be factored into an "all-things-considered" judgment that takes other values into account. Only then can conclusions be reached about public policy.

Proceeding, as Sidgwick suggested, by beginning philosophical exploration with our ordinary moral judgments (both intuitions and underlying principles), Temkin seeks a coherent explanation of these "pretheoretical egalitarian judgments" that will enable us also to refine—and, where necessary, revise—these initial intuitions. He opens with a definition of who an egalitarian is—someone who cares about equality in itself, over and above its role in promoting other values. Excluded from the fold are some self-professed egalitarians whom Temkin calls "humanitarians." They are excluded because, while they favor transfers from the better-off to the worse-off, their sole objective is the improvement in the lot of the worse-off. Thus, they would be indifferent as between a regime of redistribution that took some "welfare" from the rich and gave it to the poor and a regime that made the poor and the rich both better off.

Inequality's intended audience—whose intuitions he counts on-is the "pure," "extreme" egalitarians-a by-no-means-insignificant cohort among today's moral philosophers. Their shared intuition, he argues, is that 'undeserved inequality is always objectionable; whether or not it is unavoidable, any one is responsible for it, there is anyone for whom it is worse, or it involves different people, societies, places or times (p. 12). Temkin states that he will not defend this position: his purpose is elucidation, not justification. Other intuitions follow. It is a natural injustice for some to be born blind and others not: it is simply unfair. It is a social injustice for some to enjoy more welfare (left undefined) than others. Something needs to be done to remedy both sorts of injustice, because the social builds upon the natural. Those who are worse off have genuine complaints, and it is their complaints that matter. Temkin will try to measure and compare these complaints in various worlds.

Temkin's "impersonal teleological view of inequality" leads him to explore alternative "worlds" all exhibiting the same three basic characteristics: (1) the better-off are not responsible for the inequality that exists, either directly or through resistance to sharing their good

fortune with the worse-off; (2) people are equally skilled, hardworking, and morally worthy; and (3) those who are worse off are in that position through no fault of their own. This should sound familiar; for Rawlsian counterfactual, "original position" theorizing has permeated moral philosophy in this country for over two decades.

With few exceptions (for cases of deserved inequality, which are acceptable but left undefined), these are worlds in which no one deserves anything, including talents and attributes. It is a universe in which useful things can be said about equality by comparing pairs of worlds, each represented by two rectangles varying in height depending on the welfare levels of two groups (the better-off, the worse-off) and varying in width by the number of people in each of the two groups. A goodly portion of the book is devoted to an examination of a sequence of 999 worlds that begins with a first world of 999 individuals who enjoy a high standard of welfare (left undefined but not just utility and not just measured in monetary terms) and one person who experiences a low standard of welfare. The world that occupies the median place in the series has 500 people at high welfare and 500 at low. The final world has one at high welfare and 999 at low. The question, to be probed at great length and in exquisite complexity is, Which world is better in "respect to inequality"? Does this sequence go from bad to good (because we are more attuned to inequality when it affects an individual or a small group), from good to bad (because some of our intuitions lead us to be more offended when more people are worse off), or from good to bad to good (because the first world deviates only slightly from equality, the middle world is the worst, and the progression from midpoint to the final world is on the upswing toward nearly perfect equality), or are all 999 worlds equivalent?

In examining these competing explanations of the sequence, Temkin considers "at least twelve different aspects or positions underlying or influencing our egalitarian judgments (there may be more)" (p. 50). It is a testament to the author's diligence that he kept count. The reader is treated to a richly textured (or stultifying, depending upon one's tastes) exploration of the interplay among a maximin principle of equality, an additive principle, a weighted additive principle, a relative-to-the-average-view-of-complaints principle, a relative-to-the-best-off-person principle of complaints, and a relative-to-all-those-better-off principle of complaints, among others.

Temkin makes few absolute judgments, preferring a "some people might oppose/many would claim" approach. However, the judgments that he does make seem dubious at best. Since he does not provide knockdown arguments in defense of any of his judgments, they are difficult to criticize and even more difficult to accept if one does not share the author's initial intuitions about equality. Some of these judgments are that if improvements to the better-off can be made without worsening anyone's welfare, this would be bad, whereas worsening the better-off without aiding anyone else would be good (p. 87); that egalitarianism is comparative, so that an egalitarian has "no intrinsic concern with how much people have . . [but] with how much people have relative to others" (p. 200); that the slogan "One situation cannot be worse (or better) than another if there is no one for whom it is worse (or better)" deserves to be seriously challenged; that in the 999-world sequence, the worlds first get worse then better; and that for any two reciprocal worlds in the sequence, "that one

will be worse whose better-off group is bigger" (p. 297, my emphasis). The last claim means that the last world is better than the first: it is preferable to have 999 people at low welfare and one at high than 999 well-off and one not.

These last two claims about the sequence are "pretty uncontroversial," Temkin thinks, and he simply "assumes that they are correct"; this lack of argumentation at such critical junctures is rather maddening in its own right, but even more so when, on the very next page, in an example supposedly illustrating the claims, one reads that "between reciprocal worlds, that one will be worse whose better-off group is smaller (pp. 297-98; my emphasis). This contradiction is undoubtedly the result of inadvertence; but there is a deeper problem here, namely, that one reader at least cannot state with conviction which view Temkin endorses—and this discussion is in a concluding section labeled "Practical Implications." Here, too, Temkin seemingly disregards his own repeated imprecations that "some of the conclusions reached may not be generalizable to the real world" (p. 29) and proceeds to generalize without any indication of how equality ought to be weighed against other, often competing, moral values. Thus, we need to rethink our quest for equality of opportunity, because it will lead to greater inequality by producing a meritocratic society with the talented of all races and genders winning the social, as well as the natural, lottery

Inequality concludes with a declaration that our "pretheoretical intuitions need revision in light of moral theory" (p. 302). Yet one is left with the nagging thought that perhaps it is the moral theory itself that needs revision. For nonegalitarians, the vexation goes even deeper, to the very intuitions that undergird the entire project. Why set up an idealized, counterfactual world in which no one is responsible for anything, no one deserves anything? (When economists theorize from an abstraction of human nature, the model, representing people as self-interested maximizers who know their own interests and prefer more utility to less, is at least recognizable. That is why the deductions of economists bear a relationship to how people actually behave in markets. Beginning from counterfactual assumptions, it is also not surprising that Temkin's mission, as it progresses, moves further and further from reality.) Why assume that what matters to people is comparing their welfare to the welfare of others? Why model the world in a way that gives central position to complaints, that is, to envy? Why should we care about equality in itself, irrespective of human welfare?

Political scientists, I suspects, will find this book bewildering. Particular aspects of the book will concretize our dismay. Chapter 3 is one example. The author conducts an "informal poll" by sending a questionnaire to 40 colleagues of his at the National Humanities Center, receives 10 responses, and then defends the sample as sufficient because the results cohered with those he achieved in the balancing-of-the-twelve-principles chapter. Chapter 10 provides another example. In discussing how our world relates to the worlds in the sequence, the author argues that our world resembles one of the later worlds, and to demonstrate this he cites sets of figures on inequality that are between two and three decades old. In an aside in the accompanying note, this lapse is explained: "Though . . . the particular data cited, is now somewhat dated, the basic pattern of inequality it suggests is unfortunately, not" (p. 302).

These two examples illustrate, one supposes, why philosophers are philosophers and not social scientists: all of the former's instincts draw them to the abstract, the otherworldly, the metarealm; all of the latter's inclinations lead them to the concrete, the empirical, the measurable (or at least, the observable). Thus, what precedes is the rather skeptical—some might say unduly harsh—assessment of a social scientist.

Bowling Green State University ELLEN FRANKEL PAUL

From Politics to Reason of State: The Acquisition and Transformation of the Language of Politics, 1250–1600. By Maurizio Viroli. New York: Cambridge University Press, 1992. 331p. \$54.95.

In 1612, Trajano Boccalini took a step beyond Giovanni Botero to do what would have been shocking and unspeakable to Brunetto Latini in 1266 or even to Niccolo Machiavelli in 1512. He gave to "reason of state"—that insidious art of preserving the state of a prince—the good name of "politics." To these earlier writers, the word *politics* picked out the art of ruling a republic according to justice and reason—however necessary it might be, given Italian realities, to read or write those very different discourses on the means of preserving a state. A revolution in "politics" had plainly occurred. Hereby hangs a tale of ideological innovation and conceptual change told with considerable erudition and detailed scholarship by Maurizio Viroli.

Across four centuries of Italian writers, Viroli narrates the acquisition and transformation of the language of politics. He begins in the late thirteenth century, when the linguistic traditions of political virtue, Roman civil law, and Aristotelianism provided writers on city government the semantic resources to conceptualize politics as the art of the republic. In the next century, the civic humanists completed the elaboration of the idea of politics as civil philosophy, glorifying it as the most excellent human endeavor. In their different ways, Machiavelli and (especially) Guicciardini gave unprecedented theoretical power to the upstart art of the state, but without conflating this art with the earlier and more noble one to which they (especially Machiavelli) also contributed. The last glimmerings of civil philosophy are seen in the later sixteenth century at precisely the time that the art of the state was elevated to reason of state and, soon enough, to politics itself. Attempts to restore the noble art of the republic proved impossible or in vain. The triumphant reason of state made of it "a language of nostalgia or utopia": "The story ends where it began: the same country where the language of politics as civil philosophy was born, nourished also the growth of its mortal enemy and grave-digger" (p. 280).

From Politics to Reason of State is an exemplary history of political thought and case study in conceptual change, executed roughly along lines charted out by Quentin Skinner, to whom the book is dedicated. Bidding us to leave aside our contemporary mental habits and linguistic usages, Viroli painstakingly excavates earlier languages of politics and the state through close textual readings informed by an impressive knowledge of different conceptual contexts. An imposing list of theorists (most of them heretofore unsung) is analyzed with care and clarity. Indeed, most readers will find (or take it on good faith) that the list of theorists is exhaustive; and

only a few will find it a little exhausting. Besides its intrinsic merits, the story that Viroli tells also provides him with the material in the epilogue "to prefigurate a conception of politics to which it is worth committing ourselves" (p. 10), namely, a restoration of civil philosophy itself.

In an otherwise excellent chapter on Machiavelli as master of the arts of both politics and state, Viroli sides with the view that the purposeful author of *The Prince* was attempting to educate "the one great political man" and perhaps engaging in a bit of "self-promotion," to boot (pp. 153, 177). But the case for this is less well argued than it should be, given the detailed linguistic work Viroli has put on display. The Medici lord to whom Machiavelli dedicated and submitted his treatise was not a "political man" (in his sense): he was a corrupt grasper after his state. Viroli notes how the advice on patronage is "unconventional" and "deliberately distorted," whereas that on the utility of fortresses is downright "subversive" (pp. 149, 151). Yet Viroli takes little advantage of his own tempting remarks, even after he goes on to acknowledge that in the History of Florence, Machiavelli disguisedly remained the "implacable opponent" of the Medici (p. 168). Whatever linguistic refinements he made on the concepts of politics and state, what Machiavelli the political man (in his sense, and ours) was doing in writing The Prince is still rather a mystery.

Mysterious, too, is how we can actually restore civil philosophy in our time. Having presumably been buried by reason of state in the seventeenth century, at least in Italy, civil philosophy's twentieth-century resurrection as "the art of the city" is a bit breathtaking. What sort of art, in the era of economics and the policy sciences? What sort of city, in the era of nation-states and transnational corporations? What will make civil philosophy sound like more than a few nostalgic sentiments about justice and friendship? Given such questions, it is surely very "strange" (as Viroli admits) to hear that such an art might form a discursive alliance with "the heirs of Kant and Mill," rather than "the contemporary advocates of republicanism" (p. 192). A historically informed civil philosophy may indeed provide rhetorical and theoretical resources to criticize statists, realists, participatory democrats, and communitarians. But one is tempted to ask exactly how civil philosophy and modern liberalism can cohere, cognitively and practically. After Viroli's careful linguistic work, one is tempted to ask how they can cohere "politically," as well. Worthy as the goal is, saying so will not make it so. Everything remains to be worked out. In the meantime, however, there is no doubt that "politics" in early modern Italy has received a first-rate chronicle. The memory it revives may yet assist the restoration of civil philosophy in our time.

University of Minnesota

JAMES FARR

The Problems of a Political Animal: Community, Justice, and Conflict in Aristotelian Political Thought. By Bernard Yack. Berkeley: University of California Press, 1993. 309p. \$45.00 cloth, \$14.00 paper.

Bernard Yack's new book is an incisive and well-written meditation on the inescapable conflicts that define human communities. Unfortunately, his title is a bit misleading, since the word *problems* implies that there

are solutions, whereas the whole point of his book is to show that the tensions and dilemmas of communal living are unresolvable. Moreover, his concern is not limited to political communities but includes families, friends, business partners, and even the passengers on a ship. Yack develops an Aristotelian theory of moral and political life in which conflict is seen not as the breakdown-but as the expression-of communal bonds. What this means is that the intensity of conflict is proportional to the closeness of the bonds—which helps to explain why, for example, police officers are often terrified of domestic disputes. Yack uses this dialectical unity of conflict and community to explore a variety of major themes in Aristotle's thought, including teleology, friendship, justice, the rule of law, class conflict, and the good life. In every case, his mode of analysis brings valuable and interesting aspects of Aristotle's thought into focus.

Yack carefully positions his argument in relation to individualist and communitarian interpretations of political community. Indeed, his book is as valuable as a guide to the current interpretative debates about Aristotle as it is to the writings of Aristotle. Against the individualist view that community is merely an instrument for pursuit of an individual life plan, Yack argues that we are by nature communal animals, whose identities are largely the product of our social relations. But against the communitarians, he insists that community is not the locus of harmony and integration but of conflict and division. And against perfectionism of all stripes, Yack insists that our conflict-ridden communities are an inexorable part of the human condition: there are no transpolitical ideals toward which politics can progress. To use Oakeshott's image, the only anchor available to the ship of state is a "sea" anchor. The domains of conflict may change over time but not the nature of conflict. Finally, against all kinds of nostalgia for the alleged harmony of the Greek polis, Yack insists that we are fundamentally in the same contentious political boat as Aristotle's Athenians (which is why Aristotle's relections on politics never lose their relevance).

Yack's Aristotle might be called a "Hobbesian communitarian" because of his paradoxical emphasis on both conflict and community (though this is equivalent to saying that Aristotle is neither a Hobbist nor a communitarian). According to Yack, the debate between individualists and communitarians remains wedded to the superficial contrast of society (Gesellschaft) and community (Gemeinschaft): modern, or urban, society is the locus of exchange relations among competing individuals, while traditional or rural community is the locus of shared moral bonds. Drawing on the sociological insights of Durkheim and Simmel, Yack blurs this contrast by showing that the exchange relations of society presuppose communal norms of good faith, while the shared moral bonds of community are often the product of exchange and other kinds of reciprocity. Aristotle did not need Durkheim, says Yack, to teach him that human community is built from the complementarity—not the similarity—of its members. Beginning with marriage, we form communities with individuals who are different from us, so that through division of labor and exchange, we may benefit from the pooling of individual talents. Who wants to go to a picnic where everyone brings the

But if difference is the precondition for community, it

is also the precondition for conflict. Because of our differences, we have different views of the ratio of contributions to rewards in the give-and-take of communal life. In the absence of a common metric by which to measure the different kinds of contributions to the common good, there is no calculus to determine the right distribution of rewards. (Yack does not evaluate the claims of libertarians that the market effects such a calculus.) The conflict over distributive justice is bitter to the extent that communal bonds are strong, for we are most likely to feel betrayed by those to whom we are closest.

Yack's emphasis on the conflictual dimension of political community is especially revealing in his analysis of class conflict. Yack (following Finley and others) laments the conflation of all class struggle with Marxian class struggle. He defines Marx's class struggle as conflict between "groups of individuals who are hierarchically positioned within the process of production" (p. 211). Unfortunately, this does not do justice to Marx's theory, because, on this definition, one would expect the class struggle of feudalism to be between aristocratic landowners and peasants, whereas, according to Marx, the struggle was between the aristocracy and the bourgeoisie. These latter two classes represent not "groups hierarchically positioned within the process of production" but declining and rising modes of production, or the country and the town. Still, Yack's own interpretation of class struggle (especially ancient Athenian class struggle) does not depend on his interpretation of Marx. Yack argues, on Aristotelian grounds, that class conflict is most common among citizens, not between status groups: in ancient Athens, masters and slaves rarely fought, because they had little in common, but rich and poor citizens fought often, because they had political life in common. Each class felt betrayed by the other class, with whom it shared the political arena. Political friends make for class enemies.

Because politics has always been so conflictual, there has always been a longing for an escape from politics—a "final solution" to the "problems of a political animal." Yack explored some strategies for such escape in his first book, *The Longing for Total Revolution* (1986). There are several possible escape routes. The first—the quest for communal harmony—we have seen to be chimerical. Another, according to Yack, is legalism—the attempt to

set forth a code of laws, principles, and imperatives, based on human nature or human reason, that purport to define what justice once-and-for-all requires. Yack's Aristotle defines politics not as an attempt to embody any set of abstract principles but as the conflict over those principles—principles that are "essentially contestable." Thus, Yack (following Harry Jaffa and others) strongly distances Aristotelian natural right from any kind of natural law. Yet he acknowledges a kind of procedural natural law principle of justice when he says, Nature thus tells Aristotle who can participate in deliberation about political justice but not what they should choose" (p. 149). Since nature endows human beings with reason and speech (in order to discriminate between the just and the unjust), by nature, all rational agents should be allowed to participate in politics. But Yack denies that Aristotle has any natural law principles to decide substantive questions of political justice: when it comes to such principles, we are on our own. Aristotle, in short (unlike Rousseau, Marx, and Nietzsche), never yielded to the temptation to suggest an escape from politics.

Aristotle says that in order to become a good person we need three things: nature, habit, and reason (Politics 1332a40; see also Nicomachean Ethics 1179b20). We begin with our natural aptitudes; these are then shaped into habits; and finally, we use our reason to reflect upon our habits and to form new habits. At the political level, excellence requires the right human nature (shaped by eugenics), the right customs, and the right laws. Habits or customs presuppose nature but cannot be reduced to nature, while reason presupposes habits without being reduced to habit. In their accounts of Aristotle's ethics, different scholars emphasize one or another of these three elements. Roger Masters, in The Nature of Politics (1989), emphasizes human nature; John Finnis, in Natural Law and Natural Rights (1980), emphasizes reason; and Bernard Yack emphasizes the social inculcation of habits. Each account is thus equally valid and equally one-sided. What is needed is an account of Aristotle's ethics and politics that not only includes nature, custom, and reason but also shows how each element is related to the others in Aristotle's dynamic model of human flourishing.

Dartmouth College

JAMES BERNARD MURPHY

AMERICAN POLITICS

Real Choices, New Voices: The Case for Proportional Representation Elections in the United States. By Douglas Amy. New York: Columbia University Press, 1993. 278p. \$32.50 cloth.

Restoring Real Representation. By Robert C. Grady. Urbana: University of Illinois Press, 1993. 170p. \$29.95 cloth.

As the titles of both books indicate, these works set out to make a case for improving representation. Although both do a fine job of laying the groundwork of their theses, they make a less than convincing case that their proposed reforms would in fact *improve* the quality of representation.

The key shortcoming of both books is that the authors overlook—or at least do not address—a key lesson to be drawn from Hannah Pitkin's *The Concept of Representation* (1967). Representation is a complex, multifaceted notion which has several manifestations which are not necessarily mutually compatible. Both Grady and Amy fail to acknowledge, despite the earnestness of their endeavors, that they have merely taken stands at and in favor of different points along a representational continuum. Thus, while both books' arguments for change provide ample food for thought, readers are likely to be left wondering if the proposed changes would produce the benefits that the authors contend.

Grady begins by lamenting the "displacement of popular representation in favor of interest group representation" (vii) which is symptomatic of interest group liberalism (p. vii). He seeks in this admittedly polemical work to "make participation and popular representation once again central to governance" (p. 5).

Contending that the current state of liberal democracy diminishes the value of popular participation and civic responsibility, he seeks to remedy this as well as the lack of government accountability that characterizes interest-group liberalism. To do so he develops a vision of political participation which is a hybrid drawn from the confluence of participationist theory and corporatism "by combining corporatist functional jurisdictions, which interact with public officials and complement the roles of elected legislatures, and participatory practices within such jurisdictions" (p. 8). While retaining the corporatist structure, he extends his participationist vision to include other interests or private associations all of which are grouped under his heading, democratic functional constituencies (DFCs).

If all of his conditions were met, Grady's vision would certainly lay a coherent theoretical groundwork for a system in which voters would be provided with more incentives and opportunities to participate meaningfully in governing and, as a result, become better citizens. In Grady's vision, the government would delegate authority to DFCs (which, by definition would be democratically run) and do so with strict guidelines which would limit the discretion that the DFC leadership could exercise in pursuing a given governmental end (p. 142). Such an arrangement would enhance the tie between the government and the people who participate in various DFCs while ensuring that those who worked with the

government would not behave in the egoistic manner that characterizes interest-group liberalism.

A key proviso is that such a delegation would, by definition, be in "the public interest." It is here that Grady's argument becomes harder to follow. Insofar as he distinguishes between the public interest and the desires of the organized interest groups, Grady seeks to ensure that these discrete interests would be subsumed by some greater public interest which would be determined by legislative deliberation and made manifest in the clear guidelines which would accompany any delegation to a DFC.

This begs the question: from where would these public-spirited legislators emerge? In part, Grady's answer is implicit: the beneficial impact of workplace democracy in DFCs would manifest itself in the voter's choice of legislators. However, Grady never really explains how citizens, who would apparently define their vision of the political in terms of their DFC activities, would separate their DFC agenda from the public good when it came time to elect a legislator. As well, Grady himself admits that, somehow, legislators and DFC leaders would need to be coaxed away from behaving as they do under interest-group liberalism in order to act instead in the name of some public interest. He does not, however, explain how feasible or likely this would be.

This begs a second question: How would the truly public interest be discerned? Insofar as the legislators' restrictions would be made clear in a delegation of authority, their vision of "the public interest" could be made manifest. However, since Grady does not define what would count as truly public-spirited legislative deliberation, he offers no measures for determining whether or not the legislature was actually behaving.

Clearly, Grady places a premium on participation as the groundwork for a more legitimate governmental process. Yet he does not address whether mere participation is in fact tantamount to having one's interest represented more effectively. Granted, Grady's work seems premised on the assumption that with time the public interest would become clear to all and that individual interests would succumb to or be included as part of it. In this respect, the notion of representation qua the voicing of or "standing for" particular and discrete interests would be meaningless since the public interest (or at least an enlightened self-interest) would, ideally, become a principal motivation for more and more political action.

Nonetheless, to the extent that legislative deliberation would be a key determinant of what the public interest would be at any given time, Grady's vision includes the possibility that even under his ideal conditions of public-spiritedness and participation, there would be competing views of the public interest. As a result, certain interests would lose out insofar as their elected representative might not agree with the legislative majority's deliberations.

Thus, to the extent that individual participation can be regarded as a surrogate for if not a replacement of interest representation, Grady's thesis is intriguing. Nonetheless, the question of whether mere participation

alone is sufficient to achieve satisfactory representation of interests remains an issue. This same problem also arises in Amy's discussion.

In seeking to make the case for America's conversion to proportional representation (PR) as a first step in curing the principal ailments of the political system, Amy begins with the assertion that "the American election system is unfair, outmoded and undemocratic" (p. 1). The system would improve if: gerrymandering were eradicated (chapter 2), campaigns were more issue-oriented (chapter 3), we broke the two-party monopoly (chapter 4), we elected more women and minorities (chapters 5 and 6) and more people participated in elections (chapter 7).

He explains that eradicating gerrymandering would minimize wasted votes. Issue-oriented campaigns would provide more meaningful choices to voters and PR would encourage parties to be more issue-oriented by removing the need to garner a majority of the votes in a district to win seats and legislative influence. Increasing the number of parties and increasing party discipline would also enhance the issue-focus and quality of voter choice. The election of more women and minorities would make legislatures more reflective of the population. And higher turnout would legitimate the government. This said, one cannot help but react with skepticism when Amy suggests that the root of evil is the single-member plurality system of voting (SMP) or that a conversion to some form of PR will solve all of these problems.

Proponents of PR have always contended that gerrymandering would become less of an issue because PR would create the possibility for multimember districts. It would also appeal to the aesthetic desire for a more accurate translation of votes into seats in any given election. The issue, however, of whether a conversion to PR would result in qualitative improvements in representation remains the source of an ongoing debate. Absent strong empirical evidence to support many of his contentions, Amy's argument in favor or PR remains quite speculative.

Amy himself admits at several points that his assertions are not grounded in empirical evidence. He argues, for example, that PR would lead to more issue voting (p. 70), yet he says forthrightly in an accompanying footnote that there is no empirical evidence to support this claim. Similarly, his reliance on Blais and Carty (1990) to sustain his assertion that PR would increase voter turnout is, as he points out, qualified by

the fact that they could not explain why countries that

employ PR systems tend to have higher turnouts than those with SMP.

Even if a conversion to PR were to bring about some of the changes that Amy suggests, whether or not it would result in representational improvements remains the focus of a debate that has continued for some time. While a more accurate translation of votes into seats in any given election might have a certain aesthetic appeal, PR does little for a minority if, having been shut out of legislative representation due to discriminatory laws, a particular electoral system or gerrymandering, it now has the privilege of seeing its interests or agenda defeated in the legislative bargaining process in proportion to it numbers.

Calhoun dealt with this over a century ago in his Disquisition on Government and Lani Guinier has echoed Calhoun recently in "The Triumph of Tokenism" (1991)

and "No Two Seats" (1991). Although both Calhoun and Guinier challenged the mathematical unfairness of SMP, both also acknowledged that mere proportional representation would do little more than establish a token presence in the legislature unless the minorities were given some sort of veto power which, for all intents and purposes, would give them *disproportionate* influence in the legislature. Amy, however, cites neither critic and, as a result, his contention that PR would be a panacea for all that ails SMP systems stands shaken.

Doubtless, Amy's book serves to reassert that there are indeed two sides to the PR-SMP debate, that both are grounded in thoughtful concerns about effective representation and that, perhaps, the debate still needs to be hashed out in the United States. In *Real Choices*, however, Amy does little more than cast his lot with PR's proponents. SMP may have its shortcomings, but it certainly has the support of many eminent scholars. He dismisses these concerns much too quickly. While devoting some 190 pages to advocating PR, he dispenses with the objections to it in a mere 30 pages. The result is that his approach to the issue lacks balance and the book reads more as a thoughtful polemic than an objective analysis.

In sum, both books are provocative, yet ultimately they fall short of their goal of setting forth convincing schemes to improve representation. Insofar as the representation debate has expanded beyond the notions of simply ensuring participation or obtaining a proportional presence (instead of some sort of proportional *impact*) in the legislative process, both Grady and Amy have added support to the respective aspects of the debate upon which their works focus. Both works, however, leave many questions unanswered.

Washington and Lee University

MARK E. RUSH

The Rebirth of Urban Democracy. Edited by Jeffrey M. Berry, Kent E. Portney, and Ken Thomson. Washington: Brookings Institution, 1993. 344p. \$36.95 cloth, \$16.95 paper.

This is an earnest book and a good one. The authors seem torn between the optimism about neighborhood-based participation that is reflected in the book's title and their responsibility as social scientists to provide solid data and evaluate it with measured skepticism. While Berry and his colleagues occasionally lead with their hearts, their book offers a rich data base and serious analysis anchored in important concepts about power and democracy.

Much of what has been written about neighborhood organizations in the past has been thick with ideological celebration (leftist visions of a mass movement against the prerogatives of capital or rightist salutes to the ability of citizens to help themselves without governmental aid) but empirically spare. The authors provide an antidote to that trend. Their central theoretical issue is whether expanding citizen involvement leads to frustration, disorder, and delay (a prediction they associate with Joseph Schumpeter and Samuel Huntington) or to more satisfied, knowledgeable, and civic-minded citizens (as would be predicted by Rousseau, Mill and contemporary communitarians). Their empirical focus is on five core cities (Birmingham, Dayton, Portland, St. Paul, and San Antonio) selected because preliminary research

showed them to have unusually inclusive and effective mechanisms for citizen participation. In each core city, the authors surveyed about 1,100 residents, then reinterviewed most of them about a year-and-a-half later. A single wave of surveys was also carried out in 10 comparison cities chosen to match the core cities in size, race and class. In addition, in each of the five core cities, the authors conducted interviews with from 77 to 106 administrators, council members, neighborhood and community activists, and other political elites.

Perhaps the most surprising finding concerns the relationship between structured participation channels and actual levels of citizen involvement. Using a broad measure of community participation, the authors found that citywide structured systems for citizen participation did not generate higher levels of activity than were found in the comparison cities. Indeed, one of the core cities, San Antonio, had substantially lower levels of participation than any of the others; Dayton and Birmingham also scored in the bottom five out of the 15 cities. This finding led the authors to restructure the rest of their analysis. Rather than simply compare the five core cities to the others, they categorized all 15 cities based on two dimensions: the overall level of citizen participation and whether participation mechanisms were formally structured. This shifting of gears makes the analysis muddier and less well defined in places, but it allows for some interesting distinctions and comparisons. For example, compared to the cities with unstructured participation, the five core cities tend not to do as well in stimulating involvement by low socioeconomicstatus and minority citizens. But there is some evidence that the structured cities have a more positive impact on the sense of political efficacy and levels of political knowledge of those low-income and minority citizens who do participate.

The authors' predisposition in favor of structured participation mechanisms reveals itself in their choice of a frame of reference. In much of the book, they are content to strike blows against the belief that expanded participation will lead to elite dominance, intolerance, conflict, parochialism, and governmental paralysis. They make a convincing case that these fears are not justified. Neighborhood groups tend to serve as a counterbalance to business interests, to teach participants to be more tolerant of others, and to stimulate a sense of community. Public officials and nonparticipation citizens see these groups as legitimate actors that facilitate negotiation and compromise and, ultimately, better policies. Some readers however, will feel that the authors have delivered most of their blows to a straw man. The fear that participation will unhinge democracy is less common today than it was in the 1950s or 1960s, and its focus has never really been on the kind of structured, formally recognized, genteel and civilized forms of participation that characterize four of the five core cities considered here. (San Antonio's genre of participation is more free-wheeling and contentious; on the indicators that the authors care about, however, it does not perform nearly as well as the other cities)

A more reserved reading of the evidence they present might lead to a less exuberant endorsement of neighborhood associations than Berry, Portney, and Thomson end up with. Some of the findings they cite to discredit the notion that participation is destabilizing can also be read as evidence that these elaborate structures do not have very much impact one way or the other. They find

little evidence that neighborhood groups are a positive force in setting the local agenda; and they concede that neighborhood associations can rarely stand up to business when it comes to development issues of any size or of citywide consequence. Because they make no effort to quantify the cost of sustaining these institutions, they are in no position to conclude whether the small positive effects they uncover represent a decent return on the investment. Given the fact that they began their study by selecting cities that seemed to be models of success (the authors acknowledge that there were "few other cities that even came close to the five cities in the scope of their neighborhood-based citizen participation" (p. 294) the absence of more striking differences between the core cities and the comparison group is instructive.

It is premature to declare that the establishment of citywide, neighborhood-based structures for participation will bring about a rebirth of urban democracy. But this book is sufficiently sound and sufficiently stimulating to lead us toward a rebirth of interest in the topic as a focus of public debate and as a subject for scholarly investigation.

George Washington University

JEFFREY R. HENIG

Inviting Women's Rebellion: A Political Process Interpretation of the Women's Movement. By Anne N. Costain. Baltimore: Johns Hopkins University Press, 1992. 188p. \$28.00.

In less than 200 pages, Ann Costain analyzes the contemporary women's movement from roughly the 1960s through what she describes as its crescendo in 1975 to its current state of apathy and decline. She uses interviews and secondary archival research, as well as content analysis of news stories and official documents. The author is to be commended for integrating political process theory from sociology into political science. More specifically, she has made noteworthy contributions to the literature in establishing linkages between external political forces and ultimate governmental response.

Costain argues that the debate over what constitutes a social movement can be isolated into three competing schools of thought; mass society, resource mobilization, and political process. She sets the stage for her study by informing the reader of the theoretical development of the political process approach, which, according to Costain, grew out of resource mobilization theory. However, as opposed to resource mobilization theory, which concentrates on resources (e.g., entrepreneurial organizers), the political process approach places more emphasis on the political system, "indigenous accumulation of resources, and psychological aspects of movement identification" (p. 12).

The mass society theory, which characterizes William Kornhauser, Philip Seiznick, and Hannah Arendt's research into early twentieth-century European industrial societies, was according to Costain, applied to American politics in the early 1950s by Seymour Martin Lipset, Daniel Bell and Richard Hofstadter and, still later, by Ted Gurr in his *relative deprivation* variant. Costain then goes on the trace the development of resource mobilization theory formulated by sociologists in the 1970s to challenge mass society theory and relative deprivation (p. 6). According to Costain, resource mobilization the-

ory suggests that the principal problems confronting most movements can be traced to organization and the task of starting up while concurrently increasing new membership and retaining existing members. Costain cites the work of Robert Salisbury and his ideas about exchange in identifying ways that movements succeed. She feels that most research to date on the women's movement has adopted this perspective. She references the work of Gelb, Palley, Mansbridge, Conover, and Gray as being premised on resource mobilization. Costain also points to Jo Freeman's research, using relative deprivation, as work that also linked resource mobilization theory, in that it combined Gurr's work (women comparing their jobs and social prospects with men of the same age and educational background felt unfairly treated) with an early emphasis on the resources of communication networks needed to organize and spread information about the women's movement (p. 6). The author argues that the major implications of resource mobilization theory, as applied to the women's movement, are that the movement emphasized its nonthreatening characteristics (appearing politically innocuous) and that an early accumulation of resources followed from this focus (p. 9).

Costain feels that resource mobilization theory fails to explain early issues surrounding the women's movement, such as the public protest of the movement, the concerns of the early lobbyists for women's rights in the late 1960s and early 1970s, the larger questions of why women actually organized a social movement, the movement's rejection of hierarchical leadership (thus eliminating the most obvious means to accumulate resources), and the lack of large numbers of supporters of

the movement in its early years.

On the other hand, Costain posits that a political process theory can more accurately explain the development of the women's movement. In this approach, for a movement to emerge, its followers "must believe that problems have political roots and that they can do something to change existing conditions. . . . Most important of all, the government must at least be willing to tolerate the movement's appearance" (p. 12). Costain goes on to build on the work of Ethel Klein (Gender Politics) and others to test the two approaches empirically. Costain does so by coding events reported in the New York Times Index for the period 1950-86, contentanalyzing other secondary sources (papers of the National Organization of Women [NOW] and WEAL, the Congressional Record U.S. Statutes at Large) and interviewing lobbyists for women's groups and members of Congress and their staff. Her interviews spanned intermittent periods from 1974 to 1984. She uses discriminant analysis to assess how much influence region, political party and liberalism had on the individual decisions of members of Congress about whether to sponsor the Equal Rights Amendment (ERA). This was performed for each of the five Congresses in session (the Eightyeighth through the Ninety-Second).

Costain addresses such complex issues surrounding the women's movement in order to include the disunity among women's groups, the pro- and anti-ERA backers, the influence of such nationally organized groups as NOW and NWPC, the concurrent weakening of the New Deal electoral coalition and politicians' recognition of the female voting power, the influence of the chief executive and his leadership in the movement, and the downfall of the ERA and success of the Reagan administration in moving women's rights from an active to a largely symbolic agenda item (p. 120).

The author concludes that as women's groups marshalled their strength, Congress took the lead in boosting the legislative fortunes of the ERA: "As the ERA headed to the individual states in the fight for ratification, it continued to provide the prime catalyst that united and energized the women's movement" (p. 78). She argues the peak of the women's movement coincides with the high point of media attention given to the ERA and a sharply concentrated focus on legal equality as a solution to women's concerns. Costain feels that these equality issues triumphed over the alternative agenda emphasizing special needs, or needs that government should address because of biological, physiological, and familial differences: "This new predominance of equality issues over special needs was less a choice of the women's movement than of the legislative and executive branches of government. . . . However, the increasingly single-minded focus on the ERA set the stage for a decline in the women's movement visibility in the 1980s" (p. 99).

Costain recognizes the problems inherent in establishing linkages between external political forces and ultimate governmental action as a result of this political pressure: "Discerning the balance between government response to outside pressures and internally generated initiatives on women's issues is more difficult than it first seems" (p. 42). She also recognizes the problems that scholars have encountered in using content analysis as a method for establishing this linkage. For example, she mentions the bias in the higher probability of ' news," including demonstrations, riots, and other events to the exclusion of "soft news," including political speeches and other more analytical exchanges. However, she does not fully explain her motivation for excluding editorials, letters to the editor, comments, and other opinion-type pieces. She does point out: "Most scholars who employ events data exclude from event totals all editorial or other New York Times-generated stories, such as retrospectives on the movement or Sunday magazine articles. That practice was also followed here to cut down on the extent that New York Times' editorial judgments might skew the results" (p. 15–16).

However, since Costain relies primarily on analysis of secondary sources for much of her interpretation of the movement and for her efforts to link newspaper accounts and other documents to the political process, it would seem that some sort of analysis of editorials and thought pieces to accompany her events analysis would have enhanced, and added further credibility to, her study. This would especially appear to be the case in establishing linkage between the key political players she interviewed, what the newspaper was concurrently reporting, and the ultimate fallout from the political process. For example, would not the editorials and other opinion pieces have further reflected the movements' success and/or failure in setting an agenda item for government leaders to consider? This would seem to be the case especially because large numbers of governmental leaders acknowledge the influence of the New York Times. For example, the question arises, "Was the ERA the leading issue reported by the New York Times because of its significance for equality for women in our society or because the news media tend to focus on events, as opposed to issue analysis?" (see p. 79). Also,

era.

especially with regard to the linkage aspect, it would appear that her study would have withstood closer scrutiny had Costain also related her findings from her content analysis more specifically to the information gleaned from her interviews. For example, how were members of Congress and lobbyists specifically influenced by the events reported at the time? What role did they see themselves playing in the women's movement? What were their specific observations at the time with regard to equality for women? I wish Costain had used her interviews extensively to establish clearly just how the government facilitated the movement's success and, specifically, how pressure from various groups brought about this political change.

Nonetheless, Costain's research answers more questions than it leaves unanswered. Her work is clearly organized and written, and she makes a significant contribution to the existing literature. Finally, her research succinctly portrays the successes of the women's movement while summarizing valuable lessons learned along the way. This should be invaluable for future pursuits geared to improve the legal, social, cultural, economic, and political well-being of the American female.

Clemson University

LOIS LOVELACE DUKE

Legislative Leviathan: Party Government in the House. By Gary W. Cox and Mathew D. McCubbins. Berkeley: University of California Press, 1993. 324p. \$45.00 cloth, \$14.00 paper.

The conventional wisdom is tenacious. Political parties are weak in the United States. Indeed, congressional politics can be understood without factoring party cleavage or polarization into the analysis because the parties in Congress have declined. Or, congressional parties can simply be ignored, omitted from institutional analysis by assumption or stipulation because they are so ineffectual. Congressional politics are "committee-centered," gravitating toward autonomous committee work groups providing "subcommittee government" to the institution. Party voting is paltry, and in the event reflects merely the inexorable influences of largely incidental ideological or attributional commonalities among constituencies. Party organization is trivial, and party leaders are really only followers of the central preferential tendencies endemic within their party cohorts. Representatives care not for loyalty to their party, but only attend to their reelection the next time around. Put in its baldest form, the conventional wisdom about Congress truly is perverse.

Cox and McCubbins charge into this arena with genuinely stimulating argument and analysis. They argue that both the "traditional" and the "rational choice" views of congressional party politics are deficient. The traditional perspective overworks the extent of committee and subcommittee devolution of power, and underplays the coordinative leadership of congressional parties. The rational choice view, all too often, assumes political parties out of the model altogether. In contrast, say Cox and McCubbins, a careful analysis of the committee structure permits a variety of inferences indicating the existence of party government in Congress.

The authors' work entails manipulating an impressive variety and quantity of data. Their purview for testing

claims rests upon the House of Representatives. Most heavily relied upon are data for members' committee assignment and transfer requests from the 80th (1947-48) to the 100th (1987-88) Congresses. Ideological assessments are drawn from Americans for Democratic Action (ADA), Americans for Constitutional Action (ACA), and other interest group ratings developed from House roll-call voting. In addition, the authors make use of Congressional Quarterly's conservative coalition support scores, and the so-called NOMINATE scores (undimensional scale scores derived from all non-unanimous roll-call votes). A piece of the analysis relies upon four decades of congressional election data (1948-1988). The authors also use House roll-call voting from 1933 to 1987 to construct party leadership support (loyalty) scores for members for each biennium. Finally, selected data were gathered on bill sponsorship, and on majority and minority opinions in committee reports. To make a long story short, Cox and McCubbins' findings rest on analyses of substantial and impressive sets of data.

The principal findings from this research can be encapsulated blow-by-blow. (1) The House committeeassignment process does not appear to be one in which pure self-selection transpires, and there is plenty of room in selecting committee memberships for congressional party criteria and purposes to come into play. (2) Most House committees have memberships reasonably representative of the House as a whole in ideological or geographical terms, so most representatives "do not serve on committees that are dominated by preference outliers" (p. 79). (3) Analysis of the incidence of party voting in the House since the New Deal era of the 1930s indicates a shrinkage in party policy agendas, along with considerable stability in support among partisans for the majority (Democrats) party leaders' stands on policy issues and goals. (4) Party leaders demonstrably take party loyalty into account in making the committee assignments of freshmen representatives, and in determining the transfer of members from one committee to another. (5) That the majority party dominates committee deliberations is evidenced by patterns of bill sponsorship and dissents from committee reports on bills; majority party members sponsor most major legislation and dissent from committee reports far less frequently than minority members.

Cox and McCubbins paint a picture of congressional politics much different from the conventional view. To these authors, the congressional parties have not declined in the post-World War II years. Representatives come to Congress from a vortex of partisan electoral experiences. Intra-party ideological affinities are substantial. Members are pervasively loyal to their party leaders. The party leaders can exert considerable leverage over committees because they have "structured" the committee system itself, committee behavior tends to reflect partisan loyalties, and the party leaders tend to shape the legislative agenda. Indeed, the authors think that, in the House, "the majority party acts as a structuring coalition, stacking the deck in its own favor—both on the floor and in committee-to create a kind of 'legislative cartel' that dominates the legislative agenda' (p. 270). As in Hobbes's Leviathan, the governing representatives resolve collective dilemmas by establishing a central agency or authority, in the instance of the house by electing and following their party leaderships

Why have the congressional parties become more important in the last two decades? Cox and McCubbins

ask rhetorically, "Did parties, like Lazarus, rise from the dead?" (p. 277). They contend that "the parties never died . . ." but rather "that we have had a version of 'party government,' in the House at least, throughout the postwar period" (p. 277). But they also argue that, by the 1970s, the amelioration of the stalemate between northern and southern wings of the House Democratic majority—precipitated by the reduction in numbers and liberalization of Southern Democrats—permitted a series of decisions that empowered the majority leadership. Indeed, Cox and McCubbins argue that the committee reforms of the 1970s were understood by majority party leaders as a way to enhance their control of the legislative agenda.

I find this book enormously refreshing, even liberating. The authors begin the task of restoring political party politics and party leadership to their proper place in analyzing congressional life. The theoretical orientation advanced by the authors is a rational choice perspective, but the findings and implications of the analysis do not, in the end, depend upon this. Indeed, sometimes the rational choice lingo and formalization are more an interruption than illuminating. One chapter on collective dilemmas in general is very tedious, and in a few other places the argument degenerates through excessive abstraction. Beyond the authors' formalizing pretensions, the substantive argument will be vigorously debated by conventional congressional scholars who will cling to the "weak parties" or "no parties" assumptions and who can find allusions to "leviathan" and to a "legislative cartel" egregious, and by rational choice purists who may find that relaxing the ceteris paribus assumptions to which they have grown accustomed is a sop to institutional verisimilitude. But, on balance, I think this book is an important landmark on the road to a more interesting, more political, and a theoretically invigorating line of research on congressional politics.

Center for Advanced Study in the Behavioral Sciences and Ohio State University SAMUEL C. PATTERSON

At Women's Expense: State Power and the Politics of Fetal Rights. By Cynthia R. Daniels. Cambridge: Harvard University Press, 1993. 183 p. \$19.95.

Daniel's well-written and cogently argued book addresses the important issue of fetal rights outside the narrow context of abortion, where it is too often exclusively situated. Like Zillah Eisenstein (and Robin West), Daniels centers our focus on the pregnant body to expose the inadequacy of the liberal concept of "citizen" which is gendered male. Daniels carefully examines three legal cases to challenge the ability of classical liberalism's notions of individualism, privacy, and self-determination to defend women's rights during pregnancy.

Chapter 1, aptly entitled "Fetal Animation," traces changes in technology, rhetoric, and law on fetal rights. Each of the next three chapters spotlights one case. The 1987 case of Angela Carder erupted when hospital administrators sought and won a court order to perform a cesarian section on a woman dying of cancer over the objections of the woman, her doctor, and her family. Unlike the episode of *L.A. Law* based on the case, both

mother and fetus soon died. After reviewing the emergence of the fetus as a separate patient, Daniels draws on cases that similarly resulted in a violation of women's right to bodily integrity. Daniels reveals that only when women's commitment to motherhood is unquestioned, when women align themselves with powerful male organizations (in this case doctors), and when women can "degender" the threat to bodily integrity (in this case, constructing the issue as one of patient's rights) can they prevail. Carder's "success" consisted of a circuit court decision affirming the right of a patient to decide and the development of professional guidelines.

Daniels uses the case of United Autoworkers v. Johnson Controls to show how employers use gendered assumptions about women's reproductive capabilities and appropriate work to disadvantage them in the workplace under the guise of protecting their offspring. Johnson Controls "gender-cleansed" premenopausal nonsterilized (thus "fertile") women who worked in battery manufacturing from jobs that exposed workers to lead. Daniels brilliantly demonstrates how employer's rhetoric first creates a fissure between maternal and fetal interests and then blames women for adverse reproductive outcomes-shifting the circle of causality away from fathers or the hazardous work environment. Fetal protection policies construct employers, rather than women, as the rational protectors of the fetus. The Supreme Court's unanimous decision in 1991 declaring Johnson Control's policy in violation of Title VII of the Civil Rights Act of 1964 marks a shift from resolving ostensive conflicts between work and motherhood always on the side of motherhood.

Chapter 4 ("The Politics of Vengeance") explores the 1989 case of Jennifer Johnson, the first woman in the United States to be convicted for delivering drugs to a minor by "delivering" cocaine to her newborns through the umbilical cord. Daniel uses the Johnson case to analyze the myths about the pregnant addict in public discourse and to bemoan our collective need to blame individuals for social problems. She methodically considers the medical evidence generating the hysteria over "crack babies" and the role of the media in this episode. Even more so than "fertile" women, the pregnant addict is more easily constructed as irrational. By showing that black women are 10 times more likely to be prosecuted for drug abuse during pregnancy, for example, Daniels carefully documents how class and race affect constructions of motherhood and determine when state intervention is accepted as appropriate. The analysis in this chapter is superb and merits excerpting for those unable to use the entire work.

Those who teach law and society, women and the law, women and politics, feminist political theory, public policy, or even introductory women's studies courses will cheer the publication of this book. Daniels's prose is unusually clear, readable, and accessible to those with little background knowledge. Its length also makes it a suitable text or introduction. A virtue of the book is its crossing of legal subfields: it considers cases in constitutional, criminal, family, and labor law. Daniels's work deftly illustrates the dividends of employing gender as a conceptual tool to disrupt settled legal divisions. Unlike other legal scholarship, Daniels eschews the excessive legal citations and references to other scholars that often overwhelm the uninitiated reader.

This virtue is also a limitation. Daniels adds no new information to the stories of the legal cases. Nor does

she expand our understanding of legal doctrine. Her analysis is cursory about the social forces that produced these cases and the choices and strategies of the players (plaintiffs, interest groups, legislators, etc.) The book illuminates different aspects of the public policy process from those found in her previous scholarship. To regard these omissions as flaws, however, would be to miss the purpose and importance of the book. As a political theorist, Daniels employs careful analysis of contemporary cases to interrogate liberal individualism and rethink what constitutes citizenship. As a feminist theorist, she meets the challenge posted by historian Joan Scott to understand difference in a more sophisticated way and to unpack the gendering of social issues and concepts. She impressively meets the need to understand gender, race, and class as mutually constituting categories of oppression. In fact, the concrete illustration of this theoretically complex concept is one of the main strengths of the work.

Daniels's terse argument is unlikely to persuade those who do not share her assumptions about women's rights and reproductive choice, but that is not her aim. To those who feel discomfort when doctors perform cesarian sections over the objection of the woman, are aghast that women must be sterilized to hold a job, and look with unease on criminalizing pregnant addicts, this book powerfully analyzes each issue and brilliantly shows the connections between them. Her work is a major contribution to the emerging literature on this important topic.

University of Iowa

SALLY J. KENNEY

Regulatory Politics in Transition. By Marc Allen Eisner. Baltimore: Johns Hopkins University Press, 1993. 246p. \$45.00 cloth, \$14.95 paper.

One of the most exciting recent developments in political science is the attention devoted to historical analysis—the *rediscovery* of history, if I may be permitted a mild exaggeration. Clearly, the dominant and, to date, the most fruitful approach within the politics and history movement is institutional development. The pioneering work of Stephen Skowronek and Theda Skocpol laid the foundation for a burgeoning interest in the theoretical connection between institutional development and the broader evolution of political systems. Eisner's new book, *Regulatory Politics in Transition*, is a contribution to this literature.

Eisner presents us with a historical-theoretical essay that seeks to make sense of shifts in regulatory policy and politics in the United States. Drawing directly on Harris and Milkis's Politics of Regulatory Change (1989), Eisner sees regulatory politics as evolving from one regulatory regime to another. The major conceptual distinction between the two works is the relative emphasis placed on ideas in the emergence of regulatory regimes. Thus, Eisner defines a regulatory regime as "a historically specific configuration of policies and institutions which structures the relationship between social interests, the state, and economic actors in multiple sectors of the economy," while Harris and Milkis he notes, define the concept as "a constellation of (1) new ideas justifying government control over business activity, (2) new institutions that structure regulatory politics, and (3) a new set of policies impinging on business" (p. 1). Eisner

does devote considerable attention to ideas but does not accord them a primary role in explaining regulatory regime transition. Rather, the main trajectory of development runs essentially along the following path: economic change stimulates social groups' policy demands, which in turn give rise to a policy and reform agenda, out of which emerges new regulatory institutions and new policies. This process purportedly explains the emergence of four regulatory regimes in American history: the Progressive Era and the New Deal, both of which most scholars acknowledge without benefit of the regime framework, the societal regime of the 1970s, and the efficiency regime, which Eisner introduces to account for the Reagan and Bush administrations' response to economic stagnation and sharply increasing regulatory costs.

The major strength of *Regulatory Politics in Transition* is that it systematically assesses a defining element of the modern American state, namely, regulatory policy. Eisner correctly notes that the relationship between government and business provides an important window on political development. In addition, he strives to explain the unfolding drama of regulatory transition, as well as the roles played by the major institutional and organizational actors. In that enterprise, he invites us to examine critically the explanation he provides. Laudable as that enterprise is, it falls somewhat short, primarily because the framework of analysis weakens in its explanatory power as Eisner moves from earlier to later transitionary periods.

Eisner's conceptual framework is most persuasive in its application to the Progressive Era. For that period of regulatory development, he ably demonstrates how a momentous economic change (i.e., the transformation from an essentially regional and agrarian economy to a national and industrial economy dominated by large corporate organizations) evoked broad popular demands for government intervention. In addition, the connection is clearly drawn between popular agitation and the contemporary administrative doctrines, thereby explaining the character of progressive institutions and policies. Problems with this analysis begin to arise, however, as we move to the New Deal period.

The first difficulty is Eisner's designation of the Great Depression as the economic change variable in his framework of regulatory transition. While there is no denying that the Depression was an event of cataclysmic proportions, we may ask whether it is equivalent to the structural transformation of the economy described as the prime mover in the Progressive Era. An analagous transformation might have been the bifurcation of corporate ownership and control identified by Berle and Means, yet there is no real discussion of structural change. The question, therefore, is, What does the economic change variable mean? If it does not mean structural change but simply severe dislocation, we may ask why the severe depressions of the 1890s did not occasion the emergence of a regulatory regime. Further, if dislocation will suffice as a variable initiating the process of regulatory transition, how severe must the dislocation and accompanying fears of uncertainty be? The second difficulty in the analysis of the New Deal is that the role of "social group's policy demands" is assigned to "realignment" and the "formation of the New Deal coalition." Not only is there no satisfactory explanation of how social groups influenced either the 1930s realignment or the formation of the New Deal

coalition, but there is very little illumination of party politics outside of a discussion of FDR's Commonwealth Club Address and the 1932 party platform, neither of which, according to Eisner, shed much light on the New Deal regime that eventually took shape. That being the case, one may well ask if his causal mechanism of economic change actuating social groups to define a reform agenda is operative? Indeed, the greatest emphasis appears to be placed on the role of the brains trust and FDR's "legendary political skill," rather than the activities of social groups.

As Eisner moves to an application of his framework in the transition to the societal regime of the 1970s, more vexing questions arise about the causal variables at work. In his schematic representation of this transition, the economic change variable is designated as the "emergence of postindustrial values" and "social unrest of the 1960s." In all fairness, Eisner attributes postindustrial values to sustained postwar growth, though this linkage is asserted, rather than explicated. Still, it is difficult to rationalize the identification of emergent values and social unrest as economic change. Perhaps, with some more analysis, it would be possible to show that postindustrial values are an intervening variable connecting sustained growth in real incomes with social unrest, which in turn stimulated social groups' demands. However, this is not the model of transition elaborated at the outset. As in the case of the New Deal, Eisner does a very credible job of relating the general story of regime transformation but does not convincingly fit that story into the framework of analysis that he has laid out.

Finally, in the instance of the new efficiency regime that Eisner claims to have discerned, we find perhaps the most strenuous contortions to make the facts fit the framework. In this case, the economic change variable is "stagflation, foreign competition, and regulatory compliance costs." Presumably, this variable operated the same way as the Great Depression in evoking dislocations and uncertainty. However, that line of reasoning raises the same questions about whether economic change means structural change à la the Progresive Era or can mean simply economic hardships. If the latter will do to initiate regime transition, we face the severity issue: Were the economic problems of the 1970s (certainly less disasterous than the 1930s) severe enough to set in motion the forces Eisner postulates as the prime movers in regulatory transition? Here even Eisner cannot identify the forces he claims regularly operate. Specifically, no social groups' demands emerge that remotely resemble the kind of reformist agitation associated with earlier regimes. A telltale sign of Eisner's theoretical dilemma may be seen if we juxtapose his introductory assertion, "Rather than being the product of an oppressive elite or of a partnership between monopolies and the state, the major regulatory initiatives of the past hundred years originated in popular demands" with his argument that the efficiency regime "has its origins in elite politics and the analytic biases established as a result of administrative change" (pp. xiv, 172, my emphasis). Thus, Eisner abandons the principal causal mechanism of his analytic framework in an effort to explain the emergence of the efficiency

Ultimately the problem with this book's theoretical approach is probably that it tries to explain a recurrent phenomenon, regulatory regime change, by applying a

static model across historical time periods. At each stage of regime development, the very institutional changes that Eisner describes may have substantially changed the context in which he applies the model that works so well in the Progressive Era. Like many heroic efforts at explaining long-term political development, Eisner may have fallen short of his goal; but that places him in some illustrious company. Moreover, we are indebted to him for taking on such a challenging task both because he demands that we critically reassess our own notions of modern state building and because he provides a valuable account of regulatory politics over the last century. For the latter reason, I suspect that many of us interested in the history of regulation or business-government relations will be cribbing lectures from Regulatory Politics in Transition.

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RICHARD A. HARRIS

The Shaping of American Liberalism: The Debates over Ratification, Nullification, and Slavery. By David F. Ericson. Chicago: University of Chicago Press, 1993. 238p. \$40.00 cloth, \$14.95 paper.

The Lincoln Persuasion: Remaking American Liberalism. By J. David Greenstone. Princeton: Princeton University Press, 1993. 312p. \$24.95.

The Lincoln Persuasion is a posthumously published book that is without two of the chapters David Greenstone intended for it to have, because they were not finished at the time of his death: one on the abolitionists Lydia Maria Child and Frederick Douglass and one on Daniel Webster. It is a shame to be without Greenstone's insights on these figures, but the book as an argument is substantially complete and does not suffer from the absence of these chapters. David Ericson's book derives, in part, from Greenstone's work (his book is dedicated to Greenstone's memory), yet there are significant differences in their understanding of American liberalism.

These two books continue an argument begun by Louis Hartz in modern American political science over whether American political life is, and always has been, fundamentally liberal. Against those such as Bernard Bailyn, Gordon S. Wood, and J. G. A. Pocock, who argue that one can find a dominant or at least significant preliberal republicanism in the eighteenth century and the founding period, both David Ericson and David Greenstone maintain that the character of American politics has been consistently liberal. Yet both believe that there is merit in the criticisms made of Hartz by his critics, and each adds to these criticisms. Each tries to save Hartz's basic thesis while admitting the meritorious criticisms by arguing that within America's basically liberal tradition, there are two forms of liberalism. Hartz's critics are wrong to think that there is a significant antiliberal tradition in America but right to think that there have been important disagreements and discontinuities (albeit intraliberal ones) in American thought and practice.

It is in identifying these two forms of liberalism and their significance for understanding the Civil War and the pivotal figure of Abraham Lincoln that the two books diverge. According to Ericson's construction, the common liberal tradition understands the purpose of government to be to secure liberty and protect rights. Within this tradition, there are two distinct species of

liberalism: republicanism and pluralism. The complicated distinction between these liberal species (16 propositions characterize republicanism) can be understood as presenting the two poles of a continuum. Republicanism emphasizes the importance of participation in free government and of the common good that liberalism entails, and pluralism emphasizes the importance of the private exercise of rights. Statesmen can then be placed on this continuum, and, Ericson argues, the history of American political culture can be understood as a movement from republicanism to pluralism. A concern for at least the instrumental value of civic virtue gives way to the unchecked pursuit of private ends. To show this, he examines three significant periods—those that saw the debates over ratification, nullification, and slavery. In each case, he argues that the form of the debate is determined by the relative dominance of the republican perspective (in the first two) or the pluralistic perspective (in the third), while the substance of the debate is decisively shaped by the relevant statesman's traditionalism, social realism, or nationalism.

According to Greenstone, the two species of liberalism are humanist and reform liberalism. Both kinds of liberals believe in the common liberal understanding that just government rests on the consent of the governed and has the aim of protecting individual rights and private property. Within this consensus, the humanist liberals are primarily concerned with the satisfaction of individual preferences, while the reform liberals are primarily concerned with the development of the faculties of individuals. Or (we may say), the humanist liberals are concerned with the exercise of rights, while the reformists liberals are concerned with the development of individuals who can exercise those rights well. (To illustrate, Martin Van Buren is assigned to the first camp, John Quincy Adams to the second.)

One result of these analyses is significantly differing views of Lincoln. For Ericson, Lincoln fits neatly (or almost neatly) within his scheme as a person with anachronistic republican tendencies but whose overall effect was to accelerate the inevitable triumph of pluralism. Lincoln is a significant figure (but only one of several) within the predictable history of American liberalism's movement from republicanism to pluralism. Greenstone presents a subtler and more interesting portrayal of Lincoln. He argues that Lincoln successfully reconciled reform and humanist liberal strands (unlike the reform-liberal abolitionists or the humanist-liberal Stephen Douglas). This reconciliation enabled Lincoln to arrive at a comprehensive and politically effective understanding of the slavery issue that drew on the deepest reaches of the culture, uniting the moral and religious intensity of New England Puritanism with political realism, prudence, and devotion to union. The result, argues Greenstone, was a new liberal synthesis that amounted almost to a refounding of the country. Few writers have had the appreciation for the depth of Lincoln's political understanding that Greenstone displays.

Both books contain observations and arguments from which one can learn. Yet both have a common and serious methodological flaw that unnecessarily narrows their perspective and confuses their arguments. Both Ericson and Greenstone consider the conversation in which they are participating as bound within the confines of academic political science (and only a portion of political science, at that) and refuse to consider the people they are studying as participants in this same

conversation. The result, in Ericson's case, is to confuse his argument with such infelicitously denominated distinctions as the one mentioned between "republican" and "pluralistic" liberalism. Republican is a term with an ancient lineage stretching back to classical antiquity; pluralism is a twentieth-century American invention. Does it really make sense to use these terms from such different contexts at the basic antinomies within Lockean liberalism? The answer, in case anyone is in doubt, is no. They do not clarify but, rather, confuse the issue and trivialize what is at stake in understanding republicanism or pluralism. If Ericson took more seriously, for example, the Federalist's claim to present the true understanding of republicanism, he could not use the term in the confusing way he does. Denomination of vague collections of beliefs would have to give way to a precise grappling with the issues that actually moved American statesmen. Instead of beginning with the issues raised by Louis Hartz, Ericson would have understood American liberalism better had he first feasted with Jefferson or Madison and then let Hartz come for dessert.

In Greenstone's book, one gets the impression of a naughty boy who is not supposed to talk to strangers outside of political science but who has perhaps secretly stayed up all night talking to Adams, Jefferson, and Lincoln. On the surface, he acknowledges only to having talked with Wittgenstein and the public choice theorists, and the poor reader must work awfully hard to see the choicer conversation in the depths. Greenstone would have us believe that Lincoln is all mushiness, a vague "persuasion" that cannot rival the precision of a political scientist. His account of Lincoln and his obvious admiration for him belie this impression, but Greenstone obscures his own account because he does not want to admit the company he has kept. Nevertheless, he does somehow see that the heart of the question about American liberalism is this: Does liberalism provide an adequate understanding of, and direction to, American politics, or does attentiveness to political life compel one to go outside of liberalism for this understanding and direction? And he sees that there is no better way to begin to come to grips with this question than to listen to Abraham Lincoln.

Why not admit that the person you are studying may wind up being your teacher or at least your colleague?

University of Dallas

GLEN E. THUROW

The Craft of Justice: Politics and Work in Criminal Court Communities. By Roy B. Flemming, Peter F. Nardulli, and James Eisenstein. Philadelphia: University of Pennsylvania Press, 1992. 217p. \$24.95.

This is the third book based on a National Institute of Justice-funded study of the criminal court systems of nine middle-sized communities spread among three states (Illinois, Michigan, and Pennsylvania). In the first two books, Roy Flemming, Peter Nardulli, and James Eisenstein developed detailed arguments about the guilty plea process (*The Tenor of Justice*) and the nature of the interrelationships among the actors in the criminal court system (*The Contours of Justice*). Both are major contributions to the study of courts and criminal justice, and I have regularly used *Contours of Justice* as a text.

I had known for years that a third book was planned, and I looked forward to it as one might anticipate the last

part of a trilogy. Unfortunately, the result is more akin to a sequel to *The Contours of Justice*. This new book seeks to draw on the rich qualitative materials collected by Flemming, Nardulli, and Eisenstein in the nine communities studied, materials that had been used to some degree in *The Contours of Justice* and in several journal articles. The authors' problem was to find something to say that went beyond their previous discussions of guilty pleas and court communities.

The analytic vehicle the authors chose was a pair of metaphors: community (which had been used in The Contours of Justice and, to a lesser degree, in The Tenor of Justice) and craft (which had not been used in their previous work). The book is divided into three parts, each focusing on "the craft" of one of the core players in the court community (prosecutors, judges, or defense attorneys). The discussion covers political craft (for prosecutors and judges) and what I would label "frontroom craft" (the craft of dealing with what goes on in the courtroom itself) and "backroom craft" (the craft of preparing cases, bargaining, overseeing a docket, etc.): Throughout the discussion, the authors return to the community metaphor to emphasize differences and interdependencies that mark the phenomena they describe.

The problem is that while the metaphor of community works, the metaphor of craft fails, at least as used here. The community metaphor works, at least in part, because it is based on a solid conceptual foundation: it joins the ideas of "courtroom workgroup" and "local legal culture"—both developed by political scientists (including these authors) writing about the criminal courts in the 1970s. The key idea is that there is a community of actors, who form concentric circles of core players (prosecutors, defenders, and judges), peripheral players (political parties, news media), and outsiders who regularly become involved (defendants, voters). These communities are marked by interdependencies, norms, and sponsoring organizations. Importantly, there is a great deal of variety among the communities; and simple generalizations about how the criminal justice system operates (plea bargaining, impact of calendaring systems, etc.) are not possible.

Why, then, does the new metaphor of craft fail? Unlike community, the idea of craft, while it is something we are familiar with in common parlance, is not something that most social scientists have considered as a research construct. I was intrigued by the prospect of seeing the idea of craft applied to the work of the actors in the criminal justice system. However, I was disappointed by the absence of any systematic development of the metaphor, and I was struck by a lack of fit between the idea of craft and a lot of what is discussed in *The Craft of Justice*, much of which seems to have more to do with functions or tasks than with craft.

Even when the authors do define craft they do so inconsistently:

The craft of justice refers to how [the key players] pursue their individual careers, go about their tasks in the courtrooms, organize themselves for collective purposes, and interact politically in the institutional realm of the courthouse. Their craft is both practical and political; it refers to learned and adaptive knowledge that guides practitioners in getting things done within the courthouse. (p. 4)

Craft is practical knowledge of how others perform their work and of the relationships involved in this work. It combines personal experiences with the lessons learned by others so that legal practitioners can organize their work and manage their relations. (p 195)

I came away from these definitions quite confused: Why is knowing how to "get along" a craft (it is something that anyone working in a social setting has to do)? Why is knowledge of how others perform their work a key component of craft? Why is craft limited to practical knowledge? (Is the preparation of a legal brief not a fundamental component of craft?) What do calendaring systems have to do with craft? Why is the relationship of reputation to craft?

I could go on here with many examples. I am not suggesting that these are unimportant or uninteresting problems, only that the craft metaphor fails to inform. Of course, there are elements in the book that do clearly relate to craft. This seems particularly true in the discussion of defense attorneys. The best chapter in the book is one entitled "Defending in the Courtroom," which relates most clearly to craft-type issues (knowing when to file motions and when not, knowing when to go with an open plea, knowing how to "judge-shop").

In summary, The Craft of Justice is not as significant a

In summary, The Craft of Justice is not as significant a contribution to our understanding of criminal courts as were the previous two books that comprise the trilogy. Whether this is because the authors had really exhausted the core of their material in the previous two books or because they failed to capitalize on the theoretical potential of the craft metaphor is difficult to judge. In any case, the totality of Flemming, Eisenstein, and Nardulli's work in the nine-community study stands as a major contribution to the study of justice and politics in the lower courts.

University of Wisconsin, Madison Herbert M. Kritzer

Declaring Independence: Jefferson, Natural Language, and the Culture of Performance. By Jay Fliegelman. Stanford: Stanford University Press, 1993. 268p. \$32.50 cloth, \$12.95 paper.

Starting from the premise that the Declaration of Independence was meant to be read aloud, Fliegelman takes his readers on a high-speed chase across the landscape of American popular culture in the early national period. He argues that the revolution challenged Americans to "imagine social relations on terms other than familial" (p. 191). As the new nation embarked upon "the postcolonial project of deriving an indigenous identity from the creative appropriation of Old World materials as well as from their rejection" (p. 187), oratory became a crucial site for the reconceptualization of political authority and, ultimately, for the production of a distinctively American sensibility.

Before oratory could assume such a culturally central position, it, too, had to undergo a revolution. Thus, Fliegelman reconstructs the rise of an elocutionary ideal that was essentially performative and whose aim was to create a sympathetic identification between orator and audience. This new rhetoric treated the body as an instrument and focused on "sounds, tones, and facial expressions," rather than words and arguments (p. 48). Because the orator's task was not so much to express feelings as to call them forth in others, this ideal set in

motion (but also contained) a series of conflicts-between the natural and the theatrical, self-control and self-revelation, particularity and representativeness, and

imitation and originality (pp. 64, 89, 115).

Fliegelman is "concerned with rethinking and expanding the kinds of 'facts' that are traditionally judged to be relevant to understanding a major historical document"; "In the case of the Declaration," he argues, "those facts include . . . the structure of a fugue, the particular design of a chair, the way Jefferson wrote the letter s, accusations that Milton's Paradise Lost was plagiarized, and the eighteenth-century assumption that blacks are unable to blush" (p. 4). If this book does nothing more than alert students of political rhetoric to the wide range of cultural referents that might be brought to bear in their interpretations of texts, its author will have performed a valuable service. For some readers, Fliegelman's exuberance, erudition, and eye for the telling image will prove compelling. But those who approach the book more analytically—seeking either a fresh interpretation of the Declaration or a model for cultural history—are likely to be disappointed. Ultimately, Declaring Independence enacts the ideal it describes: its argument seems less important than the performance itself.

The book is full of interesting bits of information. We learn, for example, that the term responsibility made its first appearance in print in the Federalist and that the committee charged with drafting the Declaration of Independence also proposed a national seal whose iconography featured the Red Sea overwhelming a crowned Pharaoh after Moses and his people had made their way safely to the opposite shore (pp. 143, 160-64). But while Declaring Independence offers a wealth of new images and sources that should enrich our understanding of the early national period, it does not provide a very persuasive interpretation of the texts with which we are already familiar. For example, Declaring Independence seems unable to account for the one fact that political scientists are likely to know about oratory in the early republic, namely, that the federalists condemned demagoguery and promulgated a constitution designed to ensure that reason, rather than passion, governed political decision making. Fliegelman mentions "the Tory attack on Whig oratory as demagogical social climbing and inflammatory propaganda"; but his suggestion that this attack "failed to recognize that such propaganda rested . . . on what may be seen as a new emotive theory of representation . . . consistent with the objectives of the new oratory" does not explain why (or what difference it makes that) once the revolution had been won, the federalists adopted the Tory position (p. 76).

The weaknesses in Fliegelman's argument stem from his apparent unwillingness to grapple with questions of cultural production. Lurking beneath the postmodern vocabulary and concerns of Declaring Independence is a rather old-fashioned brand of intellectual history. Culture seems to consist of a series of texts that share the same language and concerns. Though forewarned that "this study proceeds by associative progression rather than by the isolation of different discourses" (p. 4), the reader hardly expects to encounter a work in which the only principle of association seems to be resemblance. Fond of the dialectic and of dilemmas, of ambiguity and anxiety, Fliegelman sees tension everywhere, but, of course, the same tensions keep recurring.

Curiously, this emphasis on "internal conflict" undercuts the possibility of any genuine contestation. Symptomatic of this problem is Fliegelman's failure to distinguish between federalists and antifederalists. When, for example, Fliegelman claims that the American ideal of representation called for the construction of a legislature that would be a mirror image of the people, he quotes George Mason's critique of the Constitution of 1787 as if it were a description of the theory that governed the Constitution's design (p. 43). Maybe this is just poor research (Fliegelman cites Morgan citing Mason), but it is also the kind of mistake that is a likely artifact of a method that selects similar passages from very different kinds of texts and then juxtaposes them without analyzing the rhetorical or institutional contexts in which they occur. Such an approach systematically suppresses the possibility that the same culture (or maybe the same nation) can contain radically different conceptions of its most fundamental concepts.

Fliegelman's treatment of contemporary scholarship poses a similar problem: nothing challenges his thesis, because everything can be incorporated into it. In Letters of the Republic (1990), Michael Warner made a quite compelling argument that seems diametrically opposed to the thesis advanced in Declaring Independence. Warner contends that republican ideology was embedded in a print culture that made impersonality a norm of public political discourse. Though praising the book in a note, Fliegelman dismisses its central claim in a single paragraph, stating that Warner has overlooked the fact that print culture operated alongside the oratorical conventions Fliegelman describes. "Indeed," Fliegelman argues, "what energizes a great deal of the polemical prose of the period is the dialectical relation between the authority of impersonality . . . and the authority of

sincerity" (p. 129).

This book would be considerably more powerful if Fliegelman were as sensitive to the differences between discourses as he is to the similarities across them. Both Richard Brown's Knowledge Is Power (1989) and Cathy Davidson's Revolution and the Word (1986) suggest that there was more than one way of constituting authority in the early republic and that each kind of authority had its own audience, logic, and institutions. Brown shows how, in the course of the revolution, lawyers came to eclipse ministers as the official spokesmen for their communities. Davidson discusses how both political leaders and clergy thought that the rise of the novel threatened to undermine their authority. At stake in acknowledging the contestation between discourses is not simply a preference for conflict over consensus but the difference between a static and a dynamic conception of culture. When culture is conceived of as something that leaves the same imprint on all of the artifacts of a particular era, the only ways to conceptualize change are as revolution or degeneracy. Fliegelman opens on the first note and closes on the second. By the end of his narrative, the "quest to find a voice and visual self-presentation that are true to nature, society, nation, and self has become . . . little more than an existential confidence game" and the bottom floor of the house in which Jefferson drafted the Declaration of Independence has become a clothing store whose handbills proclaim every American's right to "Life, Liberty and Genteel Garments" (pp. 196, 200).

Johns Hopkins University

SUZETTE HEMBERGER

Facing the Bureaucracy: Living and Dying in a Public Agency. By Gerald Garvey. San Francisco: Jossey-Bass, 1993. 252 p. \$23.95.

Inside NASA: High Technology and Organizational Change in the U.S. Space Program. By Howard E. McCurdy. Baltimore: Johns Hopkins University Press, 1993. 215p. \$32.95.

These two very good books use the theme of the organizational life cycle as a point of departure for the analysis of two organizations, the Federal Energy Regulatory Commission (FERC) and the National Aeronautics and Space Agency (NASA). Each story is anchored in a theoretical theme: bureaucracies lose resilience as they age; controls from the top are imposed in response to the loss of resilience; the new controls cause new transactions costs in the work of the organization and impair effectiveness; aging is only one reason for the loss of resilience; reform efforts to reduce controls and yet ensure accountability are difficult and uncertain in their effectiveness.

Garvey tells the story of the birth, life, and death of a project in the FERC to computerize and automate the system of rate making for the natural gas industry. He was a consultant to the project and writes from a participant observer stance. He posits that the "old theory" of public administration invented by the Progressives, which sees bureaucracy as efficient and politically accountable, is unable to explain the discoveries of the "new theory" about adverse selection, moral hazard, and the use and misuse of discretion. The chairman of the FERC was concerned about the even-handedness of a decentralized regulatory process that rested on bargaining between rate makers and private firms. Garvey was appointed a consultant through a bureaucratic end run that was later challenged. The process of reform worked initially, because an experienced administrator appointed a task force of computer consultants and key rate makers, which, over time, developed a new system to simplify the work of rate setting and make it more uniform yet permit discretion on the part of individual regulators. A consensus on the new standards was reached, but the achieved plan was only a half-way house toward a comprehensive system. Then an internal investigation of the project arose out of a feud among top agency officials, the method of Garvey's appointment was challenged, the appointment was invalidated, and the computerization project was killed by agency administrators worried about a more extensive inquiry.

Garvey draws practical lessons from the story. Organizational entrepreneurs can initiate directed changes against the natural changes that institutional aging brings. The key to success is public managers with political sense who understand effective strategies of organizational reform guided by prudence. Reform of this kind must take advantage of the unfulfilled personal aspirations of civil servants if it is to work; that is, the slack found in such aspiration is the best basis for helping people to do their jobs better through reform. He finds the "old theory" of public administration a better guide to action here than the "new theory," which sees bureaucracy as permeated by "self-regarding" motives. Old theorists understand the importance of organizational culture, to which new theorists are blind. Garvey sees the naïveté of both the old and new theories and seeks to combine the idealism of the old

and the tough-mindedness of the new in a thesis about prudential leadership.

Garvey does a good job of making a case study theoretically pertinent. The story is a bit slow, perhaps because it is not inherently interesting, and the participant-observor stance is one person's view of events. The ultimate normative issues are not resolved, but the book will contribute to the development of theory about organizational renewal.

McCurdy describes the origins of NASA out of agencies-charged with research-and-development for aeronautics in the interwar period. These agencies and the founding NASA created an organizational culture that gave highest value to in-house research-and-development, testing of products through hands-on engineering, and tolerance of failure through high-risk strategies—in short, a paradise for innovative engineers. The combination of youthful talent, a supportive political environment, and the historical opportunity to invent exciting technology fostered an agency that valued inquiry, open communication, attention to detail, honesty, idealism, and romance.

But after the excitement of putting men on the moon, public interest waned, political support declined, and the NASA became an operating agency, offering less challenge to its engineers and more opportunities for outside contractors. Centralized bureaucracy became stronger, and the culture of technical inventiveness eroded. Congressional oversight increased, White House control was heightened, the regulatory ethos of government grew oppressive, and bureaucratization burgeoned. Administration won out over technical invention, the dependence upon contractors increased, and the capacity to oversee them declined. The decline of in-house testing of products made the punishment of failure even worse because of limited public sympathy. The communications failures seen in the Challenger explosion and the Hubble Space Telescope mishap were manifestations of an organization that had lost the "taut communication" of the old NASA.

McCurdy concludes that high-performance bureaucratic cultures are inherently unstable. Once a governmental agency matures, the political environment that had supported its dynamism has disappeared. The most successful science and technology programs have been able to circumvent the surveillance that controls most bureaucracies. The Manhattan Project, Tennessee Valley Authority, (TVA) and other such agencies cannot be sustained in the normal bureaucratic mainstream. And once on the decline, it is very difficult to put new wine in old bottles.

McCurdy's research effort was extraordinarily comprehensive, relying greatly on interviews and surveys. Had he written a longer book, we would have a richer sense of the changing bureaucratic culture. Many of his descriptions of professional and subagency perspectives are redundant. Above all, I wish that he had given us a portrait of successive NASA leaders so that we might assess their contributions to the growth and decline of the organizational culture. As he presents it the NASA is virtually leaderless. But again, this book is a strong contribution to the theory that will emerge in time.

The two books together pose a number of important theoretical insights. First, the aging of an institution is important not in itself but for its actions and reactions. The completion of the initial missions stated by the founding charter, a changing political environment, and new generations of leaders all combine to explain the cycle of rigidity and control. Life-cycle theory is thus useful for students of bureaucracy not as a teleology but as a source of insights of opportunities and constraints on action at different times of an organization's history.

Second, politics and the pressures of politicians make it hard to keep agencies lean and innovative. Politicians will pile on encumbrances for reasons of their own often having little to do with the work of the agency. This seems to be especially true after an agency has passed its peak creativity. Incorporation into the bureaucratic mainstream effectively kills its spirit. But perhaps that is what should happen if the heroic mission has been completed.

Third, agency inventiveness and autonomy go together. The insulation from politics is the key to success. Political accountability engenders bureaucratic timidity; but again, perhaps this is appropriate if the initial agency autonomy was empowered by popular politics, as was the case with the NASA.

The systematic comparison of organizational stories like these within a life-cycle framework might permit the development of rich theory about diverging patterns in institutional history. Organizations can become stagnant like the NASA, discredit themselves by trying to stay heroic like the TVA, engage in prudent reform like the FERC, or engage in widespread organizational renewal like the General Accounting Office in recent years. Such a theory would permit us to assess the relative importance of leadership, organizational culture, and changing environments in understanding which of these paths is taken.

We also do not have sufficient prescriptions for combining bureaucratic resilience with political accountability. The two brands of "institutionalism" in political science carry different messages: the one relies on culture and norms and the other looks to mechanisms powered by self-interest. Is there a third theory that might incorporate the partial insights of the other two?

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ERWIN C. HARGROVE

Latino Voices: Mexican, Puerto Rican, and Cuban Perspectives on American Politics. By Rodolfo O. de la Garza, Louis DeSipio, F. Chris Garcia, John Garcia, and Angelo Falcon. Boulder: Westview, 1992. 232p. \$29.95 paper.

\$29.95 paper.

LULAC: The Evolution of a Mexican American Political

Organization. By Benjamin Marquez. Austin: University of Texas Press, 1993. 141p. \$25.00 cloth, \$11.95 paper.

The past few years have seen a steady, if modest, increase in the interest in, and research on, Latinos in the U.S. political system. Not only has research increased, it has also changed somewhat in tenor and direction and has encompassed a broader range of questions and methods. Some of the earlier research on Latino politics might have been viewed, or even dismissed, in certain scholarly circles as being descriptive, impressionistic, advocacy-oriented, or otherwise out of the mainstream of political science research. The questionable accuracy of such characterizations aside, the two books under review here can hardly be criticized on those grounds. Both are quite conventional in their concerns and approaches and are certainly rigorous,

serious scholarly undertakings. And both volumes represent "firsts." Latino Voices summarizes findings from the first large national survey of Latinos, political attitudes and beliefs. LULAC is the first book-length study to chronicle the rise and development of the League of United Latin American Citizens, a major Latino interest group, and the first work to apply Olson's "incentive theory" to such a group. Moreover, both books provide important—and sometimes surprising—insights about Latino mass attitudes, behavior, orientations, and interest groups.

These two books, in themselves and in what they promise for the future, are likely to be seen as key pieces in the puzzle of research on Latino politics. At the same time, their methods, findings, and conclusions evidence the complexity and ambiguity inherent in seeking to understand Latino politics. The similarities noted, the two books are different, partly due to substantive interest and partly due to methods. Latino Voices is a broadbrush treatment with a contemporary, survey data focus; the Marquez book is an in-depth, historically oriented, archivally based study. The former is consciously "pan ethnic"; the latter addresses (only) a Mexican–American organization.

Latino Voices provides an overview of findings from the Latino National Political Survey (LNPS), a survey of over 2,800 Latinos and 456 non-Hispanic whites (Anglos) conducted in 1989–90. The LNPS was modeled after—and, in fact, borrows many questions from—other major political surveys, such as the National Election Studies and NORC. Following an introductory chapter outlining the study's foci and survey methodology are eight substantive chapters with 172 tables on the Latino citizen population and one chapter with 86 tables on the noncitizen Latino population (One appendix includes the entire questionnaire.)

The goals of this volume are, as the authors explain, "modest: to provide basic information about the political values, attitudes, and behaviors of the Mexican-, Puerto Rican-, and Cuban-origin populations in the United States" (p. 1). As basic as those goals may appear, they are important in that previous general political surveys have included too few Latinos to provide reliable information. Moreover, the broad array of questions posed and the delineation of responses by specific nationality groups (including Anglos) produce a rich survey. Because the goals of this work are modest, however, the authors provide relatively brief narrative to highlight evidence in the many tables. Papers authored collectively and individually by the team of scholars involved in this research have already supplied more detailed assessments and "future volumes will thoroughly analyze" the LNPS's results (pp. 1, 6).

What are some of the more notable findings? Apart from the finding that "these [Latino] groups defy simple categorization" politically (p. 15), I suspect that readers' reactions to the data will depend on their expectations and previous knowledge of Latino politics. For several forms of nonelectoral political participation (e.g., attending rallies, volunteering for political parties, attending school board meetings), Latinos are about as likely to participate as Anglos (p. 120). This, too, is in the absence of controls for socioeconomic background; appropriately adjusted, Latino participation in these areas might equal or exceed that of Anglos. Second, the self-professed political ideology of Latinos, "regardless of national origin, was moderate to conservative" (p. 83).

Recognizing the summary nature of this work, several criticisms can be noted, nonetheless. First, while a vast array of data are presented, the tables provide only percentages. Presentation of simple summary statistics (means, medians, standard deviations) would have aided tremendously in making sense of the large and complex body of evidence. Second, many tables provide useful comparisons between the Latino groups and Anglos. However, it is not always clear why Latinoversus-Anglo comparisons are presented in some instances but not others. In some circumstances, the logic of the question indicates why such comparisons would make little sense; in other instances, that is not the case.

In LULAC, Marquez deftly portrays the history, goals, thinking, and struggles of LULAC, using archival data (group newsletters and members' personal correspondence) and interviews with group members as primary sources. Incentive theory, as developed by Mancur Olson and others, is employed to frame—and thus make sense of—the group's rise and change. Created in the late 1920s in Texas, LULAC began with "purposive and expressive" goals aimed at achieving equal opportunity—goals that ostensibly were often accomplished through litigation in state courts. LULAC underwent a second burst of "mobilization" after World War II but has been changing and, over time, has come to face a "politics of survival."

The book's "central thesis" is that LULAC's longevity has been due to "the organization's ability to effectively adjust its incentive structure during the late 1960s and early 1970s. This change in the incentive structure transformed the organization from an activist civil rights group to a staff- or elite-dominated group that would devote much of its energies to continuity and survival" (p. 7). That survival has come with significant costs to LULAC's role as a serious political actor; heavy reliance on outside funding, including governmental and foundation support, has rendered it politically irrelevant.

Despite its absorbing and systematic discussion, various questions might be raised about this study. Marquez seems to assume (perhaps too readily) that because LULAC developed along a path consistent with what incentive theory would predict, the theory is therefore (fully) supported. It may be that LULAC developed as that theory generally predicts, although there are some "deviations" from the predicted pattern noted in the book. More significantly, LULAC's development might be equally well understood with other theories or explanations, but incentive theory is the only explanation considered. I should like to have seen clearer, more definitive evidence to corroborate the accomplishments attributed to LULAC; as it stands, those accomplishments are more asserted than solidly supported.

Finally, a more extended treatment of the theoretical implications of this study would have been helpful. These concerns are hinted at (but only hinted at) in an all-too-brief (six-page) concluding chapter. I should like to have had Marquez's thoughts on various questions: What does the present state of LULAC, as well as that of other mainstream Mexican American interest groups, mean for Mexican American politics? For (broader) Latino politics? For interest group politics? How critical is the survival crisis facing LULAC in the bigger picture of Mexican-American/Latino politics, given the disadvantaged political and social status of the Latino population? What might all this mean for the contemporary state and future development of U.S. liberal/pluralist democracy?

These and related questions should be among the central ones shaping the Latino politics research agenda in the near future. The work of Marquez, as manifested in the present work and as it will (hopefully) be broadened and extended can offer one useful perspective on these questions. The LNPS has initiated another approach to these issues and promises to speak fruitfully to these concerns from another angle. Diverse approaches are not only desirable but necessary, given the complex and ambiguous nature of Latino politics that these two books and most research on Latino politics demonstrate. What is most important to recognize is that larger theoretical and normative questions should guide the inquiry and that approaches are secondary.

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The Hollow Core: Private Interests in National Policy Making. By John P. Heinz, Edward O. Laumann, Robert L. Nelson, and Robert H. Salisbury. Cambridge: Harvard University Press, 1993. 450p. \$39.95.

It has been said that the study of pressure politics in America is characterized by a surfeit of theory and a paucity of data. In this important volume, a distinguished set of coauthors who hail from the relevant disciplines of law, sociology, and political science use what is unambiguously the richest and most comprehensive body of systematic data about *Washington representatives* (advocates for organized interests in national politics) to remedy that circumstance.

The remarkable data base underlying this inquiry consists of hundreds of interviews with Washington representatives, individuals in the organizations that hire Washington representatives, and elected and appointed government officials. The samples represent something of a "stratified snowball." The authors plumbed several sources to compile lists of organizations and individuals involved in four policy domains: agriculture, energy, health, and labor. By selecting respondents from these lists with probabilities proportional to the number of mentions, they generated, instead of a random sample, a sample that overrepresents active and influential individuals and organizations. A major portion of the book is devoted to establishing who these Washington representatives are—their backgrounds and careers, the organizations they work for, and the breadth of their policy concerns.

The heart of the book (and the section that explains its cryptic title) consists of a series of smallest-space analyses that carefully chart who knows, talks to, and fights with whom in each of the policy domains. It is here that the research strategy of concentrating on only a limited number of policy arenas pays off, for the detailed examination of the patterns of relationship would have been impossible with the more scattershot set of organizations inevitably captured when all policy domains are sampled. The communications networks and the structures of conflict and cooperation vary with the policy domain. However, in no domain do the authors find a small, cohesive elite playing either a dominating or mediating role at the center. Rather than a wheel with a hub and spokes, the networks of communication among influentials resemble a soap bubble, with an empty center and a thin surface—hence, the "hollow core." In short, the authors have developed a powerful methodology that takes us beyond the obsolete notion of the iron triangle and lends precision to Heclo's notion of the porous but diffuse policy network. This methodology could be applied to policy domains ranging from civil rights to foreign policy to taxation (domains presumably characterized by quite different patterns from those examined here) to provide a detailed map of the configuration of organized interests in national politics.

In the final part of the analysis, the authors confront, in a systematic way, the knottiest problem facing analysts of pressure politics, assessing whether and under what circumstances organized interests influence policy outcomes. Because it is more difficult to sample controversies than organizations, recent survey-based studies have told us much more about which organizations are active in national politics, what they do, and whom they represent than whether they have clout. In each policy domain, the authors located 20 major policy controversies and asked respondents several questions about each controversy, including how successful the organization was in realizing its goals. With admirable candor, the authors admit that their multivariate analysis (using such intuitively relevant variables as the number of years the respondent spent in government or the number of professionals employed by the organization) predicts surprisingly little about organizational success in achieving its policy objectives. From this they conclude that the hallmark of policy controversy in national politics is the uncertainty of the outcome.

The authors may give up too soon. They are aware of some of the potential pitfalls: a better measure of success might have incorporated the results of analysis of decision making in addition to the self-assessments of success by respondents. Other variables (esp. more and better measures of, e.g., organizational budget and the size of its PAC) might have been introduced into the equations. More importantly, however, the authors take insufficient account of the problems intrinsic to their methodology and the extent to which their original sample design exacerbates those problems. We must recall that the organizations examined have undergone several processes of selection, including the authors decision to oversample the active and influential. Thus, the authors' real finding is that when active, powerful organizations get to the table to settle a relatively important issue, the variables that might have been expected to make a difference seem not to matter. Had the sample included not just the big fish but the little fish whose efforts in Washington go unnoticed, the minnows that cannot even establish a presence in Washington, and the latent interests that never even hatch into organizations, then the authors might have found many factors that affect who wins and loses in Washington. This suggests, by the way, that students of pressure politics must not neglect two objects of inquiry that the authors of this study know they cannot pursue: how issues are placed onto (or kept off of) the policy agenda and what sort of organizations come into existence and are able to establish a substantial presence in national politics.

That the index to this volume contains more references to Talcott Parsons than to David Truman says a great deal about its place in the substantial literature on organized interests: this collaborative study owes more to sociology than to political science. This is manifest in superficial ways. For example, the authors display none of the relish—seemingly universal among political scientists who study organized interests in Washington—

for colorful details and illustrative stories about particular organizations and particular disputes. Furthermore, many of the empirical findings in this relentlessly datadriven book must come as no surprise to readers familiar with the Washington scene (e.g., that Washington representatives talk to their friends and not to their enemies or that what they know counts for more than whom they know). However, it would be unjust to criticize the authors for using an expensive study and a sophisticated statistical apparatus to tell us what we already knew. That systematic data often disconfirm what we know from anecdotes or personal experience is especially important in a realm as rife with misleading stereotypes as Washington pressure politics.

The sociological roots of this study are evident in fundamental ways, as well. Its pathbreaking contribution—the development and application of techniques to permit the mapping of the networks of communication and patterns of conflict and cooperation in the realm of pressure politics—derives its inspiration from sociology. In contrast, several topics typically treated by political scientists do not figure importantly: we hear little, for example, about the constituencies of organizations, the constraints they impose and their role as a resource in confronting policymakers; about elections and the place of political action committees in organizational strategies; about the substance of what has become a series of marketlike exchanges between Washington representatives and public officials, each of whom controls important kinds of political capital. Finally, although the authors refer in passing to the abundant theories generated by political scientists to understand pressure politics, their study does not pay consistent attention to a series of questions about which students of democracy have been known to obsess: Who governs, how, and on whose behalf?

Boston College

Kay Lehman Schlozman

Redefining the First Freedom: The Supreme Court and the Consolidation of State Power, 1980–1990. By Gregg Ivers. New Brunswick: Transaction Publishers, 1992. 248p. \$32.95.

Religious deviance is one of the leading rationales human beings have given for depriving each other of fundamental rights. For worshipping the wrong deity or rock—or for worshipping the right deity or rock in the wrong way-people have been killed, tortured, and imprisoned. Religious minorities who avoid these fates are often treated as second class citizens in their homeland. State laws privilege dominant faith communities by teaching religious doctrine in schools, placing religious symbols on private property, using tax revenue to pay sectarian expenses and refusing to make exceptions when seemingly neutral secular policies place severe and unnecessary burdens on politically powerless religious communities. Given the numerous obstacles religious minorities face in many parts of the world, the recognition of the right to freedom of religion remains a precious and fragile achievement.

Ivers believes that this precious freedom is presently under assault in the United States. His *Redefining the First Freedom* correctly notes that "church-state separation and religious freedom are not privileged occupants in the contemporary [Supreme] Court's constellation of

constitutional values" (p. 2). Conservative politicians and justices, Ivers insists, have already "emasculated the law of religious free exercise" and are a vote short of "render[ing] the establishment clause toothless" (p. 2). Unless new appointments dramatically change the recent direction of judicial policymaking, Ivers fears we may again return to a state where "majoritarian religious values" dominate the "civic culture," "exclud[ing] the needs and interests of religious minorities" (pp. 2–3). Redefining the First Freedom clearly demonstrates that

the Supreme Court during the 1980s narrowed the scope of both the establishment and free exercise clauses of the Bill of Rights. In cases concerning public aid to parochial schools, religious access to public schools and religious symbols on private property, a court remade by Ronald Reagan and George Bush was consistently supportive of popular majorities that wished to infuse public life with more religious values. The court's "highly deferential posture toward majoritarian decision making" was particularly egregious in Bowen v. Kendrick (1988) when the justices sustained provisions of the Adolescent Family Life Act (p. 84). That unfortunate measure offered direct federal grants to religious groups that study and respond to problems of teenage sexuality. The only serious flaw in Ivers doctrine analysis is his tendency to claim that decisions that permit government to "accommodate" religious practices require government to do so. Thus, although the Court has found the Equal Access Act constitutional, the justices have not argued that in absence of such legislation local communities would be constitutionally obligated to permit religious organizations to use school buildings for meetings.

Although Supreme Court policymaking is the primary focus of Redefining the First Freedom, that work makes several important observations about lower federal courts and interest groups. The Court's tendency to avoid clear statements of constitutional principle in many First Amendment cases, Ivers documents, has left lower federal courts with little guidance on such matters as how many secular symbols it takes to make constitutional a state-sponsored crèche in a public place. Ivers's examination of religious groups' involvement in recent cases refutes the too common claim that persons who oppose school prayer or aid to parochial schools are somehow hostile to religion. As he clearly shows, many religious organizations, Jewish and liberal protestant groups in particular, have fought hard over the past 20 years to maintain against political and judicial efforts to lower the wall of separation between church and state (pp. 7, 107).

Indeed, significant religious opposition to the recent direction of judicial policymaking partially undermines Ivers's claim that the justices have tolerated the infusion of majoritarian religious values into the civic culture. The mainstream Protestant ministries represented by the National Counsel of Churches have historically wielded far more political influence than the evangelical groups that *Redefining the First Freedom* sees as the Court's most influential supporters. Rather than understanding the Moral Majority as representing popular majorities, a more sophisticated treatment of interest group politics might understand that coalition as a large faction that supported Republican economic policies during the 1980s in return for some support of its religious agenda.

Ivers does not adequately analyze the more general constitutional principles that might explain why the

Rehnquist Court has given more sanction to religious expression in public life. For the most part, he explains recent judicial practice as part of the reflexive judicial habit of supporting the government against claims of individual right (pp. 139-40, 173). The same Rehnquist Court justices who have done the most to limit the scope of the establishment and free exercise clauses, however, have also limited state power to pass affirmative action programs, restrict campaign spending, and take property without compensation. Hence, one might want to know why conservative justices are so eager to limit the scope of some constitutional freedoms while expanding the scope of others. Ivers might also have speculated why Justice Scalia's libertarian strand, which is so evident in flag burning and drug testing cases, suddenly deserts him when he considers the free exercise clause.

Although often thoughtful and challenging, Redefining the First Freedom has the feel of an extended ACLU fundraising letter. With rare exception, opinions that deny establishment clause claims are described as "blind," "indefensible and unprincipled," "illogical," "analytically bogus," "reactionary," "crabbed and misconceived," "scandalous" and "utterly indefensible" (pp. 71, 105, 107, 108, 117, 134, 148, 153). Moreover, each defeat is described as "disastrous" or "tragic," as if allowing a crèche to be placed on the town green or a moment of silence before class is the first step in a process that will eventually bring back the Inquisition (pp. 134, 173). Ivers's concern that if the Supreme Court permits "religious devotionals during ceremonial occasions, . . . will the establishment clause still have a role in securing religious freedom for . . . anyone at all" lacks any historical perspective (p. 53). The First Amendment may forbid any governmental act that directly helps a particular religion or religion in general, but Ivers is wrong when he claims that adoption of Justice Kennedy's coercion standard "would gut the core of the Establishment Clause" (p. 114). As religious dissenters in the former Soviet Union and Iraq will attest, a non-coercion requirement is the heart of religious freedom. Judicial decisions permitting too much accommodation of religion may violate the First Amendment, but religious freedom in the United States will not disappear if communities are allowed to place cheap crèches on public squares during the holiday season.

University of Maryland, College Park MARK A. GRABER

Sharing Power: Public Governance and Private Markets. By Donald F. Kettl. Washington: Brookings Institution, 1993. 232p. \$22.95.

Sharing Power, by Donald F. Kettl, stands in a lengthening line of recent works on the relationship of public governments to private markets in the provision, production, and delivery of traditional public services. Kettl's work is not, however, a primer on how to share power between the two. Through a description and analysis of five "case studies." Kettl identifies some of the fundamental forces in government and in the market that make the sharing of power complex and difficult, if not impossible. Specifically, he challenges the current and popular rhetoric that declares that greater reliance on the market—what he calls the competition prescription—is the solution to many public problems. While he categorically states that his work in neither an attack on nor

a defense of the competition prescription (p. 6), he argues that market and government imperfections create uncertainties that may cause the competition prescription merly to shift problems or to create more problems than it resolves. He further argues that government has a diminished capacity to manage shared power with the market. Sharing Power thus provides a cautionary note to the too easy acceptance of public governance' relying on the market and attempts to identify what is required of government and, indirectly, of the market in order for shared power to work.

As Kettl correctly notes in the first chapter, the sharing of power between public governments and the private market in the United States is not a new phenomenon. However, the historical record documents an ebb and flow in our tendency to place our hopes and faith in one or the other. Accordingly, we adopt a consensus or reliance, or (in the terms of Yehezkel Dror's Public Policymaking Reexamined, 1968) a metapolicy, that is, a policy about policies that shape reliance. In Kettl's terms, we change principal agents. Before the Great Depression our metapolicy of shared power was one of reliance on the market. Following the Depression and World War II and for the subsequent three decades, the metapolicy of the United States was one of substantial reliance on the public sector for the addressing and solving of many public problems. In the 1980s, in the United States and around the world, the perceived overreliance on, and cost of, public governance initiated the current era of heightened interest in market solutions to public problems. Government became the problem, not the solution.

In the United States, the current shared-power movement has been fueled by the convergence of three major and often ideologically incompatible forces and has taken multiple forms. The three major forces are (1) the generally antigovernment position touted by the Heritage Foundation, public choice economics, and even the 1988 Report of the President's Commission on Privatization; (2) government reformers seeking a better way to "do government"; and (3) the pragmatist attempt to address the problem of how to balance the budget in the context of great opposition to higher taxes. The forms of shared power range from contracts, grants, and vouchers to tax expenditures and denationalization. The labels include privatization, public-private partnerships, third-party government, and Kettl's government by proxy (see his 1988 book of that title), among others.

Kettl enters this complex arena to examine what this growing reliance on the market means in terms of how well public governments can manage partnerships in which they share power with the market. He concludes that government has not managed shared power very well and notes the irony in the many cases of shared power that have resulted in waste, fraud, abuse, and criminal activity on the part of the private-market participants. Kettl makes the case that shared power does not so much reduce the size of government as change the role of government from a deliverer of services to a manager of private-market service delivers. Based on the case studies, he concludes that the competition prescription fails because the overall problems of government are due to uncertainties and imperfections in government and in the market, not to lack of competition. He further concludes that even where shared power can work, government is not equipped to play the required shared-power management role. In the final chapters of this work, Kettl offers nine propositions that define the uncertainties in the relationship of government as buyer and market as seller and attempts to provide a prescription for how government can become a smart buyer of private-market goods and services and how government can become an effective manager.

Kettl is not the first to recognize the complex managing and governing relationships between public governments and private markets. Charles E. Lindblom's Politics and Markets (1977) argues that the market holds government prisoner and is thus an unequal partner. The National Academy of Public Administration's Privatization: The Challenge to Public Management (1989) concluded that privatization changes the practice of government and called for training and education of public administrators to manage shared power. And Heilman and Johnson's Politics and Economics of Privatization (1992) documented the complex relationships that exist in water-quality privatization and concluded that successful privatization would depend on the quality of the governmental and private-market contractual and management relationships. However, the study concluded that in no case should government relinquish the provision of an essential public service and a matter of governmental sovereignty (as Kettl notes). What government can relinquish or share is the production and delivery of a service. It is in these functions that a smart buyer and a capable manager become critical.

While Kettl more than once professes neutrality on the question of the inherent soundness of the competition prescription, Sharing Power is, in fact, a broadscale challenge to indiscriminate reliance of government on the market. The reader might wish for more help in sorting out the options. The organizing concept of shared power is not well defined, and the varying forms (e.g., contracting, privatization, partnerships) are used interchangeably. They are not interchangeable. Some forms challenge the sovereignty of the state, while others do not. One can be neutral on some forms and not on others. Second, Kettl's prescriptions are both too much and too little. They are too little, in that we know that effective government managers must be hired, trained, and rewarded. I would add that before hiring, they must be educated in public administration programs that teach democratic theory and public-private policy and administration. The prescriptions are too much, in that they identify the key elements needed to make shared power work-namely, to lower the rhetoric and implement the prescriptions through strong leadershipwithout providing a clue as to how to achieve either of these. I suspect, in fact, that Kettl's work might well raise, rather than lower, the rhetoric.

Nevertheless, Sharing Power addresses the important issues of shared power between public governments and private markets. At its core, this is a book about democratic theory—about what governments and markets can and cannot do. Kettl recognizes the fundamental differences between the dynamic of government and the dynamic of the market and explores what is required in order for them to share power. Anyone standing in the stream of the ongoing movement toward greater government reliance on the market (and that includes citizen, practitioner, and academic) would do well to read Sharing Power. Kettl sets the correct and relevant agenda.

Auburn University

GERALD W. JOHNSON

The Market Experience. By Robert E. Lane. New York: Cambridge University Press, 1991. 630p. \$65.00 cloth, \$24.95 paper.

Robert Lane was "blindsided in the 1960s by the counterculture" and spent the next 25 years coming to terms with it (p. vii). As a result, he has produced a book that could do more to revolutionize Western civilization than anything produced by the counterculturalists. He argues not simply that markets are bad or socialism good but that all of our standard criteria for judging the market are wrong. We should not evaluate the market on the terms upon which both its supporters and opponents agree, namely, whether it does or does not provide economic well-being through the production and distribution of goods. Rather, we should evaluate it on precisely the terms that the counterculturalists struggled (mostly unsuccessfully) to enunciate, namely, whether it enhances life satisfaction and human development through the processes of working, creating, interacting, and desiring.

Lane has not been completely converted. No counter-culturalist would agree that as between "relations with others" and work, the latter is "the major source of adult development" (p. 5). Nevertheless, his book rings with affirmations of the need for self-esteem, personal growth, and mutually satisfying relations. Similarly, no counterculturalist would turn, as Lane does, to the most rigorous experiments in cognitive psychology for evidence about human development. Still, he uses this evidence to judge markets according to the criterion that "the value of persons is as great as the value of commodities" (p. 7). Tom Hayden and Herbert Marcuse, move over!

In all seriousness, this is an extraordinary book in many ways. First, the sweep of its claims is breathtakingly broad and important: they will indeed revolutionize social science and social policy if given the attention they deserve. Second, an array of detailed analyses of social science literatures is brought to bear on the question of what actually happens as people experience the market. Lane analyzes hundreds of psychological experiments, opinion surveys, social theories, and philosophical treatises in the work of examining topics ranging from the meaning of rationality to the meaning of happiness, from the social role of money to the possibility of friendship. His ability to link these narrowly focused studies of particular locales to sweeping generalizations about human nature—and to do so mostly persuasively—is yet another extraordinary feature of The Market Experience.

It is surprisingly hard to summarize the flow of the narrative—as witnessed by the fact that the introduction lays out several lines of argument, none of which bear much resemblance to the actual order of the chapters. Lane explains why the book is not "linear" and is "reflexive," and his reasons both accurately describe and convincingly support his own project. Roughly speaking, The Market Experience begins with broad discussions of the meaning of "human capital" and of the importance of recognizing the interrelatedness of all forms of social engagement. It moves into analyses of useful and misleading ways of understanding the meaning of rationality and emotions and argues for the value of "cognitive complexity" as a substitute for rationality as a mark of human development. It then further elucidates the idea of human development by analyzing self-attribution (what political scientists more commonly call efficacy) and self-esteem. It moves at that point from individual traits to relational issues of friendship, market exchange, and the social meaning of work. From there, The Market Experience criticizes conventional treatments of money (the presumed reward of undesired work) and reformulates the real rewards of work (intrinsic satisfaction at a creative job well done, which might incidentally bring in some money). By this point, Lane is ready to analyze the meaning of happiness and the role of markets in producing it. He concludes with a brief sketch of the "elements of culture and society that promote and retard . . . economic reforms." A long, winding, and rewarding journey, with perhaps not a clear enough end.

Throughout the book, Lane refuses either to condemn or praise markets in simple terms. They are good at producing self-attributions (which are emotionally desirable even if empirically mistaken); they are misused to equate happiness with more money (above a certain level of well-being); their relation to self-esteem is complex. In general, markets do better at things they are not designed for (creating meaningful work and mutually satisfying interactions) than at things they are designed for (inducing people to work hard at hated jobs for monetary rewards). Over and over, Lane surprises with his iconoclasm and provokes with his cool, idiosyncratic, and well-defended assessments.

I cannot conclude without mentioning the most extraordinary element of The Market Experience (for political scientists, at least). Politics is almost totally absent from it. Lane hardly mentions the role of political institutions in shaping markets or in choosing among alternative systems of production and distribution. He barely refers to the role of markets in determining who gets political power or what people with political power can do in a given society. "Politics" appears once in the index, power" twice, and "government" four times—this in a book crammed with a discussion of almost every conceivable form of human interaction. Ignoring politics was clearly a deliberate choice, and I wish Lane had told us why he made that choice. In my perhaps parochial view, the absence of any discussion of power or politics leaves a large hole in this otherwise comprehensive analysis of what markets can do or actually do and in any claim about how we should think about them. We are left with a final irony: the philosopher and economist Albert Hirschman tells us more about the interaction of politics and markets (in The Passions and the Interests) than does the political scientist Robert Lane.

Princeton University

JENNIFER L. HOCHSCHILD

Redeeming America: Piety and Politics in the New Christian Right. By Michael Lienesch. Chapel Hill: University of North Carolina Press, 1993. 322p. \$45.00 cloth, \$17.95 paper.

This book examines the political thought espoused by leaders of the Christian Right in their published work. It is a welcome addition to existing scholarship, which focuses disproportionately on the activities, rather than the thought, of the Christian Right. It is also a corrective to a voluminous literature produced in different academic disciplines, which skewers the Christian Right without much pretense of taking its viewpoints seri-

ously. Lienesch notes that the movement has often been "[viewed] as intellectually insignificant and dismissed as a moralistic abnormality" (p. 20). This book objectively probes the philosophic underpinnings of the Christian Right, expounding its worldview while also unmasking its inconsistencies and outlandish claims.

The introduction is a synopsis of the contemporary Christian Right. The reader familiar with the literature will not find much new here except for the argument that the Christian Right is more than a garden-variety social movement, because it offers a comprehensive worldview "based on beliefs and inspired by values" (p. 20). Another theme here is the periodicity of conservative religious political engagement. This discussion is informative, but others have examined the subject in greater detail—such as Ted Jelen's Political Mobilization of Religious Belief (1991) and Clyde Wilcox's God's Warriors (1992).

The next five chapters compose the heart of the book, looking at the Christian Right's political thought "in the context of a series of spheres, beginning with the self and extending outward in concentric circles to include the family, the economy, the polity, and the world (p. 21). The chapter on "self" focuses on the importance of the individual's converting to Christianity, with its concomitant political consequences. The notion of conversion explains a major division among religious conservatives between those restricting their focus to saving the individual soul and those extending the idea to redeeming a morally corrupt society. Conversion also accounts for the Christian Right's fervor: the imperfect person must constantly strive to overcome-his own sinful nature or society. This chapter draws heavily from the biographies of televangelists and probably takes their claims about their individual conversions too much at face value (but with no appreciable harm to the argument).

Chapter 2 focuses on "family," and it is a strong point of the volume. Lienesch relates the deep philosophic contradictions about gender roles. Husbands are called upon to be patriarchs but also sensitive partners; wives are to sacrifice for the sake of others but, in doing so, to exert control over familial relations (a classic case of assuming power by refusing power, à la Marbury v. Madison). Ambivalence also exists over sexuality (particularly as to whether women are temptresses or virtuous mothers), with marriage deemed a reasonable litmus test. Feminists seeking a careful rendition of the Christian Right's philosophic and pragmatic objections to their cause will find it here. In a separate vein, the author shows how parents hold conflictual attitudes toward institutions that shape the family. They attack public schools for their secular orientation, for instance, while making sure that their own children receive a sectarian, private education. The difficulty of reconciling literal Christianity with contemporary outlooks is skillfully covered in this chapter.

Chapter 3 uses the language of Reaganomics to describe views toward the economy. Christian Right leaders favor trickle-down economics because it recognizes the principle of creating more wealth to benefit all, rather than redistributing money to reward the indolent. They advocate a supply-side theology that justifies the accumulation of wealth as a reward for hard work and as recognition of God's blessing. Lienesch shows how parables are tortuously interpreted to rationalize wealth, while passages attacking the rich are glossed over. This

chapter complements work on the awkward relationship between secular and religious conservatives, found in places such as E. J. Dionne, Jr.'s Why Americans Hate Politics (1991).

Chapter 4, on the "polity," analyzes the overtly political tracts written by religious conservatives. Many of those works interpret history, arguing that America is part of a providential plan gone awry as religious conservatives have left the political arena to secularists and humanists. (Hence the need to reestablish a "moral majority.") The most peculiar aspect of the Christian Right's political thought surfaces in this context, namely, the identification of a humanist conspiracy. Lienesch demonstrates that leading figures proceed beyond the proposition that secular ideologies have grown more important into the bizarre claim that a concrete number of humanists control public affairs. He further shows how that outlook abets different orientations: "At times, [Christian Right leaders] act as a morally conservative majority, committed to reclaiming mainstream values; at others, they are a militant minority, a pragmatic group of power politicians determined to implement their own political platform; at still others, they are a radical remnant seeking to overthrow the system" (p. 171). Torn between status as a latent majority or a victimized minority, Christian Right leaders present both scenarios. More attention to the type of audience receiving each kind of message would have further strengthened this presentation.

One other dimension of this chapter will intrigue political theorists and perhaps those attuned to politics in the academy. Christian Right leaders deplore the Enlightenment for advancing the causes of secularism and humanism, with Rousseau singled out for inculcating the separation of reason and revelation, as well as laying the groundwork for the French and Russian revolutions (pp. 159, 180). This body of work supports Lienesch's earlier claim that the Christian Right is more than a modern political force: it offers a worldview, complete with people for whom Locke and Rousseau are more important than Clinton. This theme also touches upon contemporary campus politics: Is the Enlightenment the flowering of knowledge, the birth of secularism as religion, or the inflation of the views of Eurocentric white men? In an odd twist, Christian Right leaders and multiculturalists seem to converge in their mutual antipathy for the traditional interpretation of the Enlightenment.

Chapter 5 focuses on the "world," and provides an exegesis of the idea of America as a "city on a hill." It does not break new ground, however—which is also the case with the chapter's anticommunist and eschatological themes. The brief conclusion, on the other hand, provides a thoughtful summary of the movement's past and future direction. Substantive notes and a solid index round out the book.

This book should be read by scholars of the Christian Right, both for understanding and for testable hypotheses. It may also interest those teaching American political thought. Lienesch has been immersed in the published work of Christian Right leaders, and he has produced an interesting and important book.

University of Maine

MATTHEW C. MOEN

Monitoring Government: Inspectors General and the Search for Accountability. By Paul C. Light. Washington: Brookings, 1993. 274p. \$31.95 cloth, \$12.95 paper.

This book, by Paul Light, is a monograph on an institutional innovation in the federal government, namely, the centralization of auditing and investigation functions at the top level of executive branch departments and agencies, with such functions subordinated to statutorily empowered inspectors general. This institutional innovation was achieved legislatively during the late 1970s and implemented administratively during the Reagan presidency. Drawing on survey and interview data collected by Light, as well as the documentary record, Monitoring Government explicates the legislative and administrative processes through which offices of inspector general (OÎGs) throughout the federal government became well endowed and institutionally secure. The book further makes a number of evaluative claims about OIGs and provides some supporting argumentation. Beyond this, the author endorses specific modifications in the institutional design of major OIGs in order to capitalize on their legitimacy, staffing levels, and civil service ethic. The book thus provides information that will be invaluable to scholars who seek to generalize about the politics of public management, and it will serve as a point of reference in debates about both the public policy emphasis on compliance-oriented accountability and the effectiveness of OIGs as a policy instru-

The book's principal empirical generalization is that inspectors general have dedicated themselves to monitoring whether government employees, contractors, and others are complying with federal rules and regulations. In strategies reminiscent of the Federal Bureau of Investigation, many inspectors general chose to focus specifically on catching as many wrongdoers and as much wrongdoing as possible. Light explains the dual focus on compliance accountability and production of statistical accomplishments by analyzing the incentives at work in the federal political-administrative system. If OIGs' main objective was institutional autonomy vis-à-vis their respective departments, they had an incentive to please Congress and the Office of Management and Budget (OMB). Congress could grant them more autonomy by amending the Inspectors General Act; the OMB could override department-level staffing and budget decisions. Offices of inspector general were therefore rationally responsive to congressional and OMB interests. For Congress, the focus on compliance monitoring and "stats" production yielded recommendations for actions that (in comparison with rival approaches to improving accountability) were politically more palatable, jurisdictionally cleaner, less expensive, and quicker to implement. For the OMB under Reagan, outputs of compliance monitoring processes were considered evidence supporting the claim that the administration was winning the war on waste, fraud, and abuse.

A second major empirical claim is that statutory OIGs in departments and major agencies under Reagan became well endowed and relatively independent of the organizations they monitored. They had grown by almost fifteen hundred FTEs by 1986. Autonomy was enhanced by the OMB's sponsorship of inspectors general as a corporative group, allowing incumbent inspectors general across government to screen candidates for

specific posts and permitting a substantial measure of self-regulation. Inspectors general retained the statutory right and obligation to report to Congress without the content cleared by department or OMB officials. In addition, OIGs enjoyed formal and informal means to override decisions made by-and circumvent obstacles imposed by-their department's own administrative staff functions (the only major exception being mandatory reliance on centralized functions for legal advice). Mainly on this basis, the author infers that "the government seemed better equipped in 1989 to catch wrongdoing, albeit after the fact, than in 1978" (p. 221). Under Bush, the autonomy of inspectors general vis-à-vis departments was diminished by changes in both the selection process and the OMB's structure and focus. However, major establishments' OIGs remained sizable and

Light proposes three specific institutional changes intended to enhance the autonomy of inspectors general vis-à-vis departments (an appointments process modeled on the Reagan administration's, a fixed term of office, and no pay-for-performance) and one change intended to enhance OIGs' capacity to conduct investigations (a statutory provision specifically granting their investigators limited power to subpoena testimony). The author argues that OIGs ought to become a principal home for evaluators within the bureaucracy. Among the grounds for this recommendation is the prescriptive claim that they should broaden their approach to achieving accountability and the empirical claim that they have the institutional muscle to secure the needed positions and depoliticize analysis. Although the author expresses serious doubts about the desirability and feasibility of this strategy for increasing and improving evaluative work within the bureaucracy, he concludes that given the current politics of public management, the strategy is nonetheless optimal.

Perhaps ironically, readers of Monitoring Government will remain uncertain, throughout the book, what Light wants to be held accountable for. It is plain that he intended to pledge himself to demonstrate the warranted assertibility of the two descriptive generalizations about inspectors general and their offices just discussed. However, he also states that their "eventual reliance almost exclusively on compliance in their search for accountability reveals a great deal about the politics of administrative reform" (p. 12, my emphasis). Even though other administrative reforms are briefly discussed, the reader is uncertain if it is fair to evaluate the book on how well the author formulates and defends descriptive generalizations at the level of administrative reform. The same kind of uncertainty bedevils the critical consideration of evaluative generalizations about OIGs. Light states that inspectors general and their offices "provide an opportunity to examine the consequences of eight specific 'innovations' in the search for accountability (p. 2). (As revealed in subsequent lines, the statement not merely prefatory.) It is impossible to know what specific evaluative generalizations about OIGs Light proffers as warrantedly assertible (as opposed to simply plausible)—not to mention what standards of evaluative inquiry he wants the reader to accept. Uncertainty on this score is not resolved in the main body or in the notes. If the book is accountable solely for substantiating the two descriptive generalizations discussed, the research project is an evident success. The collection of

data, the culling of information, and the framing of evidence is impeccable.

It may be that the author intended to provide a definitive evaluation of the implementation of the OIG concept. However, specialists in public management and policy analysis are bound to conclude that the work merely engages evaluative questions, failing to wrestle any of them to the ground. In conceptual terms, Light appears to conflate the effectiveness of the policy of reducing waste, fraud, and abuse in government with the organizational effectiveness of OIGs. Within a given policy frame (even a misguided one), OIGs may be evaluated favorably. Light clearly wants to make the case that the policy is largely ineffective (or even perverse), because overseers have reallocated resources from such staff functions as planning and evaluation units to OIGs. This case is plausibly made. However, once a distinction between policy and organizational effectiveness is drawn, it appears that the author barely engages the second issue. Four points are relevant. First, no information or commentary is provided about the basis upon which OIGs claim to have put funds to better use as a result of their operations. Since this category greatly surpasses criminal recoveries as a source of savings, one cannot begin to know whether OIGs' major indicator of effectiveness is more than mere rhetoric. Second, no consideration is given of whether inspectors general attempted to limit the adverse consequences on agency operations of their own investigations and auditing work. Third, operating and performance standards of the "compliance industry" were not brought to bear in making inferences about organizational effectiveness. Did OIGs implement formal or informal programs to encourage auditors working on civil enforcement cases to nominate cases for criminal investigation before they became stale? How much enforcement presence (an element of deterrence) was created as the result of auditing and investigation work? How efficiently was enforcement presence created—as compared, for example, with the auditing and criminal investigation functions of revenue agencies? How well did OIGs utilize the "visible odium" of criminal convictions for deterrence purposes? Finally, taking into account valid excuses provided by institutional and personal incentives, did OIGs focus effort in ways that could reasonably be seen as maximizing economy and efficiency (however defined)? Absent plausible answers to these kinds of questions, one remains ignorant about the organizational effectiveness of OIGs.

Harvard University

MICHAEL BARZELAY

The Constitution and American Political Development: An Institutional Perspective. Edited by Peter F. Nardulli. Champaign: University of Illinois Press, 1992. 238p. \$39.95 cloth, \$14.95 paper.

The United States as a Developing Country: Studies in U.S. History in the Progressive Era and the 1920s. By Martin J. Sklar. New York: Cambridge University Press, 1992. 328p. \$49.95 cloth, \$14.95 paper.

These two books are fascinating contributions to the growing literature synthesizing politics and history, though each for very different reasons. Martin Sklar's *United States as a Developing Country*, is a collection of articles representing over 30 years of his work develop-

ing new perspectives about American political institutions. The panoramic view presented by one of our most provocative intellects includes his hitherto unpublished essay, "Dollar Diplomacy According to Dollar Diplomats," which dates to his Wisconsin University master's thesis in the late 1950s; articles published in the 1960s, and more recent contributions. In addition, he composed an original opening chapter especially for this volume, "Periodization and Historiography," in which he forcefully argues that U.S. society cannot be studied "without reference to its *political* history" but, rather, must be envisioned as a developing nation whose evolutionary stages reflect the common need of all industrializing societies to come to terms with the dual forces of "capitalism and socialism" (pp. 10, 36).

As we know from Sklar's influential Corporate Reconstruction of American Capitalism, 1890-1916 (1988), he explicates the underpinnings of the American state in terms of market forces defining the parameters of political development. His collection of essays is a "must read" because it provides a more complete understanding of the nuances and scope of his own development as a noted scholar of American political change. Yet despite the author's commitment to political institutions, it leaves us somewhat in the dark about how major political structures are actually connected to political evolution over time. The book's index, for example, contains no entries for "Congress," "Supreme Court," or "Presidency." Similarly, while it lists only 6 pages in reference to some form of "constitutionalism," topics related to "corporatism" get 95, and "investment," 28. While this is not surprising, given Sklar's well-known proclivities, it nonetheless presents a somewhat skewed account of the political history of American institutional develop-

To appreciate the conjunction between political structures and subsequent historical processes defining American state development, we would do well to turn to the excellent volume edited by Peter F. Nardulli, *The Constitution and American Political Development*. In an innovative enterprise, Nardulli gathered together "recognized American politics scholars" who were "not constitutional experts" (p. viii). He then asked this eminent group of nonconstitutional experts how "constitutional factors" influenced the development of Congress, the presidency, the bureaucracy, the Supreme Court, political parties, interest groups, and federalism in the context of American political history (p. viii).

The result is extraordinary. The essays are uniformly well written and informative, and they address the main theme with exceptional coherence. Taking, as a starting point, that the Constitution embodies "the external point, that the Constitution embodies "the external standards that define American ideals" (p. 15), each author analyzes the impact of constitutional provisions upon institutional components of the American political system. Superb chapters on Congress by David Brady and Charles Stewart trace with clarity and precision how partisan politics mesh with legislative structures to translate electoral preferences into legislated policies. Bert Rockman draws upon marketplace analogies to analyze insightfully the evolution of the presidency within the confines created by constitutional provisions, and Lawrence Baum and Kermit Hall provide incisive appraisals of Supreme Court activism and federalism, respectively, as vital component of public law—to cite just a few of the many fine essays in this collection.

The historical turn taken by many scholars interested

in the confluence of politics and history extends statecentered analyses of political systems and their policy outputs. Launched by new institutionalism approaches a decade or more ago, the focus on how the presence or absence of state structures influences the policy articulation and accomplishments of societal actors not only "brought the state back in" but often resulted in significant cross-national comparisons between the United States and (usually) Western European nations. While often only implicit, these analyses had, as their subtext, historical premises about American political development that the current concentration on politics and history helps to explicate.

In this context, both of these books make important contributions. Martin Sklar highlights the relationship between the economic foundations of society and its political development, even as he all but excludes such basic legacies as the constitutional arrangement of political institutions. Yet it is this omission that informs Peter Nardulli's provocative collection. Those teaching graduate proseminars, therefore, would do well to consider both works. They feature contrasting themes, which, when taken together, broaden our understanding of American political development and the diverse intellectual landscape defining its terms of debate.

Northeastern University

F

EILEEN L. McDonagh

The Dynamics of Ethnic Competition and Conflict. By Susan Olzak. Stanford: Stanford University Press, 1992. 271p. \$32.50

Olzak offers an ambitious and insightful view into the workings of group conflict. Her contributions in this volume are both theoretical and empirical. She focuses her attention on the historical period from 1877 to 1914 in the United States. During this period, there were massive waves of immigration into this country, as well as internal migration. There were also periods of economic turbulence, accelerated industrialization, evolving and strained relations between races, and labor difficulties. There was violence as well, both white-on-black as well as against Asian and European immigrants. In short, this period contained a rich brew of conflict situations that makes it a perfect setting for the current study.

Olzak employs data from daily editions of the *New York Times*. She has collected a valuable body of data from a close reading of the actual newspaper articles rather than using the more limited information found in the *New York Times Index*. She identifies conflict situations from the largest 77 cities in the United States, and collects both qualitative (i.e., the type of conflict) and quantitative (i.e., how many people were involved) data for all observations. Olzak combines this information with additional social information relevant to the time period and pertinent to particular geographical regions. Her dominant empirical strategy is to use (wisely, in my view) event history analysis to unravel the major puzzles of the period.

The scope of Olzak's analyses is quite striking. She analyzes the influences of immigration and economic contraction on ethnically related events. She focuses particularly on labor unrest as it relates to violence against African-Americans. This later expands in the book to an analysis of lynchings and general urban

violence. The competition for jobs is an area that adds to her economic analyses. Finally, in a chapter co-authored by Elizabeth West, Olzak analyzes the birth and mortality of ethnic newspapers in relation to incidents of violent ethnic conflicts. Her results throughout, and particularly in the chapter on ethnic newspapers, are not always intuitive. Indeed, it is precisely because she comes up with so many logical but nonobvious conclusions that her work is so valuable.

Olzak's basic argument is that competition is the driving mechanism behind most ethnic conflict. She challenges previous theories that suggest that ethnic conflict arises from things like relative deprivation, modernization and development, and assimilation difficulties that result in threats to an existing ethnically defined balance of power. One of her primary hypotheses is that ethnic conflict arises not from the separation and relative deprivation of groups, but rather from the breakdown of the former social order that kept the groups apart. It is when groups mix that they begin to compete. Essentially, they compete for the same limited resources, and Olzak's perspective is not unlike that of a typical population biologist who sees much of the world in terms of competing species.

Olzak's arguments lead to some highly interesting observations. For example, she claims that violence against African-Americans can result when another group (i.e., not African-Americans) migrates into an area, thereby increasing the competition for jobs among white Americans. Thus, it is not necessary for the in-migrating group to experience the conflict (although that may happen initially), even though that group's activities may be the ultimate cause of the struggle. Conflict with the initial immigrants can subside while whites escalate their attacks on African-Americans. All of this is connected to the idea of competition for resources (e.g., jobs) between members of differing ethnic groups.

From an empirical perspective, Olzak makes her points using both tables and some useful plots. I find her empirical analyses to be both competently executed and creatively designed. However, one of her primary contributions is in the realm of theory. Her view is one that is rooted in time-dependent dynamics, not static social conditions, and I am certain that all scholars will find something either to praise or to contest among her many findings and conclusions. Ultimately, she is not saying that humans fight because there are injustices in the world, but rather because the injustices are in evolutionary processes of decay that cause groups to interact in new ways. To use a meteorological comparison, wind (read "conflict") occurs not because there is a high pressure area next to a low pressure area. Rather, wind begins when the ridge that separates the two pressure systems begins to break down and the air masses from the two systems begin to mix.

Some may even find that Olzak's views challenge their basic notion of human rationality in situations of collective conflict, since her perspective could be interpreted as more oriented along the lines of stimulus-response than in action based on decision-making and the availability of information. In short, Olzak has written an extremely interesting book, filled both with the intelligent analysis of data as well as theoretical conclusions that are both provocative and compelling.

The publication of *The Dynamics of Ethnic Competition* and *Conflict* serves a useful secondary purpose. Many

(but by no means all) of the results in this book have appeared in article form in a variety of settings. Olzak's prolific writings needed to be organized into a coherent thesis, and this book does this nicely. She has an interesting theory about group conflict, she argues her points effectively, and she leaves the reader with much to ponder. It is a book to be recommended to all those interested in the social structural mechanisms of volatile group interactions.

Emory University

COURTNEY BROWN

Out of Order. By Thomas E. Patterson. New York: Knopf, 1993. 301p. \$23.00.

This well-written book explores the mass media's influence in presidential elections. No previous study better documents the difference that media-dominated campaigns have made to American politics. Full of epigrammatic insights, this study combines exhaustive empirical research with trenchant normative observation. Out of Order argues that the media and especially television have replaced political parties as the chief screening agencies for presidential candidates. The results are baleful. The year 1960 was the turning point. Television news began drawing large audiences, and the parties started relying heavily upon primaries to select convention delegates. The parties abdicated much of their king-making function, the media generated more authority in the winnowing process. Simultaneously, they have increasingly portrayed presidential candidates as irredeemable liars and hypocrites.

Patterson argues convincingly that journalists fail to distinguish between trivial, politically necessary, or correctable dissembling (exaggerating opponents' liabilities, denying adultery) and lying that undermines the basis of elections. The latter occurs when candidates deliberately deceive voters as to their policy intentions. Patterson's research systematically reveals that contrary to the media's suspicions, elected candidates normally do keep their word: they pursue and often deliver the policies they promise.

According to Patterson, media coverage nonetheless assumes and conveys that essentially everything candidates say is motivated by politics. As such, journalists hold, it can safely be categorized as manipulative sloganeering. This assumption bolsters the the well-documented tendency for campaign journalism to focus on the horse race—who's ahead, who's behind, what strategies are working, what new strategies are planned. Even when reporters discuss issues, the view that candidates' positions are just part of the game plan permeates coverage. All this not only reduces the public's ability to learn about the candidates' public character, record, and policy plans, but it trains Americans in cynicism.

Among the book's most useful features is a series of tables tracing aspects of coverage from 1960 through 1992. One documents the growing analytical orientation of campaign news. No longer is a detached voice the norm. Reporters habitually air opinionated evaluations of candidates. The assessments focus not on substance but on the quality of campaign gamesmanship. The data suggest that descriptive material comprised over 90% of front-page coverage in the *New York Times* in 1960; by 1992, description accounted for under 20% and interpretation over 80%.

The historical-trend data also show that the proportion of stories framed in terms of policy has plunged from over 50% in 1960 to less than 20%, while game or horse-race coverage has risen from 45% to about 80%. During the same period, the balance of good versus bad news about candidates has shifted from 75% good in the earlier campaign to about 60% bad in 1988 and 1992.

There is always room for criticism of coding decisions: What exactly comprises good or bad news? Might the nature of good and bad news vary with candidates and campaigns? Is horse-race news not sometimes intertwined with issue coverage? But Patterson marshals much evidence supporting the basic validity of his argument. For example, in the same 1960–92 period, the proportion of the public responding unfavorably to both major party nominees has gone from about 20% to over 50%. The length of sound bites on television has shrunk markedly; and, Patterson shows, so has the length of candidate quotes in the *New York Times*. In all media, the derisive, prognosticating voice of the reporter is subsuming the policy voice of the candidates.

One specific example that Patterson cites is a report on candidate Bill Clinton's 1992 campaign speech to a labor group. A network report featured a sound bite of Clinton saying, of the North American Free Trade Agreement (NAFTA), "I'm reviewing it carefully, and when I have a definitive opinion, I will say so. It's a very long and complex document." The reporter then says: "Time out! Clinton has a reputation as a committed policy wonk who soaks up details like a sponge, but on an issue which will likely cost him votes no matter what side he takes, the onetime Rhodes scholar is a conveniently slow learner" (p. 6).

Patterson points out that the NAFTA document was 1,078 pages long and argues: "To suggest . . . that the 'policy wonk' Clinton should know every part of it and flatly accept or reject it in its entirety is disingenuous. Worse than that, it is fatuous. It assumes that having a fixed opinion on every aspect of every issue . . . displays true leadership, and that any hesitation or objection on a candidate's part is a practiced deception" (p. 16). This passage exemplifies the straightforward and lucid normative analysis that the book weaves through its empirical narrative.

The author believes that because the press filters innovative policy proposals through the cynical horse-race schema, the news often punishes candidates for talking in earnest about policy. The coverage tends to portray debate about substantive proposals as campaign-threatening controversies. A classic example was McGovern's proposed negative income tax: originated by Milton Friedman, the media treated this proposal as a gaffe, helping to stigmatize McGovern as a leftist radical. At the same time, notes Patterson, the media frequently chastize candidates for avoiding honest, original, detailed issue stands. Damned if they do and damned if they don't, candidates begin resembling the cautious beasts that the media construct.

No book is perfect. While Patterson's own contentanalytical material is enlightening, he sometimes buttresses his claims with others' questionable data. For example, he cites research claiming that only 2% of the Fall campaign coverage in 1992 focused on the "character" issue for Bill Clinton (p. 195). Others might have coded the many mentions of "family values," "trust," Gennifer Flowers, "Slick Willy," and similar phrases as oblique or direct comments on Clinton's character. The book would have been even stronger had Patterson stuck to his own research data, the most pertinent and convincing. Nevertheless, his eloquent reasoning and masses of evidence establish a compelling case. Patterson is careful not to blame individual journalists or news organizations for the state of electoral politics. He recognizes the interlocking structural causes of the malaise. He notes that journalism is only part of the problem and that the parties' decline has thrust upon the media a function they simply cannot carry out. The book's proposed remedies are, as always, debatable, but they are worth considering. If I were to recommend one book among the dozens on media and U.S. presidential elections that have appeared in recent years, this would be it.

Northwestern University

ROBERT M. ENTMAN

The Supreme Court and the Attitudinal Model. By Jeffrey A. Segal and Harold J. Spaeth. New York: Cambridge University Press, 1993. 390p. \$54.95 cloth, \$17.95 paper.

This is an important book by an important pair of scholars of the Supreme Court. It has already engendered a great deal of controversy, even among those who generally agree with the line of reasoning; and it will accordingly create even more controversy as the proponents of other approaches to the study of the Supreme Court find and read this book. Those who adopt this model as a guide to understanding the Court will swear by this book. For others who take sharp issue with it, I suspect it will become a book many will love to hate. Few who read it will come away with neutral feelings.

Segal and Spaeth take up the cudgels for what they call the attitudinal model of decision making on the Court and take direct aim at competing understandings of what motivates the justices to behave as they do. The main competitor is what they call the legal model. The attitudinal model, according to Segal and Spaeth, "holds that the Supreme Court decides disputes in light of the facts of the case vis-à-vis the ideological attitudes and values of the justices" (p. 65). By contrast, proponents of the legal model see judicial decisions as a response to the facts of a case in light of "plain meaning, intent of the framers, precedent, and balancing of societal interests" (p. 64).

So what motivates the members of the Court? Is it precedent, plain meaning, and the rest of the litany, or it is raw policy preferences? If I had to choose between the two, I would indeed take the latter. But Segal and Spaeth's views, at least initially, are extreme, with few subtleties. They really do not set up any realistic competitor to their model of decision making. I can think of no political scientists who would take plain meaning, intent of the framers, and precedent as good explanations of what the justices do in making decisions. Some political scientists and many lawyers think it would be nice if justices did follow precedent, the framers' intent, and so on; but that is another matter. If there is no position other than the attitudinal model and the silly formalism of the legal model, the debate is over. Yet no one who has taken Introduction to American Government, read Marbury v. Madison, or witnessed the fights over nominations to the Court during the Reagan and

Bush years is going to ascribe to the legal model. Thus, it is really not a worthy foe, and much of Segal and Spaeth's discussion of it is beside the point. Segal and Spaeth's real foes are the many political scientists and lawyers who would belittle the analysis of votes and say that we have to look at opinions as a whole. But, peculiarly, the authors do not make them the main target of the book.

Segal and Spaeth's formulation of the attitudinal model strikes me as too blunt. Justices, like other political actors, are not free to translate their preferences directly into policy in any and all situations. Înstead, the justices maximize their policy preference under the constraints of law, policy, and custom. Liberal justices, for example, do not grant certiorari on trivial cases simply to reverse a conservative outcome in the lower courts: they grant it on cases in which some live legal issue is present and in which some plausible case for meeting the Court's rules can be made. Of course, this is true of other political actors; and the distinctions many make among judges, legislators, and bureaucrats are exaggerated. All policymakers work within constraints; and, in this sense, judges are no different. Later in the book, Segal and Spaeth seem to take constraints into account; and, of course, they know that the justices do not simply enact their preferences. But the blunt formulation of the attitudinal model pops up again and again.

Chapters 4–7 constitute the heart of this book, the main evidence for the attitudinal model. In these chapters, Segal and Spaeth take up and present systematic evidence on staffing the Court, gatekeeping, decisions on the merits, and the distribution of opinion assignments. The evidence is plentiful and convincing.

The chapter on staffing shows how ideological considerations come into play from the very beginning, from the choice of the president all the way through the decisions of senators to vote for or against a nominee. The larger the ideological distance between a senator and a nominee, the likelier he or she will vote against a nominee. Questions about the qualifications of a nominee make a difference, but ideological distance magnifies the effect of this variable; that is, the larger the ideological distance between nominee and senator, the bigger the difference made by questions about qualifications.

the difference made by questions about qualifications. In the chapter "Getting into Court," which is largely a review of prior work, Segal and Spaeth present impressive evidence of the ideological nature of the justice's decision making. For example, a justice is substantially more likely to vote to grant certiorari who disagrees with the outcome in the lower court. For decisions on the merits in the law of search and seizure (chapter 6), Segal and Spaeth show the strong impact of ideological considerations above and beyond the factual circumstances of cases. In chapter 7 ("Opinion Assignment and Opinion Coalitions"), they offer a wealth of evidence on the impact of ideology on how the chief justice distributes opinions among his colleagues. For example, although chief justices strive for equality of workload, they tend, on average, to assign more opinions to those closer to them ideologically and fewer to those furthest from them.

I do not see how anyone can come away from these middle chapters without granting the tremendous impact of the ideological positions of the justices. "Ideology," "attitudes," "policy preferences," "single-minded seeking of policy"—whatever one wishes to label this phenomenon—pervade the decisional processes of the Supreme Court. Thus, if we take the purposiveness of

judicial behavior as the basic proposition of this book, I think the authors have succeeded.

Segal and Spaeth, in my view, try to do too many things here. To account for every facet of the Court in an empirical fashion in one book is a tall task: any one part could easily serve as the fodder for several monographs. Their insistence on full coverage—from staffing to compliance-sometimes precludes the full development of an aspect of the Court. The large number of topics makes for a distracting busyness. Then, too, some of the chapters do not fit well into a book of this kind. Segal and Spaeth set out a theoretical framework juxtaposed against the legal model and test it against several sets of data. Then, in the middle of the book, they give us a political history of the Court to show how ideological considerations have underpinned its decisions. I would be the first to sign up for the proposition that ideological considerations have, from the beginning, played an important role in the Court, but this short history proves nothing. Actually, this is the standard stuff of politicalscience reading of constitutional law going back at least to Corwin and Powell. If we were to look back at the first editions of Mason and Beaney's American Constitutional Law and Cushman's Cases on Constitutional Law, I would bet a large amount of money that we would find similar accounts of the same decisions. Similarly, in two later chapters, Segal and Spaeth go through the evidence on compliance with, and implementation of, the Court's decisions and the relationship between the Court and other actors in the political system. In chapter 9, on compliance and implementation, Segal and Spaeth review the existing evidence; in chapter 8 they present a number of descriptive tables. Neither of these chapters has much to do with proofs of the attitudinal model of judicial decision making. Perhaps the real problem is that this is both a scholarly monograph and a textbook. At least it has the components of both kinds of books.

Despite my reservations, this is a fine and important book, one I will certainly consult often. There is much to commend this book. First of all, it contains a wealth of information about the decisional processes of the Court, from the legal framework through the nitty gritty of voting, opinion assignment, and the like. Oddly, given the theoretical stance of the authors, this book is particularly strong on the legal framework of decision-making in the Court. Second, the authors adopt a clear theoretical stance. No theoretical waffling mars these pages. One may or may not agree with that stance, but it is out there, along with the data to support it; and others now have a clear target. Third, Segal and Spaeth use this book to bring together a great deal of work published in a variety of journals and to provide new data and results on a number of points. Fourth, since the Supreme Court Data Base Phase I supports most of the analyses, anyone who wishes to challenge the results or conclusions or to extend the work can easily do so with this public dataset.

Ohio State University

GREGORY A. CALDEIRA

The Politics Presidents Make: Leadership from John Adams to George Bush. By Stephen Skowronek. Cambridge: Harvard University Press, 1993. 526p. \$29.95.

What are the determinants of presidential leadership? Why are some presidents widely considered "great,"

while others range in historical reputation from mediocre to disastrous failures? These questions have, in one way or another, been central to the literature on the presidency, some of it centering on its constitutional function, some on the getting and keeping of real-life power, and some on the psychological qualities of the men who hold the office. Quite a bit of this literature has been framed in terms of a great wall of separation between the modern presidency since 1933–45 and the (mostly irrelevant) presidencies before then.

For Stephen Skowronek, the fundamental issues are embedded in a Constitution that, unlike society and economy in the United States, (and, for that matter, unlike the empirical operations of politics) is, in its foundations, unchanging, static, timeless and "nondeveloped"—the core of what Samuel Huntington has called the American "Tudor polity." In this world of separate powers and clashing ambitions linked to "the prerogatives of the place" (as Madison put it), the issues confronting presidents go back to the very beginning and not just to 1933 or so. More than that, there is a dialectic at work: the interplay between constitutional nondevelopment and political (and other) development in the United States produces regular cycles. Regime orders come into being in the wake of a massive, crisis-laden upheaval sequence. They persist at first robustly and then, over the longer term, more and more vulnerably as intramural infighting takes its toll and the development of society and economy creates new problems and cleavages that the existing order is ill equipped to meet. Finally, each in its turn, perishes in the next punctuational-change crisis, to be replaced, out of the ruins, by the next regime order and its transformational (Skowronek: "reconstructive") leadership. Such cycles are iterated across the entire span of chronological time. This means at least two things: (1) the contexts of presidential power are anchored in "political time," and (2) presidents occupying the office in comparable moments in political time may be profitably compared with one another, even though they may be separated by decades in the passage of chronological time.

This portion of Skowronek's model in principle permits classification of American presidents from the federalist era to the present day on two dimensions: their affiliation with, or opposition to, a given regime order and the extent to which that order is robust or vulnerable. This, in turn, gives us a fourfold classification of individuals in their contextual and political-time setting that has been well known in the field since the author zpublished his famous article on in 1986. The four types of political practice are (1) reconstruction (the great regime overthrowers and builders), opposed to an acutely vulnerable order; (2) articulation, affiliated with a robust order; (3) disjunction, affiliated to a collapsing order; and (4) preemption, opposed to a robust order. In the bulk of the text, the author systematically examines presidents and their contexts of action in the first three categories, from Thomas Jefferson's reconstruction to Jimmy Carter's disjunction.

But something else is superimposed. What forms the driving force behind presidential inputs to the world they inherit and attempt to shape is, in the first place, the inherently disruptive force of the office and its equally inherent hostility to all governing arrangements existing at the time that a given president first takes office. The keynote here was given by James K. Polk: "I intend myself to be President of the United States." If, as

Skowronek says, the office is simultaneously ordershattering, order-affirming, and order-creating in its properties, even weak incumbents tend to create significant disruption as they seek to affirm their own indentities in office. A classic case, discussed in detail in these pages, is the disjunctive presidency of Franklin Pierce. Order shattering is never far away.

Beyond that, there is not only political but secular time. If political time centers on presidential power, secular change in the mode of government operations affects, cumulatively, presidential authority. This unidirectional evolution falls into four eras that in general (if not always in detail), would probably gain wide consensus: the Patrician Era (1789-1832), the Partisan Era (1832-1900), the Pluralist Era (1900-1972), and the Plebiscitary Era (1972 to date). Thomas Jefferson is the purest and most complete example of reconstructive leadership because he operated in a near void of organized opposition at a very early moment in the development of social (and thus governmental) complexity. Over time, the growth of this complexity has produced a government that is so "thick" on the ground that its inherent resistiveness to presidential initiatives has made presidential achievement of reconstructive purposes more difficult and problematic with each new iteration of the power cycle. By the time we reach the present, impasse becomes the consequence—a major feature of the discussion of the presidencies of Ronald Reagan and George Bush, with which the work draws to a close.

Without any question, The Politics Presidents Make is the most ambitious effort at sustained pattern recognition ever attempted in this area of study. The overwhelming bulk of the book consists of a detailed, "thickdescription" effort to concretize the categories by looking at presidents in action in both political-time and secular-time contexts. It is a truly extraordinary effort at integration, synthesis, and comprehensive pattern recognition amid the myriad details of the historically contingent and the diversity of character, talents, and virtues of the men who served in this role. In general, at least for me, the argument works. Skowronek is remarkable not only for his erudition and accuracy in detail but also for the subtlety of his reflections on his cases. This is a work that is striking in its sensitivity to nuance and-above all, perhaps-in its keen awareness of the ironies of presidential action. At very many points, quite new analytical possibilities are raised and evaluated. To take just two examples, presidents in a disjunction situation recurrently tend to reify technique as substance becomes impossible to handle, and presidents pursuing the politics of preemption recurrently tend to risk or achieve de facto or de jure impeachment.

Needless to say, this study, important as it is, does not lie beyond criticism. Any effort to construct classificatory categories of this type is likely to disclose concrete cases that just do not fit. Skowronek himself identifies "three hard cases": Cleveland, Coolidge, and Eisenhower. While I see little difficulty in classifying Coolidge's general place in Skowronek's greater scheme of things, Cleveland's case is somehow associated with a realignment in which (uniquely in American history) the forces demanding change were defeated. Eisenhower "ought" to fit as a preemptor (the New Deal order was still robust, and he and his party were opposed to it); but he did not behave that way in office and also lacked the modal preemptor's temperament. (In earlier times, one could say the same of Millard Fillmore.) Real life is likely

to produce impure mixtures in concrete cases that classification schemes have difficulty accommodating. Yet if the model does, in general, seem to describe real-world trends and tropisms, such "hard cases" suggest less the need to discard the former than to work through the latter's implications.

Two more serious questions arise, both centering on preempting presidents, whom Skowronek calls "the wild cards of American politics," who have historically come to power because the normal screening or gatekeeping machinery for presidential selection has broken down for one reason or another. Inherent presidential disruptiveness in these cases has often verged toward the boundaries of the constitutionally permissible: Richard Nixon is just the latest of such cases in Skowronek's scheme. Yet the author, at the end, calls for a presidential strategy of "perpetual preemption" henceforth. In this context, it is particularly to be regretted that this book contains no detailed examination of at least one representative of this fourth category. To be sure, pretty much by definition, preemptors stand, in a sense, outside the cyclic pattern. But these cases illuminate issues directly specific to the author's closing argument, and at least one of them (Nixon?) should have been included accordingly.

Skowronek is not alone in concluding that within the past quarter-century, changes so fundamental in the organization and articulation of American politics have occurred that traditional historically derived precedents and models are becoming irrelevant to understanding what is going on in the here-and-now. From his perspective, the thickening of government has reached the point that presidential reconstructive activity is now, or soon will be, out of the question. Incumbents keep acting on cue from an old script but with diminishing effectiveness and increasingly bizarre consequences. Better, then, that they should give up acting out such roles and the public give up waiting for "Godot"—a new reconstructive president to bail them out of the mess their public affairs have got into.

Each president should thus now become his own preemptor. (Is Bill Clinton actually following this new script?) From such a change, a whole catalog of desirable consequences will follow: "Once shorn of the roles acribed to them in political time, leaders will in fact have little else to rely upon but reason, talent, ideas and character" (p. 445). In short, Skowronek's subtle dialectics of presidential power sum to an even more striking dialectic: the negation of an analysis that has occupied more than 400 pages up to this point.

One finds oneself struggling with two reactions to all this. First, one wonders, preemption for what—in behalf of what substantive values and concrete interests and coalitions in the society that elects these people every four years? In general, there is a tendency—ever more marked as we approach the present in these pages—to present an analysis that is so Oval Office—oriented that still-vital contexts of presidential action such as party and partisanship virtually disappear from view. And we do have that old problem with preemptors that Skowronek clearly identifies: How does the system keep the power drives of these figures within constitutional limits, particularly if the constraining power of other institutions continues its decay?

Second, how sturdy a support can this ideal preemptor gain from his pure qualities of "reason, talent, ideas and character"? They were just John Quincy Adams's

frame of reference for conducting his presidency. They seem more generally (as proposed unmediated drivers of the enterprise) to reflect our frequent preoccupation with eliminating politics to achieve pure policy "in the public interest"—a Progressive Era impulse that is with us still. While many factors were involved in Adams's defeat for reelection in 1828, his efforts to remain far above "mere politics" in the conduct of his office helped mightily to do him in. If there is any one sentiment among the enlightened that ought to be given up as we move into the next century, it is not the fixation on 'great presidents" as saviors but the striving finally to achieve some nonpartisan, nonpolitical ideal for government and its officials. Short of dictatorship, politics cannot be gotten rid of. For this reason, the preemptive virtues that Skowronek identifies in his heroic effort to find a way out of the present impasse would seem to be as effective a ticket to quick retirement for incumbents embracing them as it was for Adams.

All that said, however, it should also be said that any weaknesses this book contains pale into insignificance when measured by the magnitude of the accomplishment it represents. This is a magisterial work, one of the most important studies of the presidency-indeed, of American politics-ever written. Stephen Skowronek has made the most persuasive of cases for the value and significance of work in the American political development genre. He makes this case by doing the work and by revealing, in the process, a dense network of connectedness in action and context that would otherwise be inaccessible. His analysis of the impact of secular time on the presidential power cycles of political time provides (in addition to other things) a new and deeper insight into the present era's political impasse. At the end of the day, he comes very close to identifying the root problem affecting presidents, which prompts him to the sort of escape he then suggests. This is the allimportant fact that the Constitution is unchanging and nondeveloped, while at all times intersecting with a social, economic, and political world that has undergone incessant development from the beginning. The whole work may be read as an extended, powerful, and penetrating meditation on some of the global consequences of this fact.

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The Scar of Race. By Paul M. Sniderman and Thomas Piazza. Cambridge: Harvard University Press, 1993. 212p. \$18.95.

It is not possible to understand American politics without eventually engaging the question of race. Of course, the centrality and character of debates on the status of African–Americans in the polity have varied enormously. Yet since before the founding of the republic, the "race problem" has been near the core of American national politics. Issues of race figured in the drafting of the Constitution, the enslavement of blacks weighed upon the consciences of Washington and Jefferson, Tocqueville's powerful chronicle of American society dealt extensively with race, a struggle between a race-based slave economy versus a free-labor economy precipitated the deadliest war in the American experience, and a range of constitutional debates (e.g., the scope and intent of the Fourteenth Amendment) and

modern-day civil rights protections (the Civil Rights Act of 1964 and Voting Rights Act of 1965) can be traced to efforts to protect the status of African–Americans.

In the long shadow of the American political history of race, Sniderman and Piazza's Scar of Race aims to tackle afresh the linkage of prejudice and public opinion. Their focus is on the modern era. Their method is the sample survey conducted by phone, both local (San Francisco Bay Area) and national in scope. The research is driven by an effort to understand whether the politics of race has somehow changed. To assure wide accessibility, the data reporting is deliberately kept simple, rarely reaching beyond figures or tables based on simple percentage comparisons or correlations. On the whole, Sniderman and Piazza's effort is, variously, illuminating and clever; yet, on occasion, it disappoints and frustrates. Let us consider several useful contributions made in The Scar of Race.

First, the research helps to specify the current complexity of the issues involved in racial politics. At an empirical level, Sniderman and Piazza find no tight clustering of key issue attitudes. An individual's opinions on affirmative action in employment are not likely to be interchangeable with the same person's views on open housing laws or level of economic assistance, if any, that government ought to provide to black communities. The relative lack of constraint immediately implies that there is no single, profound thread of prejudice that binds together the views of white Americans on these public policy questions.

Second, the book is a useful normal-science extension of the use of experiments within surveys. Following the lead of Howard Schuman, who first applied survey-based experiments to theoretical questions in the study of contemporary racial attitudes, and the complex survey vignette studies of Peter Rossi and his colleagues, Sniderman and Piazza exploit the power of modern computer-assisted telephone interviewing systems to develop ever more complex experimental designs. One clear lesson of their efforts is that survey questions directed at delivering benefits to a black individual do not elicit a racial double standard in behavior of the type that emerges strongly when benefits are proposed to be delivered to blacks as a group.

Third, the book makes clever and often enlightening use of a measure of anti-Semitism as its bedrock indicator of prejudice. Doing so simultaneously reveals the modern political potency of prejudice and escapes entirely the analytical ambiguity of meaning that often attaches to questions with explicit race-based content. Fourth, the book is one of several recent efforts to reintroduce a concern with stereotyping into sample surveys. It provides valuable data on the persuasiveness and the delimited (but quite real) political effects of antiblack stereotyping. Fifth, Sniderman and Piazza undertake a very provocative examination of the malleability of opinions. They use counterarguments to attempt to gauge the fixity of opinion on race. The results show a potential for great movement, although this movement has a clear-cut predominant trend.

Most frustrating, however, among the weaknesses of *The Scar of Race*, is a serious misreading or misinterpretation of the *new racism* thesis as advanced by such scholars as David Sears, Donald Kinder, and John McConahay. Sniderman and Piazza typically treat the new racism thesis as contending that core American values, such as individualism and the work ethic, rather than

constituting weapons against racism, are now the main engines of racism and that this new racial bias operates with special force among political conservatives. Neither component of the argument seems a fair depiction of the theory advanced by proponents of the new racism thesis. First, the latter maintains that a new form of expressing racial animus has emerged. A hallmark feature of this new racism is a more explicit concern with such values as the work ethic, though this is intimately fused with feelings toward blacks. Sniderman and Piazza's own research strongly supports at least part of this claim. For example, they show that even among the highly educated, believing that "if blacks would only try harder, they would be just as well off as whites" or that "most blacks on welfare programs could get a job if they really tried" are powerful direct determinants of attitudes on economic assistance to blacks-exactly as the new racism thesis would predict.

Furthermore, to address the second component of Sniderman and Piazza's treatment of the new racism thesis, I know of no survey-based research suggesting that this new racism operates with special or differential force among ideological conservatives. Hence, while it is quite useful to show that whatever antiblack bias exists

when policies focus on group benefits is not unique to conservatives, this finding does not challenge the core of the new racism argument. The new racism thesis has many weaknesses, but this research has not taken us far toward resolving them.

Less troubling but still problematic is the inattention to group conflict theories as an alternative to both the new racism thesis and Sniderman and Piazza's politicization-of-race theses about modern race politics. Conflict models have considerable power to explain the difference found between popular receptivity to individual benefits versus collective or group benefits. Moreover, it might help explain why the tests of opinion malleability find the greatest movement on the group benefits question and that the trend of movement is disproportionately from pro- to antibenefit delivery.

Despite several limitations, *The Scar of Race* adds a needed perspective. It will advance discussion among specialists in the area and contribute to current efforts to understand the tenacious forces of race and racism in national politics.

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LAWRENCE BOBO

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COMPARATIVE POLITICS

Hostage to Revolution: Gorbachev and Soviet Security Policy, 1985–1991. By Coit D. Blacker. New York: Council on Foreign Relations Press, 1993. 268p. \$16.95 paper.

Coit Blacker has produced a compact and very readable book that reviews the turbulent changes in Soviet foreign and security policy under Gorbachev. He writes in a well-organized and accessible manner, effectively using subtitles and verbal "road maps" to help the reader along. On the whole, Blacker presents a balanced description of the Soviet policy revolution that led to the end of the Cold War. After reviewing the Brezhnev legacy, he examines the effect of Gorbachev's "new thinking" in four areas: the conceptual basis of Soviet policy, military doctrine and strategy, arms control, and civil-military relations. In both style and content, the book is quite suitable for an upper-level undergraduate course on the history of Soviet or Cold War foreign policy.

At the same time, Hostage to Revolution is much weaker at an explanatory level, doing little to advance the scholarly debate. Despite the author's claims in the book's introduction, he presents a rather standard Sovietological analysis. Blacker has read a wide array of (translated) Soviet sources and, based on these and his understanding of the changing content of policy, offers a plausible reconstruction of Gorbachev's motives and reform strategies. Keeping with the Sovietological tradition, the book is also for the most part atheoretical. In other words, Blacker is at his best describing the content and implications of the Gorbachev-era changes in security policy. He is much weaker at explaining the why and how behind the reforms.

Indeed, Blacker's explanatory focus—economic crisis as the motor of change—is so simple and self-evident that the reader is left with a lingering question of "So what?" What insights are gained from an argument of this sort? Blacker rightly criticizes (in both his introductory and concluding chapters) international relations theory as being poorly equipped to anticipate or explain peaceful systemic change such as that brought about by the Gorbachev reforms. Yet he does not offer or elaborate a domestic-level alternative of his own, except to suggest that "economic crisis" is central and all other explanatory factors are essentially derivative (e.g., pp. 190–91).

As Blacker's story unfolds, we are told at various points that ideas matter, institutions are important, and leaders learn. Who would disagree? The analytic challenge is to explore the conditions under which such factors become relevant. To do this, however, requires greater attention both to domestic politics and theory. How and why did certain ideas reach Gorbachev and come to inform the content of his "new thinking"? If the old ideas on international politics were so deeply embedded in the pre-Gorbachev system (as Blacker persuasively argues), how could they be overturned with such relative ease once a new leader came to power? Blacker is right on the mark to argue that economic crisis "opened up" Soviet politics. To stop there, however, is

to leave many of the most interesting questions unanswered.

It is fair to ask whether the domestic political process under Gorbachev could have been explored in the detail necessary to address such questions. The answer is *yes*, but not with the sources Blacker has utilized. For reasons that are not entirely clear, he has chosen to rely almost exclusively on U.S. government translations of the Soviet media. Aside from the evident problem of selection bias (a point Blacker never addresses), reliance on such materials intended for wide consumption seriously hinders the author's ability to reconstruct the process behind the "new thinking."

Much, in fact, has been revealed through Soviet/ Russian materials not typically translated by the U.S. government. One scholarly journal, for example, has published the transcripts of the Reykjavik summit, a consideration of which would have strengthened Blacker's interpretation of this key episode (pp. 104-8). More important, a number of key figures have published memoirs (former Chief of the General Staff Akhromeyev, academic specialist Georgiy Arbatov, advisor Anatoliy Chernyayev, and former Politburo members Ligachev and Ryzhkov, to name only a few). While such works must be read with caution because individuals maneuver to secure their place in history, they nonetheless offer many remarkable insights. Beyond written works, one can, of course, "go to the source" and interview the architects of this remarkable revolution. While Blacker mentions his many helpful conversations with Soviet specialists (p. 8), his notes give no sense of what he gleaned from the interviews or how such information was integrated into the analysis.

To appreciate how neglect of domestic political process hinders Blacker's analysis, compare Gorbachev's foreign policy with that of post-Soviet Russia. Economic crisis, Blacker's key explanatory variable, is present in both periods. Yet in today's Russia, the "new thinking" has come under withering attack, while a different set of ideas more in keeping with traditional notions of power politics has come to the fore. Russian foreign policy, reflecting this combination of old and new, has become a strange mixture of realpolitik and liberal internationalism.

How to explain these changes? Clearly, part of the answer is the dramatically different international political environment of post-Soviet Russia (especially important here is what Russians call the "near abroad"—the successor states to the USSR). Equally important, however, are fundamental changes in the domestic setting; the process has opened up in ways unheard of even at the end of the Gorbachev era. Indeed, in Russia today, the problem faced by promoters of new ideas is not getting access to top decision makers (as it was in the USSR) but ensuring, once they reach elites such as Yeltsin, that the ideas have some lasting influence on policy.

Blacker's main concern, however, is the Gorbachev period, where the challenge is to get some sort of analytic leverage on an extraordinarily complex set of changes. The way forward is to incorporate, and expand upon, the work of scholars such as Matthew Evangelista and Jack Snyder, both of whom have sought to build much-needed bridges between international relations theory, comparative politics, and "Soviet" studies. Indeed, if recast with more explicit attention to theory, Blacker's study would have been much sharper analytically, with the potential for contributing to several different research programs in international relations and comparative politics. These include work on policy ideas, civil-military relations, negotiation/bargaining theory, and (more generally) domestic sources of state behavior. As written, however, the book leaves the reader to tease out any connections to the broader literatures.

In short, Blacker has written an informative if limited book. Students and members of the policy community will find much of use here. Hostage to Revolution is a good account of six years of revolutionary change. However, the reader who is interested in new interpretations that draw upon new source material and contribute to the broader social science literature will have to look elsewhere. Perhaps reflecting a field ("post-Soviet studies") in transition, Blacker's study is, ultimately, a mixture of good and bad. It is better than many Sovietological analyses yet still falls short on both methodological and theoretical grounds.

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Parliamentary Change in the Nordic Countries. Edited by Erik Damgaard. Oslo: Scandinavian University Press, 1992. 224p. \$39.95.

Coalition Politics and Parliamentary Power. By Mats Sjölin. Lund: Lund University Press, 1993. 230p. \$21.50 paper.

These recent books on parliaments and parliamentary practices are the products of a persistent endeavor, on the part of some Nordic scholars, as well as research milieus, to study the impact of institutional forces in political life. Although clearly not flying the flag of the behavioral movement during the sixties and seventies, they continued presenting the kind of basic research that other scholars took for granted and that today enjoys a modest revival among Nordic political scientists. Damgaard's edited volume and Sjölin's monograph are different both in scope and intent and will have different value to different readers. They share, however, the same analytical background in contemporary comparative parliamentary research—even though the moorings are not quite similar.

In the Damgaard volume we are presented with accounts of recent parliamentary trends in Denmark, Norway, Sweden, and Finland sandwiched between an introduction and a comparative analysis, both produced by the editor, who is also responsible for the chapter on Denmark. The analysis of the three remaining parliaments are written by country specialists: Dag Anckar, Hilmar Rommetvedt, Anders Sannerstedt, and Mats Sjölin. According to the editor, the purpose is to analyze important trends in the parliamentary development of the Nordic countries during the past few decades. Criteria for "importance" is partly centrality within the comparative literature discussions, partly political importance within the countries in question. In practice, the "decline-of-parliaments" thesis functions as a kind of overarching point of reference, even if explicitly "not

the main object" of the analysis (p. 16). The editor is also evidently skeptical as to its research potential.

Parliamentary analysis deals with both internal developments in parliament and its relations to the external world, particularly the executive. Starting with Danish experiences during the 1980s, contextual particularities immediately create problems for comparative analysis; for these experiences are indeed quite remarkable to those used to a fairly straightforward distinction between government and opposition. In Denmark during the Schlüter "four-leaf clover" government (1982-88), the very terms evaporate when analyzing the dealings in the Folketing. There were "alternative majorities" in several policy areas—most notably in security matters. this means that the minority coalition government, headed by the conservative Schlüter, accepted its minority position in several fields and let the "opposition" determine "government" policies. The era of "the governing parliament" came about because one of the parties not in government—the radical liberals—wanted the government to continue and the government was willing to pay the necessary price.

The chapters on the four country developments outlines the major parliamentary trends over the last two decades. They all focus on changes along the parliament—government axis, as seen from parliament. The historic accounts reveal that the old label for the Scandinavian region ("consensual democracies") has to be reconsidered. In short, there is more open and fierce politics on the parliamentary scene than there used to be in the 1950s and 1960s. Still, we also get the proper warnings against jumping to the quick but unwarranted comparative conclusions. Sannerstedt and Sjölin argue, for example, that in Sweden the "image of consensus is less salient, although political negotiations continue to be of great importance" (p. 99).

In his summary, Damgaard points to three substantial changes: weaker governments, increased parliamentary activity, and more parliamentary power. There is a decline of consensus but not a decline of parliament. We have, however, the Finnish exception—not, according to Dag Anckar, that Finland has gone through a decline in parliamentary power (though that belief is widespread) but that Finnish governments are increasingly majority governments. Due to legislative procedures that give one-third of the members the right to postpone and, in fact, "torpedo" almost any government proposal (p. 161), the threshold for a sustainable government majority in the Finnish parliament is 67%, not 51%. As Anckar points out, this gives the term "oversized majority" a quite different meaning in the Finnish case.

The Damgaard volume is highly recommended as an updated analysis of Nordic parliamentary trends. The country chapters are somewhat uneven, but all provide the kind of information useful for anyone aiming to keep a professional eye on developments in Nordic politics. The style and presentation would also make it suitable for courses on Nordic or West European politics. Some rather standard queries could, however, be raised about the book if viewed exclusively as a comparative project. The country presentations are bound together not by any coherent analytical frame but by a rather loose set of questions. The empirical material varies according to availability and author interests. Rommetvedt for example is particularly keen on the analysis of party dissents in the committee recommendations in the Norwegian Storting. More unnecessary, perhaps, is the individualistic approach to theories and conceptualization. Inevitably, therefore, the comparative discussion by Damgaard toward the end is nurtured mostly by the

empirical qualities of the book.

Coalition Politics and Parliamentary Power is by Mats Sjölin, the coauthor of the Swedish chapter in the Damgaard volume. Its theme is the role of parliament and parties in political decision making. Empirically, it is a book about Sweden, but he also argues the theoretical case that the theory of political decision making could invigorate the study of parliaments and parliamentary practices in general. And although the decline-of-parliament thesis is much in focus in this book too, Sjölin's view is that this debate "is in need of theorizing, which applies to parliamentary research in general, and in need of specification, which applies to the question of decline in general" (p. 202).

The problem to be analyzed in Sjölin's book is "power relations and patterns of cooperation and conflict between the political parties in the struggle over the framing of policies" (p. 26). The empirical basis for this is the reports of the committees in the Swedish Riksdag regarding government proposals. The period covered is from 1970 to the early 1990s. Two empirical questions recur repeatedly throughout the book. First, how can we explain government-opposition processes in Sweden? Second, has the power of the Swedish parliament declined? It turns out that the opposition muscle is about as strong (or weak) in Sweden as in comparable Western democracies. Sweden is at least no deviant case in this respect. There has been a trend toward a higher level of changes in government proposals, enforced by the opposition, but equally striking is the ebb and flow in this relationship, connected with government base and party constellations in the Riksdag. There is no convincing evidence to substantiate the parliaments-in-decline thesis. In choosing between remaining alternatives—stable or increased parliamentary power-Sjölin argues that the ups and downs of parliament during the latter two decades leaves the issue unsettled. Still the weakening of governments may indicate a change toward increased parliament power. The question, however, is whether this derives from temporal fluctuations or lasting structural change.

Sjölin's eclectic approach in developing his theoretical argument makes refreshing reading. Here we also find his main mission, namely, to widen the field of traditional coalition analysis to include discussions about parliamentary decision making and coalition politics. The fatal fault of the comparative literature on government formation and parliamentary politics alike is the lack of trespassing—in other words, the insufficient amount of theoretical cross-fertilizing—within the tradition. Sjölin sets himself the task of bringing together various approaches at least in the sense of considering, within the same empirical analysis, the fruitfulness of coalition theory of the public choice tradition, the mainstream comparative government tradition, and "modern power theory."

Does he succeed? The aims are admirably ambitious, and Sjölin clearly has a point in attacking the compartmentalization of much research within this field. His discussions of the various approaches are fair and balanced. Still, there is not much new in, for example, his criticism of the formal coalition theory. At times, the presentation and discussion of the literature could have been shortened. The exercise takes a more fruitful turn

when Sjölin comes to the bit in which he himself has more faith—the power theory, or tradition of power analysis. Employing concepts and distinctions like "autonomy" and the various "faces of power," he manages to put across the inadequacies in the debate on the decline of parliaments and also to present the complexities of parliamentary analysis in general. From the many shrewd observations on the exercise of power in parliamentary politics it also appears that Sjölin feels more comfortable among the foxes than among the hedgehogs in the community of scholars. The trouble with that position, however, is (as all foxes know) that it does not make the world easier to understand.

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KNUT HEIDAR

China's Quest for National Identity. Edited by Lowell Dittmer and Samuel S. Kim. Ithaca: Cornell University Press, 1993. 305p. \$42.50 cloth, \$16.95 paper.

This remarkably coherent symposium should command broad attention throughout our discipline because it illuminates in numerous ways the confusing complexities and uncertain relationships among such supposedly self-evident concepts as state, nation, country, culture, society, and civil society and such basic sentiments as nationalism, patriotism, and a variety of national, social, and political group identities. With embarrassing regularity, scholars will labor to define these concepts but then, in the pages that follow, slip into treating many as synonyms. The differences are important because political systems function quite differently depending upon the relative strength of each entity and the emotional attachments of their populations. Although this book is largely devoted to examining the Chinese case and although Chinese details, with the peculiar spelling of names, can be overwhelming for nonspecialists, there is enough food for thought in it to enrich the theoretical understanding of generalists, especially comparativists and theorists

Indeed, the two editors in their joint introductory and concluding chapters explicitly address larger theoretical issues of national identity. In the process of relating the concept of national identity to political development theory, they argue that "the substantive content of national identity is the state and that the state defines itself by what roles it plays... in the international arena" (p. 17). Fortunately, however, they do not abide by such a narrow, state-oriented concept of national identity. (Possibly because the chapters are jointly authored, they are enlivened by a few harmless contradictions.)

The Chinese case—involving, as it does, a distinct civilization—is particularly helpful for highlighting many of the fundamental issues associated with national identity. The issue of whether the Middle Kingdom, a bureaucratic empire headed by emperors, should be classified as a nation–state highlights the problem of arriving at a proper definition of the nation–state. What does it mean that in modern times the Chinese "state" has undergone dramatic changes but the Chinese people have remained seemingly secure in their "Chineseness"? Is a powerful sense of cultural identity that does not involve the state a real form of nationalism? If these constitute valid questions, then maybe Michael Ng-Quinn is a bit too quick and easy in calling the ancient

dynasties nation-states. Michael Hunt picks up the historical account with a review of China's reaction to the modern West and concludes that "the preoccupation with state power stood—and still stands—as one of the single features of Chinese national identity" (p. 76). His skillful recounting of the history does drift a bit from the question of China's "quest for national identity," so it is unclear whether the desire for a "strong state" reflects a weak sense of national identity or, rather, a sense of cultural identity that was so strong that the Chinese became hypersensitive to any sign of being inferior. Unquestionably the challenge of modernization forced many Chinese, like people in other non-Western societies, to question their traditions. In their confusion over why China was weak and the West seemed so powerful, Chinese intellectuals became obsessed with the idea that China needed a "strong state." Few Chinese intellectuals would have appreciated George Unwin's observation in Studies in Economic History: "The expansion of England in the seventeenth century was an expansion of society and not the State. Society expanded to escape from the pressure of the State." In contrast to the intellectuals, ordinary Chinese have generally known that things are always better, especially economically, when the political authorities are not overly strong. What neither the intellectuals nor the common people have ever understood is that China never developed an effective "civil society." It lacked a tradition of autonomous interests, engaged in articulating competing policy demands.

With respect to this deficiency, it is significant that Merle Goldman, Perry Link, and Su Wei start their interesting coauthored contribution on the current role of the intellectuals by noting a student's slogan at Tiananmen, We Love Our Country but We Hate Our Government, which showed that the students were able to distinguish between "state" and "society"—something that has historically eluded most Chinese intellectuals. The anthropologist James Watson clarifies a great deal by suggesting that in pinning down national identity, we should not necessarily focus so much on matters of beliefs but recognize that in some cultures identity is based more on valuing everyday practices, particularly ones with ritual overtones. That is, praxis counts for more than ideology. (Could it be that the Christian tradition has left secular Western scholars with an exaggerated sense of the importance of beliefs, so that we assume the durability of Communism in China depends upon whether, in their hearts, the Chinese really "believe" in the ideology when, in fact, it may be enough that they continue to act as though they believed?) In any case, an awareness of shared practices is apparently more important in defining Chinese national identity than questions of deep inner feelings and values. This view is in part confirmed by Richard Wilson, who reports on recent research findings that current Chinese socialization practices produce both continuities and changes in what it means to be Chinese.

The question of Chinese national identity is of growing political importance because it is widely assumed that the void created by the erosion of Marxist-Leninist-Maoist thought will be filled by Chinese "nationalism." But what is the content of that nationalism likely to be? The glib idea that economic growth can solve China's identity problem is challenged by Lynn White and Li Cheng, who show that the people in the economically dynamic coastal provinces continue to have identity problems. China's more active role in foreign affairs has

also been of limited help, as Peter Van Ness demonstrates in his analysis of Beijing's attempt to identify with the Third World and Robert Scalapino in his recounting of China's relations with her neighbors.

In short, therefore, it is far from self-evident that the Chinese have a set of shared symbols, values, and myths that might be easily brought together to shape a Chinese self-image that could both guide domestic development and be projected in foreign relations. Elsewhere in the world, the end of the Cold War has forced us to reexamine the importance of various forms of primordial group identities. Good scholars that they are, the authors of this book have sought to build on the earlier work on political development and identity crises. (At the risk of seeming to be petty-minded, I must point out that there are some inaccuracies in their understanding of the contributions of the Social Science Research Council's Committee on Comparative Politics, but on the other hand it is good that they have recognized the significance for political analysis of Erik Erikson's theories.) What is important is that all who want to study the increasingly significant question of nationalism and national identities will have to build on this work.

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Making a Market: The Institutional Transformation of an African Society. By Jean Ensminger. New York: Cambridge University Press, 1992. 212p. \$49.95.

Jean Ensminger has written an excellent book that addresses several different audiences. She is an anthropologist, but much of what she has to say is addressed to economists, sociologists, and political scientists. She "combines the individual-actor approach of economics, anthropology's appreciation of institutional constraints, incentives, and ideology, and the attention to power that we associate with Marxist analyses" (p. 1). The fact that the book cuts across so many different disciplines is one of its principal merits. The author draws from these different fields not ad hoc but in a systematic fashion. She argues that only through a synthesis of these different fields of study is it possible to obtain an effective understanding of how market forces contribute to social change. Her main objective is to show how rational choice and new institutional economic theories can be powerful analytical tools in explaining the process of change among the pastoralist Galole Orma of northeastern Kenya.

Ensminger argues that the best way to understand the process of change among the Orma is to go beyond the static analyses of classic economic and anthropological theories. She situates her analysis of the Orma within the analytical framework of Douglas North's new institutional economic perspective. The advantage of this perspective is that it assumes that institutions have an important effect on the options available to, and taken by, individual and social groups. Two key assumptions, in particular, inform the perspective. The first assumption is that transaction costs are not zero, so that change of any kind usually involves some costs. The second assumption is that economic exchanges are shaped by more than price and quantity: there is an underlying normative (qua ideological) dimension that cannot simply be dismissed on the grounds of being not quantifiable. For anthropologists and social scientists interested in socioeconomic and institutional change in the developing world, these two assumptions are important because they highlight the fact that in the short and medium term, economic change might generate more costs than benefits.

In her book, Ensminger sets out to explain how the spread of market forces transformed labor, social, economic, and property relations among the Galole Orma. She relies on historical and longitudinal data of a group of households to show the asymmetric effects that the penetration of market forces have had on the Galole Orma. By employing both the new institutional economic perspective and the ethnographic approach of anthropology, she is able to analyze the process of institutional and ideological change within the Galole Orma community. In particular, she shows how these changes gradually led to a shift within the Galole Orma community away from pastoralism toward a more sedentary community of cattle herders.

Her application of the new institutional economic perspective to the study of the process of change among the Galole Orma is generally convincing. For political scientists, the most striking claim made by the author is the one made in chapter 6. There she argues that "in the aftermath of increasing economic growth and resultant economic diversification among the Orma, there was a breakdown in community and a failure of collective action" (p. 143). While she is quite persuasive in her argument as to why there was a breakdown (particularly because of the increasing inability of elders to enforce property rights), she overdoes her case when she claims that the failure of collective action at the community level was effectively supplanted by the state in Kenya. Her attempt to explain state formation within the context of new institutional economic theory becomes dangerously mechanistic at times. Her argument that the benefits offered by the state through its increased institutional penetration of peripheral communities such as the Orma have led to growing acceptance of the state assumes a rational outcome that is difficult to sustain in Africa today.

The fact that the penetration of market forces and state institutions have had a significant impact on pastoralists like the Galole Orma is undeniable. To make the claim, however, that "the lessons from this case study of state incorporation speak also to the issue of state formation, and more generally to the relationship between economic growth and the breakdown of communities, and the institutions that support communities, in this case marriage, lineage, clan, gerontocracy, patriarchy, and patron-client relations" (p. 143) suggests that the author does assume that change always moves in the direction of increasing efficiency or economic growth. Although she recognizes that there have been losers, particularly the nomads, she implies that there has been a convergence of interests between the state elite and Orma elite and nonelite alike.

The main point is not that the penetration of market forces and state institutions has not reshaped the Orma community and led to a breakdown of traditional patterns of collective action. What is at issue here is that this process of change in Africa rarely, if ever, involves the complete breakdown of "lineage, clan, gerontocracy, and patron-clinet relations." In most instances, these elements continue to influence institutional change and have an impact on economic development. Whether

elites use these quasi-traditional elements to enhance their own political and economic positions, or the elements persist because institutional and economic change is "lumpy" and reflects multiple asymmetries, the outcome in Africa, at least in terms of institutional and economic development, has generally been a suboptimal one.

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DWAYNE WOODS

The Politics of Food in Mexico: State Power and Social Mobilization. By Jonathan Fox. Ithaca: Cornell University Press, 1993. 280p. \$39.95.

The sun has not set on hopes that state bureaucracies in developing countries can implement reforms biased in favor of the poor. This hope persists despite the relatively sad record to date. The literature is replete with examples of reforms that have been stillborn or sabotaged or have produced unintended and even damaging consequences. Constraints on reformers with grand objectives are considerable. They must convince political leadership to back their change strategy. They must devise the policy with little information on how it will fare in practice and show quick results to maintain momentum. They must be highly skilled in managing the administrative, political, economic, and ideological dimensions of the new policies until they are selfsustaining. Reform policies, however, raise a red flag to supporters of the status quo. The policies divert resources from existing programs, whose committed supporters in the bureaucracy and civil society oppose and undermine the changes. The poor are typically unorganized and offer few resources to support the reformers other than a promise to accord legitimacy to the regime if the reforms work. Economic cycles inevitably squeeze the government treasury, and political leadership changes-resulting in a review, and sometimes abandonment, of policy just when the new program might be showing accomplishments.

Jonathan Fox confronts each of these issues head-on in a theoretically relevant, well-documented, and sensitive analysis of food policy in Mexico. His case study is the Sistema Alimentario Mexicano (SAM), a rural-development-cum-food-distribution policy instituted under Mexico's Lopez Portillo presidency (1976-82). In 1980, the Mexican government was alarmed by rapidly rising food imports but was not about to scrap the 1917 peasant landholding legislation, which, through the sacrosanct collective farm unit (the ejide) suboptimized domestic food production. José Lopez Portillo gave status and backing to a young team of agrarian reformers who had new ideas: make peasant producers the prime beneficiaries of state subsidies in seeds, fertilizers, credit, technical assistance, grain purchase, and water, while extending the state food distribution network to remote rural areas. the goals were to increase production, save foreign currency, reaffirm the viability of the peasant farmer, raise nutritional levels, and enhance political legitimacy for the regime from groups who, seven decades before, had been the footsoldiers for the Mexican Revolution.

The reformers ran into typical problems: opposition from agricultural bureaucrats who wanted all subsidies to go to large farms in irrigated areas; decline in highlevel backing as Lopez Portillo became a lame-duck president; a state-centered political system in which peasant organization was inherently weak; and sharply reduced state finance after the 1982 debt crisis. Fox traces the orientations and capabilities of all the major actors in this drama (e.g., DICONSA, a food distribution agency) and concludes that the policy was most effective where reformers succeeded in organizing the peasant beneficiaries in the context of the reform itself. For example, newly created community food councils used official regulations, shrewdly drafted by the reformers, to hold DICONSA agents accountable for delivering food to rural stores where peasants could buy at subsidized prices.

This research forms part of the literature voguishly called the "new institutionalism." Fox eschews the label and deals industriously with the timeless concerns of organizational effectiveness and responsiveness. The book quite adequately explains the position of the major players in the complex Mexican political system, which helps show why the reform took the course it did. Fox dismisses what he sees as a dichotomy between state-centered and society-centered analyses of bureaucratic behavior and posits the *interactive* alternative, which combines the two. An example of an interactive finding would be change instituted by elites which impinges on beneficiaries, who constitute themselves into groups, which ally with the elites to create a reform coalition.

This analytical concept is linked to a suggestive strategy for reform mongering. The "sandwich strategy" is not a food handout but, rather, an objective alliance between entrepreneurial reformists and autonomous social movements to offset the power of entrenched authoritarian elites. The "sandwich strategy" is the SAM reformers' contribution to praxis, skillfully articulated by Fox, who quite rightly claims that the strategy has cross-national implications. Scholars and practitioners alike are well advised to study Fox's findings in this realm for theoretical and practical guidance, and chapter 7 (which studies the technique) would fit well into any course on policy formulation and implementation.

One criticism is that Fox neglects to discuss how food policy and peasant agriculture were affected by subsequent events. After the Lopez Portillo government, agrarian reformers vainly waited for a "Son of SAM" to rescue the collective-landholding ideals of the Mexican Revolution while allowing Mexico to feed itself. Under Miguel de la Madrid (1982-88), the Mexican state was penniless and ignored the peasant farmer. Carlos Salinas de Gortari (1988–94) then amended the 1917 Constitution in a way that many analysts believe will end the ejido and permanently change the context for official policies toward the rural poor. The value of Fox's contribution is thus not his description of the SAM but the expertness with which he uses Mexican food policy to draw important theoretical and practical lessons for state-induced change. The findings, which emphasize targeted organization of the poor more than class mobilization, are salient in one degree or another for Asian, African, and other Latin American cases, as well as for traditionally neglected groups in the United States and other richer countries. The Politics of Food in Mexico is a fine example of a case study grounded in exhaustive field research. It addresses real world problems in a theoretically innovative way.

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Peter S. Cleaves

Nations of Immigrants: Australia, the United States, and International Migration. Edited by Gary P. Freeman and James Jupp. New York: Oxford University Press, 1993, 250p. \$18.95 paper.

Is the process of state-formation among "nations of immigrants" distinctive in some important ways? Strange as it seems, despite the considerable weight attributed to immigration in explaining the political, economic, and social development of the United States and the small number of other countries that fall within this category, little attention has been devoted to the more general question. And perhaps even more surprisingly, historians and social scientists concerned with particular countries within the category have seldom availed themselves of the possibility of comparison to test their observations, or at least enrich them by engaging in the sort of counterfactualism that is rendered possible by relatively similar experiences elsewhere. As the editors of the present work remark in their introductory essay, "One of the very few serious attempts to understand settler societies in a comparative framework is Louis Hartz's The Founding of New Societies," an edited volume published thirty years ago, when "political culture" reigned unchallenged (p. 9). But although the Hartz collection contains some stimulating essays, surely even the most conservation-minded among us would agree the field has remained fallow long enough to stand vigorous cultivation.

Although less ambitious, the present volume is a well-executed collaborative examination of the sources and consequences of recent developments in immigration policy in the United States and Australia by scholars from both countries. It is a stimulating beginning in a promising area of research, and contains valuable suggestions for more comprehensive projects along the lines noted.

Comparison, here, serves not so much to achieve broad generalizations as to provide a better understanding of each case by highlighting its specificity. The introductory chapter by the editors sketches the distinctive historical configurations of the two countries, highlighting differences as well as similarities. The juxtaposition of the two cases brings to mind that the notion of "nation of immigrants" should be made time-specific, reflecting a combination of objective criteria (e.g., the rate of immigration in relation to population) and subjective perceptions. In this perspective, the United States was not a "nation of immigrants" at the time of the Founding, but became one from about 1830 until it acted very effectively to change its character in the 1920s; Australia in effect foreclosed its transformation into a "nation of immigrants" by closing its borders in the 1890s, before the arrival of a significant flow of continental Europeans and Asians. Both countries reopened in the late 1960s; but although Australia is today much more a "nation of immigrants" than the United States (about one out of five Australians was born abroad, as against only one out of twenty Americans), immigration into the United States has been and remains a source of much greater ethnic diversity. Despite the broadening of sources of immigration in the post-war period—first to encompass southern and eastern Europeans, and later Asians—as of 1986, 75% of the Australian population was of British or Irish descent; and immigrants of "English-speaking background" still account for nearly half of the annual intake today.

The chapter also provides some intriguing theoretical suggestions regarding the role of immigration in shaping the very different patterns of working-class formation in the two countries. Overall, Australia emerges as a "might have been" California—if the transcontinental railroad had not been built—i.e., an isolated white labor commonwealth that managed to maintain control over membership until the middle of the twentieth century, and thereby shape a distinctive path for capitalism and state-formation. The chapter might have been further enriched with some maps, tables, and graphs with basic information regarding historical and contemporary patterns.

Part I provides detailed accounts of immigration policy proper as well as of its underlying political dynamics. Although uninformed American readers will find the chapter by Robert Birrell on Australian policy especially useful, Frank Bean and Michael Fix also provide a very good summary of recent developments on the American side. The comparative analysis by Freeman and Katherine Betts of "the politics of interests in immigration policymaking" is of greater theoretical import. They conclude that immigration generates conflicts between the same sorts of interests in both countries, but their relative weight varies. In keeping with the distinct historical patterns noted, labor remains more influential in Australia; and despite the fact that ethnic groups have been given a corporatist status in the Australian system, they have greater legitimacy as political actors in the United States.

Part II is devoted to the impact of immigration on the economy; reviewing the recent macro- and microeconomic literature, its two chapters concur that, notwithstanding significant differences in the skill level of immigrants, in both countries immigration has had a "Pareto optimal" effect, i.e., it has increased the welfare of the immigrants without generally reducing the economic position of most residents, while substantially benefiting many of them.

Part III, on "Immigrant Settlement," highlights one of the sources of persistent differences between the two countries. From the very outset until quite recently, immigration to Australia was sponsored and funded in whole or in part by government, whereas on the American side (with the major exception of African slaves) "immigration begat immigration," by way of the networks generated by earlier arrivals. Concomitantly, Australian authorities exercised much greater control over the choice of immigrants, and played a more active role in the process of settlement as well, whereas on the American side the immigrants were largely left to their own devices after arrival. By and large this is still the case today in both countries. Consequently, there is an ongoing national debate in Australia regarding the appropriate policy of incorporation, whereas on the American side the subject is much more diffuse. While the Australian situation is well presented by James Jupp, the American chapter by Robert Bach is more problematic. In part, as Bach points out, the very structure of the situation makes it difficult to talk about American "settlement policies"; nevertheless, more attention might have been given on the American side to refugeescurrently, some 130,000 a year-whose settlement involves legally prescribed intervention by all levels of government, and to the extensive activities of states and especially urban governments in this sphere.

Last but not least, Part IV is addressed to what

constitutes throughout the world of industrial democracies the hottest aspect of immigration—the matter of incorporation of culturally distinct groups. Although entitled "Immigrant Minorities and Multicultural Policies," the contribution by Stephen S. Castles, one of the most valuable in the book, amounts to a deconstruction of the doctrine of "multiculturalism," which was launched in 1989 as the official policy of Australia. Contrary to what the term evokes in the American context (and thereby in Europe as well), in Australia multiculturalism "is not defined in terms of cultural pluralism or minority rights, but in terms of the cultural, social and economic rights of all citizens in a democratic state" (p. 190). As presented by Castles, most of the measures under this umbrella are designed to reduce various forms of institutional discrimination non-Anglo immigrants, old and new, face in egalitarian Australia. In short, this "multiculturalism" is highly universalistic in its import, and when applied to immigration policy means that Australia will welcome highly skilled immigrants even if they are Asian. Beyond this, the adoption of multiculturalism as a general policy also entails limited government support for ethnic organizations and ethnic media, features that are highly developed in the United States, possibly because of the longer experience of minority advocacy where a critical mass was present because of sheer size (e.g., the organizational flowering of Jewish and Catholic minorities around the turn of the twentieth century) and the concomitant dynamics of the market.

Although DeSipio and de la Garza provide some suggestive observations regarding incorporation on the American side, particularly on the subject of access to citizenship (which is not covered with regard to Australia), their essay unfortunately does not provide the materials for a systematic comparison with the Australian situation as presented by Castles. Given the acute public concern with incorporation in all contemporary receiving societies, and the conceptual confusion in the proliferating literature on the subject, a systematic comparative analysis of the situation should rank high on our research agenda.

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Environmental Risk, Environmental Values, and Political Choices: Beyond Efficiency Trade-offs in Public Policy Analysis. Edited by John Martin Gillroy. Boulder: Westview, 1993. 189p. \$42.00 paper.

Contributors to the Gillroy volume argue that when traditional market methods, alone, are used to analyze public choices, conflicts over environmental risks often resist resolution. Including original essays by chemists, economists, philosophers, and political scientists, the collection is divided into three sections. Parts One and Two—with essays by Gillroy, Steven Kelman, Robert Paehlke, Vicente Medina, Christopher Bosso, and David Henderson—analyze the nature of environmental values and risks and provide suggestions for handling conflicts concerning them. Part Three, with essays by William Gunderson, Talbot Page, Barry Rabe and Gillroy, and Douglas MacLean, surveys several cases of citizens' NIMBY (Not in My Back Yard) responses to attempts at siting environmentally risky facilities.

Because the Gillroy collection encompasses articles exhibiting a wide range of disciplinary, methodological, and ethical standpoints, there is no specific thesis for which all the essays argue. Nevertheless the excellent introductory essay (chapter 1) by Gillroy provides two general themes addressed by all the contributors. One theme is the complicated nature of environmental risks and values, complications caused in part by the collective nature of many environmental goods. The second theme is the necessity of incorporating environmental values into our analyses of public decisions regarding risk. Although virtually all of the authors do an excellent job of defending and articulating the first theme, Page, Rabe and Gillroy have the best specific suggestions regarding the second theme, how to incorporate environmental values into policymaking. This incorporation is difficult, Gillroy argues, because economic methods, such as cost-benefit and risk-benefit analyses, help to construe the preferences of individuals in terms of a polluter's (prisoner's) dilemma. The dilemma is that, given the strategic reality of interpersonal relations, individual preferences result in a non-optimal collective provision of environmental quality. Incorporating environmental values into policymaking is also difficult, for example, because federal regulatory agencies fight each other in environmental turf wars (p. 82); because the Environmental Protection Agency has little clout (p. 86); because the American political system devalues planning and activism (pp. 90-91); and because the public assesses environmental risk in an unscientific way (p. 106).

Showing how to overcome such obstacles to including environmental values in public decisionmaking, Rabe and Gillroy use a Canadian case study. The case, on siting a hazardous waste facility, provides an illustration of a new strategy for environmental decisionmaking. Instead of modeling such decisions on the prisoner's dilemma, Rabe and Gillroy propose an assurance-game model in which universal cooperation, rather than being a free rider, is the preferred choice (pp. 156–157).

Obviously Rabe and Gillroy theoretically are correct about the desirability of the assurance-game model. The question their analysis raises is how to motivate the players in an environmental game to operate in terms of cooperation strategies. Hence the real question their analysis raises is not whether assurance-game models are desirable, because obviously they are, but whether real situations of environmental conflict can be resolved by following such models. Gillroy admits that using the assurance game—to model decisionmaking regarding environmental risk in NIMBY situations-requires full information, no uncertainty, and no suspicion on the part of any persons involved in the NIMBY situation (p. 166). Because such idealized conditions are rarely met in problematic controversies, and because risk imposers typically withhold information from the public, it is difficult to believe that the conditions necessary for using the assurance-game model are applicable to most NIMBY controversies. Moreover, it is nothing new that analytic models often fail to solve real-world problems. As Gillroy recognizes, cost-benefit analysis falls short in environmental problem solving. What we require now are precise and empirical arguments that show assurance-model strategies are more believable and more realistic than cost-benefit models. For real situations to follow the assurance model, we need evidence about the possibility of motivating persons to behave in cooperative ways. If Rabe's and Gillrov's Canadian case study about siting a hazardous waste facility is representative, then their conclusions about assurance-game models are correct. If their single case study is not representative, then their proposed model may be less promising than they believe. Regardless of which is the case, the book provides us with a heuristically powerful, concrete solution that can be attempted and tested in future conflicts over environmental risk. Moreover, apart from whether the assurance-game model will work in most cases of environmental conflict, Gillroy has done a great service by pointing out that successful models of conflict resolution (like the proposed assurance model) presuppose full disclosure of relevant risk information, unquestionable integrity on the part of all parties to the controversy, and great public trust. Whether or not the assurance model works, in clarifying it, Gillroy has done a brilliant job of exposing the most potent reasons for continuing environmental conflict and for lack of cooperation among parties. If he is correct, the conflicts come not so much from poor or incomplete science but from withholding information and lack of credibility and trust. He has produced a compelling argument for examining political and ethical, rather than merely scientific, components of environmental risk.

As a policy analyst, Gillroy provides a persuasive and clear dissection of collective-goods problems. His essays are among the best in the volume, not only because of the quality of the analysis but also because of their comprehensive survey of types of environmental values and the complexities of environmental risk. This particular asset of the volume makes it useful as a text. Yet another strength of the collection is its insightful and thoughtful treatment of some of the flawed market mechanisms for dealing with environmental risk.

Apart from questions about the real-world applicability of the assurance model, the main shortcoming of the book is that, despite the unity of the Gillroy essays, many of the other contributions in the collection are not tightly connected to each other. Although there are two general themes running throughout all the chapters, the diverse contributions to the book have no common, specific focus. For example, the Henderson essay, on flawed public understanding of science, is outside the general theme of the volume, as is the MacLean essay on conservation versus preservation. There is also some redundancy in the collection, particularly in the discussions of prisoner's dilemmas. Nevertheless, the excellent analyses by Gillroy and the volume's inclusion of original essays by some of the best persons working in the field-Kelman and Page, for example-make it an important book for anyone interested in environmental risks, political choices, and game-theoretic strategies.

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Politics, Society, and Democracy: The Case of Spain. Edited by Richard Gunther. Boulder: Westview, 1993. 272p. \$44.00.

The Return of Civil Society: The Emergence of Democratic Spain. By Victor M. Pérez-Díaz. Cambridge: Harvard University Press, 1993. 357p. \$35.00.

The two books under review here are quite different in scope, but they share a common interest in exploring the

recent history of Spain and the reasons for its successful democratization. The Return of Civil Society, by Victor Pérez-Díaz, is a book that can only be described as magisterial. It moves easily and successfully along the boundaries of history, political science, and sociology, presenting a carefully crafted argument about the democratization of Spain while reflecting intelligently and provocatively on many other subjects. Among these, the conditions for democratic development; the nature of the relationship between social institutions, market, and state; the changing nature of religiosity and the adaptive capacities of religious institutions; and the impact of neocorporatist or territorial arrangements (he refers to these as mesogovernments) on the efficacy of national governments.

Politics, Society, and Democracy makes a different kind of contribution to the literature. Ably edited by Richard Gunther and part of a four-volume festschrift in honor of Juan Linz, this book focuses on Linz's contributions to the study of Spain. Its chapters, written by former students and colleagues, testify to the breadth and intellectual rigor of his work, his collegial spirit, and his capacity (as José Cazorla puts it) to employ Spain as a "paradigmatic" case (p. 71). Although not entirely immune to the problems associated with edited volumes, it is an important book and helps us better understand not only the work of Juan Linz but also important aspects of the Spanish experience in the twentieth century.

Politics, Society, and Democracy is part intellectual biography, part political analysis. In part 1, Amando de Miguel paints an affectionate portrait of Linz, while Manuel Gómez-Reino and two others from DATA, S.A. (the social science survey research firm that Linz helped found) show the link between Linz the citizen and Linz the social scientist, between Linz's commitment to democracy in Spain and his use of Spain as a prism through which "to better comprehend the most important problems of the contemporary world" (p. 11). Miguel Jerez takes up a third dimension in the life of Juan Linz—his role in the development of the social sciences in Spain.

Part 2 focuses more directly on themes related to Spanish politics and society. Among the topics covered in its 11 chapters are a comparison of the labor movements in Spain and Italy (Malefakis), authoritarianism and its legacy (Cazorla), the legitimacy and meaning of democracy (Montero), the role King Juan Carlos played in the democratization process (Polodny), the role of business (Martínez) and labor (Fishman) elites, the evolution of the Basque party system (Llera), and health policy reform under the Socialists (de Miguel and Rodríguez). I should like to draw attention to those chapters whose themes relate closely to the issues discussed in the *The Emergence of Civil Society* by Victor Pérez-Díaz, namely, the role of societal actors and the cultural foundations of democracy.

The role of societal elites in the construction of Spanish democracy is the subject of the chapters by Martinez and Fishman. The Martinez chapter demonstrates the positive role that the business community played in the democratization process. His conclusions clearly support the argument that the diffusion of economic control is an important stimulus to democratic development. (My only quibble with this otherwise excellent chapter is that Martinez fails to analyze in much detail the political role of the banking sector. He is right to call the gran banca "more a polemical than an analytical concept"

[p. 118], but one does not need to have a particularly conspiratorial bent to assume it had an active hand in the disintegration of the UCD.)

Fishman covers the other side of the societal-industrial divide. His chapter is an excellent example of how to explore the differential impact that subnational differences and variations in social structure had on working class radicalism in Madrid and Barcelona. More relevant to our discussion, however, he demonstrates why radical working-class elites in Barcelona failed to retain their influence among a working class that was more moderate.

Cazorla and Montero touch on issues related to the cultural foundations of democracy. The chapter by Cazorla ("The Theory and Reality of the Authoritarian Regime, Thirty Years Later") is extraordinarily suggestive and useful. Not only does it discuss some of the debates surrounding the concept of authoritarianism, but it also explores the "residues" of franquismo, specifically the lingering distrust of politicians and the emergence of what Cazorla terms "mass-level clientelism" and the "collective patron" (p. 83). His conclusion is that "the rapidity of our political change has produced an asynchronism—the survival of the remains of an authoritarian mentality within a minority but visible sector of the population, combined with the inertia of important institutions and improvisation or a lack of scruples by some of their leaders" (p. 87).

By contrast, Montero offers a rather more optimistic assessment of the situation. Focusing on legitimacy and efficacy (two central concerns in the work of Linz), Montero analyzes the so-called desencanto of the late 1970s and early 1980s. He argues that despite lingering positive feelings about Franco (29% of respondents were "sympathetic" toward him in a 1982 survey), the legitimacy of the democratic regime was never in question. Among the chief reasons for this, Montero explains, were that Spaniards, having recently experienced a transition from authoritarian rule to democracy, could make quite "separate evaluations of system performance (efficacy) from support for the current democratic regime (legitimacy)" (p. 155). The outcome of the crisis of the early 1980s may have been partisan dealignment (or, as Samuel Barnes, Peter McDonough, and Antonio López Pina put it several years ago, "volatile parties and stable voters"); but there was (and has been) very little support for antidemocratic solutions or antisystem parties. Montero also advances a second line of argument, insisting there has been a "displacement of the traditional dichotomy liberty-equality by (a) new materialist-postmaterialist cleavage" (p. 166). There is certainly empirical evidence in this regard, but I think Montero might not stress the connections between these "new" values and the "residual" ones inherited from franquismo enough.

The debate over civil society and the cultural foundations of democracy are at the core of contemporary political science debates. And it is in this area that the other book under review here makes its most important contribution. The Return of Civil Society offers an absorbing alternative reading of how and why democracy came to Spain. The heroes of this tale are not the usual cast of the political elites or clase política. "Political transitions," Pérez-Díaz cautions, "can be reconstructed ex post facto as critical choices by the elites, but most often those choices are not so much deliberate calculations as belated reactions to ongoing processes the elites hardly understand, must less control" (p. 7). Indeed, when he reviews critical events (such as those of 1976–77), Pérez-

Díaz emphasizes not so much what the elite did but how the public responded. The new political rules, he argues, were "not so much invented *de novo* as translated, so to speak, from (the) meta-rules of social, economic and cultural life" (p. 28).

For Pérez-Díaz, the "emergence of democratic Spain" is about the "return of civil society," that is, the development of "social institutions such as markets and voluntary associations and a public sphere which are outside the direct control . . . of the state" (p. 57). It was in the civil society, with the trauma of the Civil War and the experience of Francoism as primary points of reference, that a democratic tradition was invented, whence it was eventually transported to the political sphere.

In seeking to explain the emergence of democracy in Spain, Pérez-Díaz finds insufficient those arguments that center either on structural variables (be they economic or cultural, domestic or international) or on elite choices-not that he dismisses those variables, for the reader will find a sympathetic and knowledgeable discussion of their explanatory strengths and weaknesses. (Particularly notable is his discussion of the impact that international variables had on the democratization process. Those who are familiar with the transition literature know how often this factor is ignored.) Rather than dismiss structural or elite variables, Pérez-Díaz proposes to shift their terms of reference; to approach structural factors in terms of the traditions, rules, and institutions at their core; and to view actors "less as choice makers than as rule followers (or as rule deviants or rule breakers)" (p. 26).

It is from this perspective that Pérez-Díaz examines the role of societal groups and institutions (business, labor, and the Church) in the "emergence of democracy" in Spain. His conclusion is worth quoting at length:

The effect of businessmen and trade unions has been greater and more constructive than the rather more modest influence exercised by intellectuals. The image politicians like to put forward of themselves as holding together the world which is breaking apart seems plausible when one compares them with the cultural groups, but much less so if they are compared with socioeconomic groups. The market and industrial development have been important binding forces throughout the Spanish territory for the last century and a half. Business and trade unions have been powerful centripetal forces, tending toward the integration of the national community (p. 209).

The Return of Civil Society is more than just about why democracy "emerged" in Spain. It is also about examining the challenges that democracy confronts (there and elsewhere). From this perspective, Pérez-Díaz explores two sets of issues: (1) the nature of mesogovernments (neocorporatist arrangements and territorial formulas of various types) and the "problematic effects" these structures may have on governability and on the economy and (2) how the nature of the relationship between workers and unions has changed, especially with the breakdown of the post-1945 "social compromise" and the growth of a "rational, instrumental, or experimental attitude" toward unions and parties (pp. 193, 284, 298 and chap. 4). Addressing this last theme from the same "societal" perspective he employed in discussing the "emergence" of democracy, Pérez-Díaz argues that given the "moral and cognitive development" of the workers, "unions and political parties could not but

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(have) followed a strategy of *de facto* accommodation with capitalism" over the past several decades (pp. 311–12).

capitalism" over the past several decades (pp. 311–12). The Return of Civil Society is an eloquent call for "bringing society back in" to the study of comparative politics. Although not necessarily agreeing with everything Pérez-Díaz says, the reader will gain many insights from it. My only complaint pertains to his concluding chapter. I wish he had connected his earlier discussions of clientelism and political corruption more clearly with his later argument about workers, unions, and the social pact. This I say primarily because I wanted Pérez-Díaz to expand on this issue and its relation to governability. Pérez-Díaz has written a superb book that will make a significant contribution to the ongoing debate about the process of democratization.

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EUSEBIO MUJAL-LEON

Apple of Gold: Constitutionalism in Israel and the United States. By Gary Jeffrey Jacobsohn. Princeton: Princeton University Press, 1993. 284p. \$39.50.

Once dismissed as an artifact of eighteenth-century Europe, constitutionalism seems to be enjoying a certain renaissance. New or revised constitutional texts, inescapably accompanied by debate over more broadly constitutive matters, now figure prominently in dramatic political transformations nearly everywhere—all over the post-communist region, in South Africa, in Israel and Palestine, in India and in Canada. Since this new round of constitutional politics follows on the heels of the recent bicentennial of the American constitutional text and Bill of Rights, it is not surprising that constitutional scholarship is blossoming. Gary J. Jacobsohn's book is among the most astute and far-seeing of the current batch of books on comparative constitutional issues. In fact, it is hard to overstate the importance of his book because its significance transcends its immediate subject. He compares Israel and the United States, but his comparison speaks powerfully to the identity crisis of liberalism in a multicultural era.

Liberalism has been entwined with constitutionalism ever since they emerged in the eighteenth century as apparently fraternal twins. So much have their careers been linked that the term "liberal constitutionalism" is often taken to contain a superfluous adjective. But while constitutionalism-somehow "the rule of law" as against "the rule of men"—is riding high, liberalism is under siege. Non-western intellectuals join western postmodernists to attack the notion of individual autonomy rights as a form of naive universalism, and to unmask the ethnic and moral "neutrality" of the liberal state as a covert form of coercion. And many states now face new demands for group rights and for state patronage of religious or ethnic identities. These demands arise out of increasingly multicultural conditions everywhere, suggesting that the very social pluralism which was supposed to necessitate liberalism in the first place now threatens to strangle liberalism with its own equality principle. In the American heartland of liberalism, for example, the crisis is visible in the Kiryas Joel case now before the U.S. Supreme Court. The case involves a challenge to a public school district specifically created to provide handicapped education to a Hasidic Jewish community in New York state. It forces the Clinton Justice Department (which must weigh in before the

Court) to choose between two parts of its own soul—between liberal multiculturalism on the one hand and liberal religious neutralism on the other.

With Jacobsohn's help we see how vastly different such questions appear when considered from the perspective of Israeli constitutionalism, which he calls an "alternative pluralism." In the U.S., pluralism is said to require the state (and its schools) to be scrupulously neutral among rival sub-communities and their values. In Israel, pluralism requires the state to accommodateand empower—the cultural institutions of various religious sub-communities, even while it publicly affirms the centrally Jewish identity of the polity as a whole. The American rule of religious neutralism, if transplanted unmodified, would shatter the identity of the Jewish state, and would be regarded by most Israelis (Jew and Arab alike) as a homogenizing doctrine corrosive of the actually existing plurality of communities. Americans may find such thinking incongruous. But in a brilliant move of internal comparison, Jacobsohn points them to the one "glaring exception" to America's liberal constitutionalist norms: the native American tribes (p. 18). Based on early treaties and their claim to primeval nationhood, the tribes, as communities, enjoy constitutional recognition in a manner more consistent with the Israeli than the American brand of constitutionalism. Most revealing was the 1968 "Indian Civil Rights Act," opposed by native Americans as an assimilationist instrument whose exaltation of individual rights would erode tribal cohesion. Jacobsohn notes that the final version of the Act pointedly omitted any no-establishment-of-religion clause, since the integration of spirituality and public order often constitutes tribal identity. As Jacobsohn says of Israel, such communal autonomy "supports political stability by providing non-dominant (and inassimilable) groups with mechanisms that enable them to minimize the effects of their inferior position in the larger society" (p. 35).

Liberal doubters might dismiss the native American case as a constitutional regime's pragmatic compromise with a marginal anti-constitutionalist sub-community. The bigger question, then, is whether such arrangements qualify as a version of constitutionalism when, as in Israel, they mark the scheme as a whole. Does the Israeli experience represent its own principled form of constitutionalism, as Jacobsohn seems to hold, or does it qualify only to the extent that it conforms to the norms of American-style individualist liberalism, with deviations regarded as anti-constitutionalist malfeasance?

The State of Israel does not, of course, have a formalized constitutional text; it has an incomplete set of "Basic Laws" on different subjects passed by the Knesset, and no list of constitutionally protected individual rights. In the absence of a definitive document, the 1948 Declaration of Independence has loomed large. Along with the 1950 Law of Return, the Declaration continues to define the Israeli policy, and under its terms the Israeli Supreme Court has developed a practice of judicial review that includes substantial protection for individual civil liberties. Score one for Israel, at least by American criteria.

Yet Jacobsohn does not gloss over the lack of a text. Creating a unified text is a daunting task, he explains, in the face of the tensions which comprise the Israeli polity. For Israel is constituted by its dual commitments—announced in the Declaration—to being a Jewish state and to ensuring equal rights and freedoms for "all its

inhabitants." Jacobsohn calls this "a mix of universal and particularistic principles" (p. 237). In several rich chapters, he examines the political and jurisprudential consequences of this mixture. He examines the recent controversy over amendments to the Law of Return (Who is a Jew?). He shows how the Israeli Court's dominance has been curtailed by the mix of constitutional principles just as much as by the tradition of parliamentary supremacy. He reviews education policy, national security censorship, and free-speech jurisprudence, most intriguingly the Kahane case and the ban on the showing of the film *The Last Temptation of Christ* (banned briefly out of respect for the sensibilities of Israel's minority Christian community).

Jacobsohn shows how ambiguity characterizes Israeli jurisprudence in all of these areas. He is far from portraying Israeli law as consistently communitarian or illiberal; by his own account the outcomes often match those of American courts. But with careful attention to nuance, he demonstrates how, even in those cases which seem to track most closely with American jurisprudence, there remains among Israeli judges a principled reticence to embrace the liberal view wholesale, and a consistent recognition that Israel's Jewish identity, and its fragile cultural context, make it unwise—even unconstitutional—to replicate the jurisprudence of individual autonomy rights.

In his final chapter, Jacobsohn broods on the theoretical implications of his comparison. He struggles to articulate the meaning of constitutionalism independent of liberal criteria. As illuminated by the Israeli experience, constitutionalism "is definitely not *illiberal* politics," he says, "but its conceptualization should not be confined only to polities that embody an unambiguous, robust commitment to the ends of liberal democracy" (p. 237). I think his own remarkable argument suggests that we go further still. He makes me wonder whether a mixed constitution of the Israeli variety isn't in fact more authentically constitutionalist than *any* of the unmixed varieties which (like "liberal constitutionalism") do not obstruct the despotism of their own principles.

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GRAHAM WALKER

Decision Making in the European Community: The Council Presidency and European Integration. By Emil J. Kirchner. New York: St. Martin's, 1992. 165p. \$24.95 paper.

The entry into force of the Maastricht Treaty on European Union on 1 November, 1993 came after much delay, including a razor-thin referendum vote in France and two such votes (the second was to deal with an initial rejection) in Denmark. The treaty has introduced a new level of complexity (some would prefer to characterize it as a new level of confusion) into the nomenclature, decision-making process, institutional framework, and relations between Brussels and the national governments of the 12 European Community member states.

The European Union is now the umbrella term referring to the already-existing "three-pillar" structure of the European Community (the European Coal and Steel Community, European Economic Community, and European Atomic Energy Community) and the two new "pillars" constructed by the Maastricht Treaty: the Com-

mon Foreign and Security Policy enhancing the new European Political Cooperation process and certain areas within the domains of the justice and interior ministries (especially greater collaboration/cooperation between police and other authorities on crime, terrorism, and asylum-immigration issues). The European Community still continues to exist as a separate legal entity within the broader framework of the Union; but, in view of the difficulties of delineating what is strictly EC or Union business, the term of choice is now the European Union, rather than the more limited Community, as contained in the title of this book.

Compounding this complexity, there exist two Councils: (1) the Council of Ministers of the European Union (formerly "of the European Communities") comprising (usually but not exclusively) the foreign affairs ministers of the 12 member states and (2) the European Council, comprising the heads of state or government of the member states. The "ministerial" Council meets quite regularly (about 80–90 times a year), whereas the "prime-ministerial" Council meets at a biannual summit. Each Council has its "Presidency"—a rotating (sixmonth term) chief presidency officer or "President-in-Office."

At the level of the European Council, the head of state or government of the member state holding this rotating six-month office acts as host, chairs the meeting, sets the agenda, acts as official spokesperson, and (depending upon a series of conditions) may be able to set the tone and nature of Union activities for the six months and initiate various public policy proposals. At the level of the Council of Ministers, the foreign minister (or another portfolio, depending upon the specific agenda) of the member state holding the rotating presidency plays a leading role.

Emil J. Kirchner, Director of the Center for European Studies at the University of Essex and a long-time observer of European Community/Union politics, has written a timely and important evaluation of the decision-making process at the level of these Council Presidencies and the offices' role in, and impact on, the European integration process, the management of international cooperation, and institutional community building.

The author, through a judicious use of archival material and personal interviewing, along with an analysis of decision-making theory and the various theoretical approaches to political integration, examines how the Council Presidencies impinge on the integration process, especially since the Single European Act and the two 1991 Intergovernmental Conferences (on the European Monetary Union and European Political Union). Assessing the balance and distribution of power between the actors at the Union and national levels, the author consistently refers back to the basic question, Is the Council Presidency primarily a device to maintain and strengthen "national control" over decision making within the European Union, or do the offices go beyond national prerogatives and enhance integration?

The negotiations that led to the Single European Act and the two Intergovernmental Conferences are interpreted by the author as requiring a new approach to, or understanding of, the relationship between the Union-level institutions and the member states. The author concludes that European Union decision making is not solely a transfer of powers from the national capitals to Brussels but also a sharing of competencies between

national and Union institutions and a "pooling of sovereignties" among the member states that evokes, at least for me, the American phenomenon of "cooperative federalism."

Kirchner has succeeded in presenting a most readable assessment of the dynamics of European integration, but this success has, in a sense, limited the book's overall attractiveness. *Decision Making in the European Community* is highly useful and informative to the professional well versed in the intricacies of the European Union and to the advanced graduate student, but it is not recommended for the general student, the public, or as a text.

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LEON HURWITZ

Party Policy and Government Coalitions. Edited by M. J. Laver and Ian Budge. New York: St. Martin's Press, 1992. 448p. \$59.95.

Legislatures in the Policy Process: The Dilemmas of Economic Policy. Edited by David Olson and Michael Mezey. New York: Cambridge University Press, 1991. 221p. \$39.50.

The two volumes reviewed here report findings and conclusions from ambitious research projects investigating aspects of policy making in a cross-national setting. In both, the research emanates from on-going collaboration by international teams of scholars, the participants individually expert in the politics of particular countries and guided, collectively, by cross-national research strategies. The editors lay out first the theoretical and methodological approach of the research, followed by applications in various national settings. They conclude with a chapter summarizing results across cases. The projects are differentiated both by institutional focus and the role assigned to policy in the design of the research. The Laver and Budge collection seeks to determine whether "policy matters" in the decisions made by parties to participate in the formation of governing coalitions. In contrast, the Olson and Mezey anthology explores ways in which legislatures may independently influence the making of economic policy in institutional contexts where political executives have become preeminent. The commitment to cross-national comparison suggests that the goal of both projects is to establish theoretical propositions by empirical generalization.

The projects depart from quite different theoretical and methodological traditions. The Laver and Budge work is rooted in three decades of formal and empirical work investigating coalition processes and behavior. While a certain mathematical elegance attaches to the coalition theories that have been developed, attempts to verify propositions empirically have generally produced disappointing results. Laver and Budge argue that this weakness stems from a general failure to take sufficient account of contextual constraints in adapting theoretical concepts for empirical testing. Thus they develop two expectations regarding coalition behavior in cabinetforming contexts: that the motivation of actors (parties, generally) is "policy seeking" rather than "office seeking," and the governments that form will be "policy viable" (not constrained by numerical criteria). Drawing upon cross-national longitudinal data coded from party manifestoes and coalition policy declarations, a number of hypotheses regarding these expectations are tested.

Lacking the tradition of formal theoretical work that supports the coalition policy research, the Olson and Mezey project relies upon the largely descriptive literature on comparative legislative behavior to generate a series of hypotheses designed to investigate the ways in which legislatures matter in the making of domestic economic policy. With policy activity (sometimes, policy participation) defined as the dependent variable, 16 hypotheses are stated. The hypotheses are grouped into three general categories to focus on particular contextual variables believed to condition legislative policy activity. These include extra-parliamentary institutions and actors, the internal structure of legislatures, and particular attributes of economic policies. The authors of individual chapters discuss a variety of specific policies and policy processes within the general framework established by this set of hypotheses.

With respect to the establishment of cross-national empirical generalizations in their areas of investigation, both works are, overall, disappointing. This is especially the case with the Olson and Mezey study. Their research design is explicitly and self-consciously structured as a linear regression problem where the objective is the cross-national explanation of variance in legislative policy activity/participation. The hypotheses they advance state a series of bivariate relationships suggesting that their testing will indicate an appropriate multivariate equation explaining legislative policy activity. However, no set of cross-systemic (equivalent) indicators is developed to measure the variables stated in the hypotheses that guide research nor is there a multivariate equation that emerges from the within-country assessments of the bivariate relationships. As such, the descriptions and analyses presented in the individual chapters of this work are not generalizable across cases.

The Laver and Budge effort, in general, avoids this problem. The research design calls for the creation of policy spaces from data sets that use common categories for coding party election manifestoes and government policy declarations, over time, in each of the countries considered. These data are reduced by factor analytic techniques to produce for each of the countries studied a series of policy maps locating the major parties in both one-dimensional and twenty-dimensional policy space. The policy "distance" separating the parties then provides the basis for testing a number of hypotheses relating to coalition formation and policy payoffs. While analysis is confined to individual systems (countries), the design of the research allows results to be directly comparable across countries and across time.

The problem with the Laver and Budge project lies in interpreting the results that are produced; they are either equivocal or disconfirmatory of initial expectations. Indeed, policy-based predictions of coalition formations are poor overall and the policy preferences of governing parties are not reliably reproduced in government policy declarations nor does membership in a cabinet guarantee that government policy will reflect party preferences better than being in opposition. More to the point, Laver and Budge find no pronounced cross-national patterns from the country analyses regarding the specific expectations stated in the research design. Thus, it is not that the results are not generalizable, it is rather that they do not generalize. Since it is scarcely conceivable that policy "doesn't matter" to parliamentary actors when they bargain over membership in government and making policy, the lack of

consistent results is discouraging. One suspects, as do the editors themselves, that the main difficulty lies in the analytic and methodological choices that were imposed in executing the research design.

The two projects reviewed here point to a central dilemma of comparison, namely, eliminating sources of residual variation in hypothesized relationships. In a classical formulation more than two decades ago, Przeworski and Teune noted the threat to scientific generalization that is inherent in reliance upon idiosyncratic "country factors" to explain cross-national political phenomena, and they exhorted us, where possible, to substitute variables for country names. Thus, to say that an hypothesized relationship holds except in countries X and Y, where some other result is observed, does not invite us to explore the nature of deviance in such cases so much as to search for common (universal) factors that account for cross-national variation. This methodological commitment defines the role of "country experts" in cross-national research projects: namely, to aid in establishing the equivalence of indicators that are selected to measure theoretical concepts. The object is to moot cultural and institutional particularities by controlling their effect, allowing empirical generalizations to be established.

Without a guiding theory of legislative policy influence, the Olson and Mezey project relies upon its contributors to give country-specific interpretations of the policy process. While the research is loosely organized around a series of hypothesized relationships, no elaboration of basic concepts to establish common categories for analysis seems to have been undertaken, nor is there a consistent measurement strategy applied to the various country analyses. In short, no attempt is made to establish cross-national equivalence for either basic concepts or their indicators. The theoretical and methodological weaknesses of this project reduce the role of the country experts to that of providing their own descriptions of national practices, and herein lies the value of the work. To a greater or lesser degree, the various chapters of this volume provide contextual detail and a qualitative understanding of the institutional role of legislatures in the policy process, helping to build a descriptive base for the eventual development of empirical theory.

For Laver and Budge, the guidance provided by traditional coalition theory is at the outset judged deficient for neglecting the influence of contextual constraints operating in national settings. Thus for example, the central theoretical concept of "winning coalition" is replaced in this research by the contextually determined notion of a "policy viable" coalition. This reformulation, produced jointly by the editors and the team of country experts, seeks to nullify the effect of idiosyncratic system factors by creating a theoretical concept that is equivalent cross-nationally, as Przeworski and Teune would have advised. In spite of such efforts, however, the importance of the Laver and Budge project is vitiated by the failure of cross-national patterns to emerge from empirical analysis. The consequence is that the explanations of policy relevance for behavior they offer is significantly dependent upon the judgment of their country experts regarding the influence of system specific factors

The projects considered here begin, respectively, with the belief that "policy matters" or "legislatures matter." It would, however, appear more to the point to say that "country matters." The value of both works, then, is found in gaining an appreciation of the ways in which similar phenomena are manifested differently across countries. Both works document the importance of contextual constraints on political processes and behavior, and (in the Laver and Budge project particularly) point to the difficulties they pose for the establishment of cross-national empirical generalizations. If, as was observed earlier in this review, we can evaluate the results from these projects to have been disappointing, it is as pertinent to observe that cross-national research is not simple. These and other projects that involve international scholarly cooperation continue to be urgently needed in the effort to generalize knowledge of important political processes and behavior.

University of Wisconsin, Milwaukee Eric C. Browne

Germany Divided: From the Wall to Reunification. By A. James McAdams. Princeton: Princeton University Press, 1993. 250p. \$29.95.

German Unification in the European Context. By Peter H. Merkl. University Park: Pennsylvania State University Press, 1993. 448p. \$65.00 cloth, \$18.95 paper.

Perfect hindsight has led many scholars and practitioners alike to conclude that Germany's division was always destined to be temporary. Politicians in Bonn wrangle over the proper share of credit for having pursued the strategy that made reunification happen. A. James McAdams reminds them all just how stable the country's division into two separate states appeared for several decades. He gently chides many for conveniently forgetting that much scholarship, as well as an entire generation of diplomatic activity, rested on the premise that that division would last. His book's central aim is to explain how partition came to be accepted even (or especially) by Germans themselves.

McAdams suggests that domestic political processes in Bonn and East Berlin, however different in character, both contributed significantly to this gradual process of adaptation to the status quo. McAdams defines process broadly to include the interaction of a variety of domestic political factors, including the interests and goals of top government and party leaders.

Germany Divided advances decade by decade through to the 1990s, though the era that McAdams treats in greatest depth is the 1980s. In each section, he pays detailed attention to the pronouncements and actions of political elites in both German capitals. The author shows how Bonn and East Berlin each gradually abandoned the belief that national division could be ended on terms most satisfactory to its own vital interests.

McAdams's work rests on a wealth of research and is thoroughly documented. One of the book's most important sources is a set of interviews that the author conducted with top figures from the former East German establishment as part of an oral history project on that regime deposited at Stanford University's Hoover Institution.

Germany Divided makes another unique contribution to scholarship on the preunification period by demonstrating quite convincingly just how much of what led to that event was inadvertent, even a result of miscalculation. The East's often overconfident and panicky yet always myopic communist elite was busily digging its own

grave by loosening, tightening, and again loosening emigration or travel restrictions. And West German political parties, understandably anxious to preserve a sense of stability and normalcy (which was also popular with voters), placed a premium on the "doable" things: while describing the division itself as deleterious, they put ideas of unification on the back burner in favor of practical cooperation with the regime in East Berlin. Like a budget deficit, national division had many sincere critics, but almost all found a way of living with it.

Many scholars of former East Germany have long held to the view that the Communist regime generated some legitimacy, making it less than totally dependent on Soviet military support. Their critics have contended that this premise blurred the fundamental distinctions between a police state and a liberal democracy, thus obscuring the major reason for the East's total collapse. Germany Divided does devote equal attention to domestic political processes in the two states but also shows how those features which set them apart also affected relations. Nor does its thesis necessarily view domestic politics as the sole determinant of each country's policy toward the other in the years before 1989. Whatever view one takes of the highest-charged scholarly debate to emerge from unification, McAdams's careful, thorough analysis contributes in a significant way to a better understanding of the dynamics that moved German-German relations for decades.

Where McAdams focuses on the impact of one set of factors in the shaping of policy during past decades, Peter Merkl's study, German Unification in the European Context, offers a broad examination. While that might make for a somewhat less compact thesis, the book more than compensates with a breadth of scope encompassing past and future—as well as domestic and international—dimensions of the German Question. Moreover, Merkl frequently returns to a basic, underlying theme, namely, his view that modern Germany's stake in a prosperous, stable environment should reassure those given to concern about unification and its impact on Europe. He repeatedly stresses, for example, that any signs of nationalistic euphoria during the unity drama were restricted almost exclusively to the older generation, while even "flag-waving patriotism" of the American variety has remained rare among younger Germans.

Unlike Elizabeth Pond's Beyond the Wall (1993), Merkl's book is not an analytical narrative account of events in 1989-90 but a detailed interpretation of unification. It is structured thematically, though the treatment of each separate topic moves in a generally chronological fashion (at the cost of some unavoidable repetition). In this manner he covers the general erosion of the Soviet bloc, the rise and decline of communist East Germany, the West German response to the historic opportunity of 1989-90, the constitutional order of united Germany, the economic problems associated with unification, the reaction of other countries to this sweeping change in the structure of Europe, and finally the role this "new" country will play in the continent's (d)evolving security regime. Toward the end, the author credits Chancellor Helmut Kohl "for steering the country through the shoals of reunification" (p. 398), contending that he had no choice but to proceed hastily lest international and domestic resistance (including the reluctance of taxpayers to foot the bill) mounted.

In over four hundred pages, few issues are left untouched; the author provides a wealth of documenta-

tion, including, above all, survey data on almost every question treated. At the same time, he does not hesitate to enrich his analysis with personal reflections and an occasional poetic turn of phrase, such as the suggestion that a few guard towers on the old German–German border be preserved "as a monument to the living and a reminder that one person's myth can be another person's dungeon" (p. 421).

To be sure, perhaps not all readers will be fully reassured about the future of German politics especially in light of a recent upsurge in right-wing radicalism. Others might remain unpersuaded by the slightly overdrawn contrast between Anglo-American jingoism and idealistic German pacifism. Yet this book makes a major contribution to understanding the multidimensional character of the German unification process and its various, lasting implications. In particular, the author makes a convincing case that (appearances to the contrary) unity did not merely represent an extension of West German institutions and practices to the East: in almost all respects, much remains that divides these two halves of the country, including (as Merkl again documents with survey data) a mutual antagonism in popular attitudes across the old border. The author leaves a clear impression that ending Germany's partition was the easy part; overcoming the effects of four decades spent in entirely different political and social systems is a task that will take much more time to complete, with complete success by no means assured.

Aside from the general topic of Germany unity and quality of scholarship, these two books share certain other similarities. To be sure, neither author is primarily concerned with the German issue's relevance for broader theoretical issues in comparative politics or international relations. Yet both emphasize the degree to which politicians (above all, in West Germany) internalized the very concerns about their country's impact on European stability that long made neighboring states worry about unification. That phenomenon may ultimately be the one legacy of Germany division that ensures the success of German unification.

College of William and Mary

CLAY CLEMENS

The Sacred Cause: Civil-Military Conflict over Soviet National Security, 1917–1992. By Thomas M. Nichols. Ithaca: Cornell University Press, 1993. 259p. \$32.50.

Like many other features of the Soviet political order, Soviet civil-military relations have recently attracted new scholarly scrutiny. Even during the comparatively stable Khrushchev and Brezhnev eras, Western analysts' disagreements about the nature of Soviet civil-military relations had important practical implications. But the participation of senior Soviet officers in the abortive August 1991 coup and the role of military units in the October 1993 showdown between President Yeltsin and the Russian parliament have given these long-standing debates greater currency.

In The Sacred Cause Nichols makes a significant contribution to a better understanding of recent trends in Soviet and Russian civil-military relations. Although the book covers the entire span of Soviet history, most of it deals with the period from Khrushchev through Gorbachev. Drawing on a wide range of Russian-language sources, Nichols concentrates on the development of

military strategy, weaving into the account a substantial amount of information about other topics. He rightly pays close attention to the civil-military contest over the proper relationship between "military science," a domain in which the military was acknowledged to have special expertise, and "military doctrine," in which the Party traditionally claimed the final say.

The book's main thesis is that civilian Party officials and Soviet military officers were divided by conflicts inherent in the Communist system. Due to the absence of any meaningful Soviet constitutional tradition and to the Party's formal advocacy of transcendent political principles, civilian leaders could never establish unconditional control over the military. In the terminology of Samuel Huntington's The Soldier and the State (1957), Party leaders resorted to "subjective" rather than "obiective" means of control. That is, they relied on such devices as police surveillance, political criteria of promotion, ideological indoctrination, and occasionally promulgation of military policy by Party fiat, rather than on an political-military division of labor that granted officers full professional autonomy in exchange for a military commitment not to intervene in politics. This approach, however, had unintended consequences. In the twilight of Soviet power, an officer corps steeped in Marxism-Leninism was pitted against an increasingly pragmatic Party elite. The Army became "the watchdog of the Party rather than vice versa," producing a lasting confusion of military loyalties that "may be among the greatest threats to the emerging post-Soviet democracies" (pp. ix-x).

This interpretation is novel and thought-provoking. It is a useful attempt to evaluate Soviet civil-military relations by comparative standards of constitutional loyalty not generally applied to the USSR by Western analysts. Prof. Nichols underscores the mingling of Soviet Party and military officials in decisionmaking bodies such as the Party Central Committee-and, one might add, in the Party Politburo after 1973. The author could have further buttressed his argument for the Party's reliance on subjective mechanisms of control by giving fuller attention to the use of the secret police to prevent potential challenges from the military. The blurring of the institutional boundaries between the civilian and military realms has become especially pronounced in the post-Soviet period. Due partly to a faulty understanding of the concept of the separation of powers (at least one-sixth of the deputies in the lower house of the new Russian parliament are employees from executive government agencies) military officers have been elected to legislative bodies and have plunged into bitter debates about Russia's socioeconomic crisis.

Although Nichols is right to note the Soviet precedent for this post-Soviet blurring of civil-military boundaries, the depth of the military's actual involvement in civilian matters before the late Gorbachev years remains debatable. In my opinion, the author's general depiction of civil-military relations does not take adequate account of the substantial variations that occurred during the Soviet era, even though his historical exposition notes these variations. For example, he states that the military had a predominant role in the formulation of military doctrine in the 10 years after 1925 and in the 18 years of Brezhnev's rule, which together amount to more than one-third of the history of the USSR. In addition, the book's empirical focus on military policy—the domain in which uniformed officers had the most legitimate

grounds to question the judgment of party leaders—makes it hard to assess the degree to which military men actually staked out positions on civilian matters. The most senior commanders undoubtedly had opportunities to "log-roll" on civilian issues during Central Committee and Politburo meetings, and in the early Stalin era a few senior commanders registered objections over the disruptive manner in which agricultural collectivization was implemented. For most of the Soviet period, however, the public pronouncements of senior officers focused almost exclusively on military issues and gave few indications of efforts to influence decisions on non-military matters.

Although this pattern changed in the late Gorbachev years, it is questionable whether national security policy, undeniably the source of sharp disagreements, became the most important bone of contention between Party leaders and military men. To judge by the timing, preserving the Soviet state against the risk of internal collapse was the issue that propelled many senior officers into outright defiance of Gorbachev in 1991. Differences over external threats probably played a secondary role in precipitating military involvement in the attempted coup. In addition, the concept of an ideologically indoctrinated military establishment does not capture the splits which had riven the officer corps by 1991 and which Nichols himself notes in passing. If the 1990 voting behavior of various military delegates in the Russian legislature is any guide, many senior commanders remained loyal to Marxism-Leninism, but many middle-rank and especially junior officers were hostile to Marxism-Leninism (as were a surprising number of low-level Party officials). Adherence to Marxism-Leninism among senior commanders helped trigger the 1991 coup attempt, but disbelief in Marxism-Leninism among other members of the officer corps helped defeat it.

Whether the reader accepts all the propositions contained in the Sacred Cause, the book makes a substantial contribution to reassessing the dynamics of the Soviet system and the implication of those dynamics for current upheaval in Russia. Scholars interested in such issues will read it with profit.

Johns Hopkins University

Bruce Parrott

Citizens, Political Communication, and Interest Groups: Environmental Organizations in Canada and the United States. By John C. Pierce, Mary Ann E. Steger, Brent S. Steel, and Nicholas P. Lovrich. Westport: Praeger, 1992. 256p. \$47.95.

This work is self-consciously directed at addressing in an empirical way an important normative issue in democratic theory: How are citizens to face the mounting informational challenges posed by policy issues in "postindustrial" society? Specifically, they ask, Do interest groups help citizens meet the challenges of scientific and technical complexity effectively? The authors claim that they do. Unfortunately, their data fall very far short of establishing that claim. The authors show rather convincingly that environmental interest groups in Canada and the United States are important information sources for both policy elites and interest group members. But they have failed to build into their study any measure of effective communication and so cannot equate desire for information or information per se with adequate

information or a reduction of complexity. The reason for this gap is that the authors' democratic instincts are too deeply rooted in an economistic model of citizenship. Hence, more (information) is better: the citizen-as-information-consumer demands information; interest groups supply it; ergo environmental groups are good.

supply it; ergo environmental groups are good.

The study's fundamental thesis is that "environmental interest groups may act as a unique mechanism for assisting democratic publics in dealing with their need for help in evaluating and employing scientific and technical policy-relevant information" (p. 106). A secondary thesis—which unfortunately occasionally threatens to overwhelm this focus—is that political culture influences the functioning of political institutions (in this case, interest groups). The latter thesis is born out by the analysis in a rather straightforward way; not so the

work's primary thesis.

The authors' central thesis, rephrased toward the end of the work, is that "interest groups in both the United States and Canada have the capacity to assist citizens in coping with the problem of the technical information quandary" (p. 186). This thesis is supported by three separate findings: (1) at the macro level, the authors discover some convergence in political culture, which they believe suggests "a common potential for having interest groups play an important political communication role in linking contemporary citizens to the public policy process"; (2) at the micro level, individuals differentiated between different groups as sources of political information and identified information as the primary motive for membership in environmental interest groups; (3) at the level of group processes, environmental interest groups were found to "rely extensively in carrying out their activities on the communication of policy-relevant information to their members and lay citizens" (pp. 176-77, 186). The cross-national survey data, supplemented by elite interviews with environmental activists and policymakers, appear to support these three propositions. The problem is that these propositions do not themselves add up to the book's basic thesis.

If the authors wish to equate the citizen's need for information with "a preference for information"-as they often appear to do (pp. 93, 97, 118, 150)—then one cannot speak of interest groups' doing anything more than gratifying an information itch. Talk of "effectiveness," "dealing with complexity" or "coping with complexity" all becomes synonomous with the instrumental delivery of utility-as-information and interest groups are simply (information) satisfying or not, nothing more. Fortunately, the authors do not really believe in this notion of a free market of ideas. In several places, they suggest that there is more to political communication than informational hedonism. For example, toward the end of the book, they note that interest groups may assist individuals learn by "cue-giving, resource-pooling, and influence-exerting activities" (p. 186). The problem is that these activities are not scrutinized here, much less tied to any explicit theory of learning. Nor, it might be added, are these tied to any explicitly democratic theory, either. (Does effective communication require any communication from the citizen to the interest group? What kind?) One finishes this book impressed by the empirical research and buoyed by the findings but wondering just what they mean. "Interest groups can get the information job done," the authors argue. But just what is information? And what is getting the job

Although the authors' theoretical grasp exceeds their empirical reach and although their argument is weakened in several places by a lack of conceptual clarity, there is much to commend in this volume: if it attempts to say more than it should, what it does say is important and stimulating. It matters a great deal that citizens' relationship to interest groups appears to be motivated, above all else, by a desire to seek information. If the authors stray beyond the data, they also take us into interesting and even provocative terrain. And if they fail to settle the important issue they take up, they at least take us some of the way there and point to the next step. Interest groups process a lot of information: But how well do they learn? How well do they teach?

Texas A & M University

ADOLF G. GUNDERSEN

Red Sunset: The Failure of Soviet Politics. By Philip G. Roeder. Princeton: Princeton University Press, 1993. 317p. \$49.50 cloth, \$16.95 paper.

Accounts of the political system of the USSR have alternated between emphasizing its routinized, bureaucratic character and considering it anarchic, potentially unstable, and prone to personal dictatorship. In the late 1970s, Western analysts became increasingly aware of the substantial decline in the regime's performance, because the enfeebled party leadership was increasingly hostage to the great bureaucracies that the regime had created.

In Red Sunset, Philip Roeder renews this line of argument. He finds that the regime grew stagnant because of the strength of the political ties connecting the top party leadership with the powerful bureaucratic interests represented in the Central Committee of the Communist party. He argues that past accounts have paid too little attention to the party General Secretary's dependence upon a base of support in the party machine, the military, the state bureaucracy, and other "second-tier" interests. He advances a concept of "reciprocal accountability" that, he believes, was implicit in the original requirement of the Bolshevik Revolution to delegate power to perform specific functions to bureaucratic and regional power centers. The Stalin period represents an aberration from this "constitution," according to Roeder; but immediately after the dictator's death, Stalin's successors restored the older collective leadership model. Ultimately, however, that very model became an obstacle to deep reform, because the rulers could not introduce significant redistributive change without jeopardizing their own political coalition. As a result, the system lost its capacity to maintain itself. Gorbachev's radical reform efforts simply proved the intrinsic incapacity of the system to preserve itself through democratization: a successful balancer at the outset, Gorbachev finally provoked his allies both on the Right and the Left to abandon him in 1990-91.

Roeder's account has a number of strengths. It provides a theoretically reasoned interpretation of the development of the Soviet political system, which accommodates both the apparent stability of the Brezhnev period and the rapid dissolution of the union in 1991. Moreover, he offers three case studies to demonstrate how Soviet coalition politics affected, and was affected by, policy decisions. The book also has the ambitious objective of proving the utility of a neoinstitutionalist perspective for the study of authoritarian regimes,

where their application has been inhibited by the fuzzy, tacit, and arbitrary character of rules and procedures governing political life. In a sense, the book stands the usual problem of social choice models on its head: instead of asking how majority rule can yield stable policy outcomes, Roeder asks how rules under authoritarianism can yield stable political coalitions. In turn, this leads him to reach the conclusion that the very stability of an authoritarian regime, based on a compact among oligarchs, may be untenable if the regime itself cannot cope with serious performance problems.

Roeder wants his model to turn our attention back to purely political processes and away from the more recent preoccupation with state-society relations. In this sense, he returns us to the classical concerns of Soviet political studies: how power and authority are built and maintained, how political coalitions are linked to policy lines, what different patterns of representation on leading party bodies tell us about the balance of power within the party state. On these matters, Roeder builds on, and formalizes, many of the findings of earlier Sovietological literature. But these gains come at a price. Left out of sight are social changes that altered the balance of power between regime and social groups, between center and regions, and certainly between the predominantly Russian political elite and the increasingly self-aware political and social elites in national republics and other ethnic-national territories. The problem of corruption, with its implications for the nationality dimension of politics, is not discussed. Perhaps even more central is the question of property rights. Could any communist party state have permanently reversed the decline of economic performance without establishing a secure property rights regime?

Roeder may, indeed, exaggerate the degree to which the regime operated under accepted rules of the game over the seven decades of its existence. Different leadership phases developed distinctive sets of understandings about legitimate means of seeking power, making policy, removing opponents, rewarding allies. These rules and informal constraints differed less as between the Khrushchev and Brezhnev periods than as between those two periods together and Stalin's reign. The "constitution" of the Stalin period, in turn, only gradually emerged out of the struggles of the NEP and war communism periods. Some scholars have gone so far as to say that there was, in fact, no single "Soviet system" because of the deep discontinuities between the operational codes of various leadership periods. This is surely overstated. But it is likewise misleading to speak of a fundamental "constitution of Bolshevism" unless it is also understood to mean the original Bolshevik project of building socialism in Russia.

Emory University

THOMAS F. REMINGTON

The Birth of Judicial Politics in France: The Constitutional Council in Comparative Perspective. By Alec Stone. New York: Oxford University Press, 1992. 312p. \$45.00.

Judicial Politics in France is the first full-length study in English of France's Constitutional Council. An exhaustive account of a neglected institution, it adds an important chapter to our understanding of French politics. In a careful study of its activities from 1958 to the present,

and relying almost entirely on official French sources, Alec Stone traces the Council's gradual transformation from a relatively inactive quasi-judicial agency into a major policy-making institution of the Fifth Republic. Designed originally to assist the president in checking parliament, the Council has evolved into a weapon that legislative minorities have used to check the president and parliament as a whole.

The critical year in this transformation was 1974, when any 60 legislators in the Senate or National Assembly won the right to petition the Council for a ruling on the constitutionality of bills passed by parliament but not yet promulgated as law. With this expanded right of petition (it had previously been limited to the president, prime minister, and presidents of the two parliamentary chambers) and the Council's increasing readiness to annul or alter bills referred to it, parliamentary parties opposed to the government have been able to wrest changes in nonpromulgated legislation from both government and parliament. Stone documents this process in detailed case histories of legislation dealing with the nationalization of industry (chapter 6) and media broadcast policy (chapter 7). From these studies and related data, he concludes that the council's activities have significantly altered French executive-legislative relations, as well as subverted France's historic commitment to the principle of parliamentary supremacy

In Stone's account, parliamentary minorities refer an exceptionally large number of adopted bills-one out of seven in the 1980s-to the Council for scrutiny on constitutional grounds. These minorities have virtually converted the Council into a third legislative chamber. In nullifying or laying down conditions for the final passage of valid legislation and thus delaying its promulgation as law, they often force legislators to renegotiate the terms of earlier agreements. Even threats to take bills to the council prompt parliamentary majorities to accept legislative changes they would otherwise reject. Stone invents fancy terms such as autolimitation and corrective revision to describe parliament's reaction to the council's review of legislation, a response that transposes debates on policy into constitutional arguments and legalistic quibbling. The consequence of this process is the "judicialization" of the legislative politics and the 'politicalization" of constitutional review.

As its subtitle suggests, Judicial Politics in France introduces a transnational perspective. In part 3, "Toward a Comparative Understanding of Judicial Review," Stone compares the Council with the German, Austrian, and Spanish constitutional courts. Part 3, however, is little more than an addendum to an otherwise exemplary study of a single institution in one country. Its burden is to show that constitutional courts empowered to nullify legislation on abstract judicial review (i.e., review of legislation outside the framework of ordinary litigation) are engaged in an activity more legislative than judicial. Other constitutional courts, he finds, are engaged in a genre of political decision making no different from the activities of the council. In the end, Stone views judicial review as an undertaking rooted entirely in politics, and that view, he thinks, should drive the comparative study of judicial review.

Although provocative and interesting, part 3 will be seen as less than convincing by some constitutional scholars. Stone recognizes that constitutional review in France differs from its practice in other countries—i.e., that constitutional review of unpromulgated legislation

is different from the judicial review of laws already in force—but he nonetheless presses the work of the latter into the mould of the former. A glance at the notes may explain the reductionism. When Stone writes about the Council, he exhibits a surefootedness grounded in primary sources; but when he shifts attention to other countries, he betrays a reliance on secondary materials of limited value. What else would explain his acceptance of the view that German constitutional decision making is driven as much by partisan politics as the work of the Constitutional Council? What else would lead him to the conclusion that abstract judicial review is all politics and no law?

In fairness, however, Stone's comparative report should be seen as less a definitive account of the politics of judicial review than an ambitious effort to define a framework for the transnational study of judicial review. Stone is engaged in a worthy attempt to join constitutional law to the study of comparative politics. Constitutional scholars should applaud this effort and the strategy generally of deploying public law for the purpose of providing a more complete and nuanced account of policy-making in constitutional democracies. By the same token, as he continues his work on constitutional courts outside of France, Stone may begin to qualify his account of these tribunals and perhaps even concede that constitutional interpretation, at least in some legal settings, has its own integrity above and beyond politics.

As a comparative politics scholar, Stone is a rara avis. He is one of the first in the subdiscipline of comparative politics to take courts seriously, and he appropriately credits Martin Shapiro's work on "political jurisprudence" for inspiring him to turn his attention to judicial institutions. This is a happy development. Up to now, the comparative study of law and courts has been the preserve of public law scholars within political science, just as they and other legal scholars have dominated the study of foreign constitutional courts. The heavy doctrinal and normative emphasis of these legal scholars may be one reason comparative politics has failed to give their work the attention it deserves.

Stone, by contrast, views constitutional courts and their decisions as normal parts of the political process that any inclusive account of politics must consider. He shows that these courts are important in defining the rules of politics and that an account of their work and influence are essential to a fuller understanding of the political systems in which they operate. On the other hand, these courts also define the goals and aspirations of the constitutional polities in which they operate, a function that requires some distance from ordinary politics and some measure of devotion to law's integrity and the principle of judicial independence. In this respect, finally, Stone's account of the Constitutional Council's political jurisprudence might be contrasted to John Bell's recent study of its normative jurisprudence (French Constitutional Law, 1992).

Judicial Politics in France prompts a concluding comment on style. Stone has a tendency to italicize every important point in the book. Almost every page features italicized words or remarks, a practice that draws attention to itself and annoys the reader. Ordinary English words should not be italicized. The author, if he has any mercy at all, would allow his readers to do the underscoring.

University of Notre Dame

DONALD P. KOMMERS

INTERNATIONAL RELATIONS

Japan in the Posthegemonic World. Edited by Tsuneo Akaha and Frank Langdon. Boulder: Lynn Rienner, 1993. 295p. \$40.00 cloth, \$17.95 paper.

The Challenge of Japan: Before World War II and After. By Nazli Choucri, Robert C. North and Susumu Yamakage, New York: Routledge, 1992. 383p. \$59.95.

The two books under review are most welcome additions to the increasing literature in English on Japan's foreign policy, of which Gerard Curtis's collection, *Japan's Foreign Policy after the Cold War* (1992) is a notable example. They give readers fresh readings of Japan's foreign policy at a time when the world is changing very steadily.

Despite their differences, they share a common preoccupation with hegemony and a common underlying question: Once the U.S. hegemonic era is gone, is Japan

likely to be the next hegemon?

Their approaches are quite different. The Challenge of Japan focuses on the pre-World War II period and tries to see the post-World War II period with the same internally coherent conceptual framework. Japan in the Posthegemonic World focuses on the contrast between the pre-Cold War and post-Cold War periods, with the contributors focusing more or less on bilateral relations of Japan in specific policy areas. The Challenge of Japan argues that what the authors call lateral pressure (i.e., the demand for outward expansion derived from the configuration of population, resources, and technology) determines Japan's foreign policy, using simultaneous equation models based on macroeconomic time series data. In other words Japan's large expanding population, relatively poor resource endowments combined with a rise of demand for resources due to steady industrialization, and the advances in technological level are major determinants of lateral pressure for Japan. Whether it takes military or economic outward movement is determined by international environments. For the pre-1941 period, lateral pressure mounted, and Japan manifested its lateral pressure through military aggression. For the post-1945 period, the framework does not work very well. The authors admit this by saying, "In the long run, continued success in exporting manufactured goods was important in diverting Japan's lateral pressure away from the military form or mode (p. 294). Japan in the Posthegemonic World argues that "there is no hegemon to rescue us, no deus ex machina" and that "it is no longer a matter of either U.S. or Japanese hegemony; it is cohegemony (bigemony) or, a little more generally, a Pax consortis'." (pp. 88, 262).

The credibility of the approach and framework of *The*

The credibility of the approach and framework of *The Challenge of Japan* would have been further enhanced if the authors had confined the data to the pre-1945 period, while evoking the same conceptual framework both for the pre-1945 period and as they look to Japan's ascendance and its possible explosion in the future. In contrast to this, *Japan in the Posthegemonic World* does not well integrate later, empirical chapters with the first two or three chapters concerning the theory of global hegemony and the nature of Japan's power (the relatively sanguine pursuit of national interests combined with "a more constructive notion of leadership in a world of

complex interdependence" [p. 66]). Further efforts could remedy these shortcomings. For The Challenge of Japan one could develop the conceptual framework of lateral pressure on the basis of global market access in trade and investment, with population, resources, and technology components kept underneath in an era of protectionism and regionalism on the one hand and global environmentalism on the other. The conceptual framework could be devised more directly to the changed international environments. For Japan in the Posthegemonic World, we might justifiably hope for a singleauthored volume on the same theme. Although much has been written on Japan's foreign policy, still more remains to be done. Despite the increasing demand for books on the subject, the relative scarcity of specialists in Japan's foreign policy, especially in North America, leaves big lacunae unfilled for some time to come.

University of Tokyo

Takashi Inoguchi

The Presidency and the Rhetoric of Foreign Crisis. By Denise M. Bostdorff. Columbia: University of South Carolina Press, 1993. 316p. \$34.95.

As George Bush's meteoric public approval ratings in the wake of the Persian Gulf war aptly demonstrate, leadership in foreign crises from the Oval Office can provide clear domestic political benefits. But such approval can also be a short-lived aberration that flashes across the electoral scene like a Scud on a starless night. Denise Bostdorff correctly notes that presidents, although they do not create the world they face, play a large role in shaping public perceptions of that world by promoting, defining, and (sometimes) resolving foreign crises. Presidents influence world events and public perceptions not only with military and diplomatic initiatives but also with sometimes carefully chosen words and symbols. Their words and symbols then influence the policies they choose.

Bostdorff devotes her attention to the public rhetoric of presidents in foreign policy crises, arguing that we can gain insight into both the presidency and the men who have occupied it through close textual analysis of their speeches. The research design is both simple and ambitious: she picks one foreign crisis for each president from Kennedy to Reagan and looks closely at all relevant public statements by the president. She also looks at internal memos promoting particular policies and rhetorical statements and official statements by sanctioned spokespeople. In this regard, Bostdorff's work is a real advance over previous studies of presidential rhetoric focusing on "major" speeches, such as State of the Union addresses or other formal addresses. She recognizes that a few unguarded comments can do as much to influence public perceptions as the most carefully crafted formal address.

To her credit, Bostdorff does not present rhetoric as an independent creation, but attempts to situate each discussion in both the foreign and domestic politics of the time. She begins each chapter by setting the international scene and the domestic pressures that the presi-

dent faced. It is impossible not to admire the sheer volume of materials (archival documents, public papers, and a range of historical and analytical treatments) that she synthesized in order to assess the real importance of presidential rhetoric. At the same time, the foreign policy specialist or historian may find some of the necessarily brief treatments of context frustrating. For example, Bostdorff notes, in her chapter on Jimmy Carter and the Iranian hostage crisis, the speculation that candidate Ronald Reagan cut a deal with the Iranian government to ensure that Carter's efforts would not succeed while he was in office. Bostdorff provides no critical perspectives or evaluations of these charges.

The heart of Bostdorff's analysis is a formal dissection of presidential rhetoric, using a method developed by Kenneth Burke, a theorist of rhetoric. Putting language at the center of concern and finding motivation within the texts, Bostdorff attempts to discover how each president portrayed the "crisis" situation, the style the president employed in managing the crisis, and the bases of identification of allies and enemies he encouraged in the public. Through this method, she intends to illuminate differences in style and substance among the presidents and derive some insights about the nature of crisis promotion in contemporary presidential politics. (Bostdorff's use of Burke's method will be new to many political scientists, who may find it difficult to wade through the textual analyses.)

The differences, as expressed in close readings of each case, are subtly drawn and produce sometimes surprising insights. Bostdorff finds that President Ford, for example, departed from the standard practice of crisis promotion when the Mayaguez was seized off the coast of Cambodia. Whereas other presidents inflated such events, promoting crises to promote their own leadership, Ford himself was silent on the matter. His public affairs staff provided only brief descriptions of events as they developed. When the crisis ended, Ford described the events and his actions only briefly. Bostdorff suggests that Ford's silence was a deliberate rhetorical strategy to bolster his standing as a national leader. Unelected, Ford desperately sought to cultivate legitimacy as president by allowing his staff to portray him as competent, active, and busy responding to the problems of the day. Aware of his own difficulties with public speaking, Ford limited his public speeches and even his answers to questions at press conferences. In this way, he was able to maximize the political benefits attendant to the crisis-by saying less.

In stark contrast, Jimmy Carter initially spoke frequently about his handling of the Iranian hostage crisis, emphasizing publicly the principles by which the United States would manage the affair. At the same time, by constantly reiterating principles but rarely reporting progress, he undermined public perception of his efficacy as a leader. "The idealism of Carter's rhetoric," Bostdorff writes, "also constrained his crisis management by narrowing the range of policy options he could pursue to those that he deemed consistent with the

moral principles he set forth" (p. 159).

Of course, the analysis of Carter points to a critical question Bostdorff dodges: To what extent is rhetoric a reflection or a cause of presidential policy in a foreign crisis? Bostdorff considers explicit political planning in conjunction with the psychic vulnerabilities of American presidents, without placing anything above the texts of presidential speeches, both improvised and committeecrafted. The real role of rhetoric as reflection and as source of, or substitution for, policy remains to be explored.

John Kennedy, she argues, established formal mechanisms and legal justifications for crisis promotion and management, an infrastructure that remains. While there are recurrent patterns in presidential rhetoric, the infrastructure has grown and strengthened. Bostdorff concludes: "The relationship between power and discourse appears to be symbiotic. The president's control over foreign affairs allows him to indulge in crisis promotion; at the same time, crisis promotion frequently helps the commander-in-chief to expand and consolidate his power. This is a dangerous combination indeed" (p.234). Scholars of the presidency-and of foreign policy more generally—owe Denise Bostdorff a debt for framing this problem of modern democracy so sharply.

University of Michigan

DAVID S. MEYER

The State of the European Community, Volume 2: The Maastricht Debates and Beyond. Edited by Alan W. Cafruny and Glenda G. Rosenthal. Boulder: Lynne Rienner, 1993. 435p. \$49.95.

The 1992 Project and the Future of Integration in Europe. Edited by Dale L. Smith and James Lee Ray. Armonk: Sharpe, 1993. 260p. \$45.00 cloth, \$17.50 paper.

Part of the professional creed of political scientists is the belief that narrowly focused academic volumespreferably monographs—are the most worthy. Yet this has not consistently been true of the scholarly study of the European Community. To be sure, the field was launched in the 1950s and 1960s by a series of monographs by Ernst Haas, Leon Lindberg, Joseph Nye, and others. The next two decades, however, saw a series of substantively broad-ranging yet theoretically sophisticated volumes assessing the state of the European Community, including those edited by Leon Lindberg and Stuart Scheingold, Helen and William Wallace and Carole Webb, Alberta Sbragia, and (most recently) Robert Keohane and Stanley Hoffmann.

Like its distinguished predecessors, each of the books reviewed here has two stated purposes: to document the current condition of European integration and to theorize about it. The 1992 Project, edited by Dale Smith and James Lee Ray, contains 12 essays, four on integration theory and eight on specific topics ranging from defense cooperation to the role of subnational development agencies. Without striving for comprehensive coverage, the authors uncover a wealth of interesting information. Several essays cut sharply against the grain of conventional wisdom. John Conybeare rigorously grounds his skepticism about the future of transatlantic trade relations in optimal-tariff theory. Paulette Kurzer offers a provocative analysis of the European Monetary System as instrument and indication of the "passing away . . . of social democracy and the postwar settlement" in small corporatist democracies (p. 140). David Garnham traces in intriguing detail the Anglo-French flirtation in 1990 with an anti-German alliance. Russell Dalton and Richard Eichenberg seek to revitalize the study of public opinion and integration.

As its title suggests, *The Maastricht Debates and Beyond*, edited by Alan Cafruny and Glenda Rosenthal, does aim at comprehensiveness—and splendidly achieves it. Second volume in a biennial series sponsored by the European Community Studies Association, it contains a rich and timely collection of 23 essays on European Community developments over the past two years. The reader senses the unusually astute eyes and firm hands of two editors committed to maintaining a strong standard of selection and execution. The essays are uniformly concise and focused, yet lively.

Cafruny and Rosenthal seek to provide a detailed overview of issues ranging from immigration to telecommunications and (though with less enthusiasm than Smith and Ray) to provide social scientific explanation of those events. In the service of the first goal, special mention must be made of the useful, detailed review of the Uruguay Round negotiations by Finn Laursen; Sir William Nicoll's analysis of the Maastricht negotiations, which reflects an intriguing mixture of federalism and skepticism; and the especially thorough treatment accorded Germany and the EMU negotiations. On the latter topic, David Andrews and Wayne Sandholtz document (as widely reported at the time) the initial success of Helmut Kohl's personal efforts to strengthen, at least on the margin, the German commitment to monetary integration. Particularly insightful is Paul Welfens's analysis of the macroeconomic contradictions of Bundesbank policy.

Here, too, are contributions that employ social-scientific analysis to generate unexpected insights. Dale Smith and Jürgen Wanke make elegant use of descriptive statistics to determine who wins and who loses by "Europe 1992." Walter Goldstein provides a toughminded review of the European Community's internal tension between market liberalism and industrial policy. Robert Geyer carefully compares British and Norwegian socialism in the new Europe.

Yet, while both books mention fundamental theoretical questions of European Community studies, they ultimately decline to provide distinctive answers to them. The current conjuncture of the Community has raised many such questions, many closely linked with enduring concerns of comparative and international politics: How does the relative power of the Commission and member governments vary across issues? Does Community integration strengthen or weaken the state internally? Under what conditions and in what ways does membership in the Community alter the relative influence of domestic political groups? Has national support for European integration undergone a fundamental process of politicization, transforming a "passive" into an "active" consensus? Under what conditions do European countries share an underlying interest in moving from "negative" to "positive" integration, that is, from market liberalization to the joint provision of public goods like macroeconomic stability, environmental protection, and a common defense? Is the Community evolving into a new sort of polity, or do present trends presage an extension of long-term patterns?

If a theoretical theme can be discerned in these two volumes, it concerns the last question. Both books stress the increasing complexity and diversity of the European Community. For Smith and Ray, complexity emerges, above all, from the Community's institutional structure. The European Community is one of the few interna-

tional organizations with a supranational officialdom possessing significant, though limited, autonomy. With the inclusion of another actor and forum, what Robert Putnam has termed a "two-level game," played by statesmen on domestic and international "boards," becomes a "three-level game." (Indeed, Smith and Ray hint that Community external relations and transnational coalitions add a fourth and fifth level!) At the same time, the Commission seeks to empower subnational actors, thereby further diffusing political power. In his analysis of regional policy in The Maastricht Debates, Gary Marks asserts that "we are seeing the emergence of a multilevel governance, a system of continuous negotiation among nested governments at several territorial tiers—supranational, national, regional, and local" (p. 392). A causal link between greater complexity in policymaking and greater diffusion of political power is surely an important proposition about the development of the European Community (although, it must be added, a more highly debatable one than either volume hints). My own view is that on balance, far from undermining the state domestically, the Community actually strengthens it.

Even if we provisionally accept the hypothesis that complexity is diffusing power in some issue areas, simply to celebrate complexity is to make a virtue of necessity. The next step, an essential one for social scientists, moves from observation to explanation. What is the theoretical significance of complexity and diffusion of power? Under what conditions does the relation hold? What are the implications for integration? Here both volumes leave the field to others. Despite their admirable ambition in this regard, Smith and Ray do not move beyond the metaphor of "three-level games" to theory—retreating, instead, in their concluding chapter, to the reassertion that political union is a sort of European destiny. Perhaps constrained by the format of their book, Cafruny and Rosenthal, too, make little attempt to postulate an overarching model or a common set of hypotheses, contenting themselves, instead, with the modest conclusion that intergovernmental negotiations between member states appears to remain central. Marks asserts only that the starting point must be the assumption that the Community is a "complex, opentextured and fluid" polity. But if this is the theoretical starting point, where is the endpoint?

The problem is not a new one. It closely resembles the predicament facing theorists of "complex interdependence" in the 1970s, who found that the assumption that everything is connected to everything else is, ultimately, no assumption at all. Only the more detailed analyses of trade and capital interdependence since the mid-1980s, firmly grounded in explicit microfoundations, have provided strong testable propositions. The study of interdependence has grown richer as a result. Similarly in European Community studies, analyses such as those found in these two books perform an indispensable function by opening our minds to a wide range of possibilities; but it now falls to theoretically structured empirical analyses to help separate the wheat from the chaff. It is here-more than in reviews of recent eventsthat social scientists can claim a distinctive comparative advantage over the myriad annual reviews of the European Community that have sprung up in recent years.

There are two complementary paths forward. The first, as Leon Lindberg notes in the Smith and Ray volume, is to develop more explicit, generalizable theories of economic interest, interstate bargaining, and

international regimes. Modern theories based on specified causal mechanisms at the micro level offer many of the tools to move beyond ideal-typical description to explanation. Recent works by Giandomenico Majone and George Tsebelis, for example, suggest subtle, counterintuitive consequences of complexity in the Community. The second path is to pursue serious cross-issue, cross-national, and cross-temporal comparative analysis. The sudden success of the 1992 initiative has encouraged, among scholars, a historical myopia in European Community studies that is more pervasive, yet considerably less justified, than that of the 1960s. Moreover, there is a dangerous tendency to generalize from specific issues or single decisions. Only explicitly comparative analysis-within the Community and between the Community and other organizations) can tell us whether the current conjuncture is the continuation of old politics under new circumstances or the emergence of a new institutional patterns of politics altogether.

This sort of sustained social-scientific inquiry is quite consistent with the careful analysis of recent events found in these two books. One hopes that forthcoming volumes of this type will serve as vehicles to unite the two.

Harvard University

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Andrew Moravcsik

The New Global Economy in the Information Age: Reflections on Our Changing World. By Martin Carnoy, Manuel Castells, Stephen S. Cohen, and Fernando Henrique Cardoso. University Park: Pennsylvania State University Press, 1993. \$25.00 cloth, \$12.95 paper.

For the reader looking for new arguments or evidence about change in the world economy, this book will be a disappointment. On the other hand, it offers a succinct, albeit one-sided, summary of familiar arguments and data about the world economy in four areas (each treated by a different author): the new world informational economy, the continuing power of multinational enterprises, the decline of American hegemony, and the marginalization of the poor developing countries.

The underlying theme is that national policies still matter in the new world economy. Manuel Castells, in the opening essay, argues that the "'developmental state', a code word for Japan, played a fundamental role in affecting the transformation of the world economic structure in recent years" (p. 23). Martin Carnoy writes that although "multinational enterprises are powerful economic actors, . . . nation-states can also be key actors in shaping what multinationals do," especially in promoting local human-capital development and researchand-development financing (p. 93). Stephen C. Cohen is even more emphatic that national policies dictate success. He points primarily to a "set of institutional innovations" pioneered by Japan that employ the state as gatekeeper, the keiretsu industrial organization, and the targeting of key technologies and industries (p. 100). Finally, Fernando Henrique Cardoso concludes that developing nations, too, can succeed if they pursue the right policies: "Either the South (or a portion of it) enters democratic-technological-scientific race, invests heavily in R&D, and 'endures the information economy' metamorphosis, or it becomes unimportant, unexploited and unexploitable" (p. 156).

The theme of the book is directed against those who

claim that freedom (individualism) and global markets have triumphed and that national policy choices must be subordinated to economics. This theme is welcomed as a corrective to an earlier overemphasis on interdependence and dependence, to which some of these same writers contributed, particularly Cardoso. But the authors are a bit one-sided in what they consider the right national policy choices to be.

They see Japan as the preeminent leader of the new informational economy. In fact, Japan lags in the information age. According to Bill Baumol and his associates (Productivity and American Leadership), both Japan and Europe have a smaller share of their labor force in the new information and service sectors than does the United States (57% and 59%, respectively, compared to 71% in the United States). In addition, the United States is more competitive in the information economy. Recent studies by McKinsey, the international management consulting firm, show that American manufacturing and service industries have restructured well ahead of Japan and Europe and today enjoy a substantial lead in the information sectors that is, telecommunications, software, computer networking, multimedia technologies, and financial services. Ignoring these data, the authors also give no hint of the fundamental structural problems faced in Japan (and Europe). Recently, Japanese investment and growth have slowed precipitously. In 1992, Japanese manufacturing productivity dropped 6.2% while that of the United States rose 4.2%.

The volume also overstates America's decline. It repeats the litany of criticism of U.S. policies in the 1980s but says nothing about the downsizing and productivity improvements in American industry in that decade or the powerful surge of American exports from 1986 to 1992. American manufacturing productivity grew at a rate of 3.5% per year in the 1980s, against a rate of 2.5% per year in the 1950s and 1960s, and American exports doubled over the five years prior to 1993, regaining market share around the world. There is much to criticize in American policies in the 1980s, but it is an overreaction to label these policies "a romantic reaction, a return to the simplicities and uncertainties of America's uplifting past" (p. 123).

The failure to the drawbacks of Japanese—or the benefits of American—policy choices leads to curious recommendations for European policy. Europe is counseled neither to deregulate aggressively as in "Europe 1992" or to exploit trade and investment opportunities with the low-wage countries of Eastern Europe and the former Soviet republics as the United States deregulated and shifted production to the Asian advancing nations. Instead, Europe is urged to maintain its welfare system, develop national champions (even though elsewhere the authors acknowledge this strategy has failed), and pursue a European-level response involving "substantial amounts of government action in order to protect and transform the structure of European industries in profound ways" (pp. 95, 143). No mention is made of Europe's near-catastrophic unemployment problem (12%), budget deficits in Europe that on average are larger than the U.S. deficit (5.1% compared to 4.5% in 1992), or wage and benefit costs in Europe that far exceed those in Japan and the United States and cripple European competitiveness, precisely in those sectors where former communist countries could supply European markets much more efficiently and equitably.

The essay on developing nations is more reflective.

Cardoso, who did much to popularize the role of the state and dependency in North-South relations, now concedes that "politics is not the center of all change" and that "the theory of exploitation (of classes and of nations) has been shaken by an earthquake of magnitude 8 on the Richter scale" (pp. 151-52). Contrary to dependency logic, much of the South did develop; and it did so, according to Cardoso, not because of "more" or "less" state intervention but because of "better" state policies reflecting the "new rationality" of "decentralized forms of management and decisionmaking" (p. 155). Unless poor developing nations adopt the same policies, Cardoso fears they face an even crueler fate of marginalization and irrelevance. Unwilling to jettison his socialist preferences yet ambivalent about centralized policies, Cardoso calls for a democracy of "collective humanism" that creates "public spaces" and "a plurality of decisionmaking levels, making feasible a 'polyarchical utopia'," whatever that may be (p. 155).

Most of the essays were completed before 1992. An epilogue is therefore added that points to Clinton's election, the rise of nationalism in the former communist world, and the grass roots, populist revolt against the Maastricht Treaty as proof of the book's thesis that national policies are essential to rein in the individualist and laissez-faire tendencies of global economic liberalism. "The source of Clinton's landslide election," the authors conclude, "was his program to revive the American economy, with emphasis on American" (p. 162). While landslide is apparently meant to include the Ross Perot vote, the attempt to validate scholarly analysis with electoral results only confirms the one-sidedness of this study.

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George Washington University

HENRY R. NAU

International Peacekeeping. By Paul F. Diehl. Baltimore: Johns Hopkins University Press, 1993. 211p. \$36.50.

Alas for authors in the field of international relations in the mid-1990s and, above all, for those who write about something as contemporary as United Nations peacekeeping! International Peacekeeping evidently went to press just before the "three cases from hell"—Bosnia, Somalia and Haiti—hit the political fan. This is a shame, because it might have enlivened a rather pedestrian, albeit comprehensive, survey of the universe of multi-lateral peacekeeping. The experiences of UNEF I and II, UNFICYP in Cyprus, UNOFIL in southern Lebanon, and others are all well known and not particularly controversial, to the point where a new truce-supervising peacekeeping venture is a dog-bites-man event.

But since 1991, the United Nations has ventured further into an unchartered and distinctly controversial realm, variously called "peacemaking," "humanitarian intervention," and (in Secretary General Boutros Boutros-Ghali's word) "peace enforcement." It began promisingly in northern Iraq after the Gulf War ended, with a still-resilient Saddam Hussein now battering his Kurdish population in the north and Shi'ites in the south. Public indignation drove the United States and United Nations (an inseparable pair for this purpose) to help those hapless souls despite the opposition of the Iraqi government—a first for peacekeepers. The breakup of Yugoslavia raised similar humanitarian concerns, as did anarchy and famine in Somalia and trash-

ing of democratic government in Haiti. But Bosnia revealed the depth of Europe's moral insufficiency, Somalia and Haiti the superficiality of American's new commitment to multilateralism. It was not just manbites-dog but watch-everyone's-hair-catch-fire in New York and Washington. The upshot was a jamming on of the multilateral brakes and an agonizing reappraisal of U.S. policy.

The present author missed all of this through no fault of his own. But his rather plodding approach to traditional peacekeeping (one of history's genuine political—military innovations) suggests that even an updated version of the book might not stimulate the student or scholar to pursue it further. That is the bad news. The good news is that for an inventory of names and numbers of the players, along with some references to relevant political doctrines that are buried in the text, this book is probably the most recent on the topic.

Massachusetts Institute of Technology

LINCOLN P. BLOOMFIELD

Bridging the Gap: Theory and Practice in Foreign Policy. By Alexander L. George. Washington: United States Institute of Peace Press, 1993. 170p. \$24.95 cloth, \$14.95 paper.

Alexander George undertakes an ambitious task in Bridging the Gap and does a more-than-creditable job in accomplishing it. George sets out to "encourage better communication and closer collaboration between academic scholars who study foreign policy and practitioners who conduct it." After describing the disparate cultures of academia and policy-making, George gets to the heart of his argument by identifying three types of knowledge that can help policymakers decide whether and how to employ a particular strategy: abstract, conceptual models of each strategy; generic knowledge of the conditions that impinge upon the likelihood of success of a strategy; and actor-specific, behavioral models of the adversary. George carefully analyzes U.S. policy toward Iraq from 1988 through 1991 and concludes that there was an inadequate knowledge base for that policy. In the book's concluding chapter, he offers six implications for scholarly research and policymaking.

Bridging the Gap does an excellent job of diagnosis. The author's substantial experience in both the academic and government policy-making worlds combines with his insight to allow him to present a cogent analysis of the differences between the cultures of academia and government policy-making. Both academics and policymakers should find themselves nodding in agreement with George's observations about the two cultures. The "gap" may or may not be bridgeable but the necessary first step in attempting such a task is the type of carefully thought-out diagnosis of the problem that George provides.

The three types of knowledge that George puts forth, as essential for helping policymakers decide whether and how to employ a particular strategy are all quite important. I find the call for such models to be highly insightful, but I do urge an embellishment of that call. In order to understand and assess virtually any set of

circumstances in foreign policy (including the recent U.S. policy toward Iraq that George uses as his primary example), one must analyze more than dyadic interactions. It is important both that such actor-specific behavioral models include the adversary's view of other actors and that they be developed for more actors than just the acknowledged adversary. These embellishments would substantially enhance the policy utility of such models

George understands that theory can be a pejorative term in the policy-making community. For that reason, he employs the term generic knowledge when arguing for the relevance of theory. My first reaction is sadness at George's undoubtedly correct analysis of the potential response of many to the term theory. However, I am buoyed by the emphasis, within that discussion, on what one might term scope conditions for generalizability. Such scope conditions are clearly a critical part of any bridge between theory and practice. The plea for such scope conditions is one that, if heeded, would also enhance communications between a variety of academic communities. Also, George is to be commended both for addressing some of the shortcomings of applying a strictly neorealist paradigm to foreign policy questions and for highlighting research that more carefully analyzes the theory of appeasement than any other post-World War II author of whom I am aware. George attempts to apply evidence from more cases than Nevelle Chamberlain's ill-fated policy toward Hitler in drawing his conclusions.

As George undoubtedly appreciates, he is the latest in a long line of scholars attempting to bridge the gap between theory and practice in foreign policy. In much of Bridging the Gap, I am reminded of Davis Bobrow's "Relevance Potential of Different Products" (World Politics [1969]). Among other similarities, both argue for the relevance of theory, the importance of bridging the gap, and then offer a "construction plan" for the bridge. However, where Bobrow concentrated more on the importance of focusing on policy-manipulable variables (also mentioned by George) and offered only a variety of brief examples, George's use of a set of examples all drawn from U.S. policy toward Iraq communicates well to readers in both academic and policymaking communities. Emphasizing the relevance of the proposals that George is making by applying them to such a recent foreign policy case will certainly broaden the audience that pays attention to his message.

Building upon the message of George's book would be an important next step. In his concluding chapter, he provides a number of possible avenues for accomplishing this. I will now add to that list. A more specific and comprehensive application of the types of knowledge for which he calls is crucial. Either recent U.S. policy toward Iraq or another case should be employed even more comprehensively to show exactly how the types of knowledge for which George calls could be employed to produce improved policy results over failure to employ such knowledge. George outlines what models employing the three types of knowledge should contain. Building such models based on "conditional generalizations" and interacting with policymakers about them might start the type of dialogue that George is seeking.

Ohio State University

DONALD SYLVAN

Minorities at Risk: A Global View of Ethnopolitical Conflicts. By Ted Robert Gurr. Washington: U.S. Institute of Peace Press, 1993. 448p. \$37.50 cloth, \$24.95 paper.

Ted Robert Gurr is probably best known for his Why Men Rebel (1970) which remains the fullest application of relative deprivation theory to conflict analysis. In Minorities at Risk, Gurr surveys the world to present "an integrated substantive and empirical analysis of communal status and conflict since the end of World War II, with special attention to the decade of the 1980's" (p. xi). This time, Gurr steers a median course between relative deprivation and group mobilization explanations of conflict and also between primordialist and instrumentalist interpretations of ethnicity. In this reasonable trade-off, theoretical incisiveness is sacrificed to a looser analysis that better accommodates a multitude of cases.

The empirical core of the book is a comparative, cross-national analysis of 233 politically active communal groups (whose members number some 915 million persons) in 93 countries. These groups are coded into five categories: ethnonationalists, large groups with a history and dream of political autonomy (e.g. Croats, Québecois); indigenous peoples, conquered descendants of original inhabitants (Miskitos, Maoris); ethnoclasses, low-status minorities descended from slaves or immigrants (African-Americans, Koreans in Japan); militant sects, communities politically defined by religion (Northern Irish Catholics, Lebanese Maronites); and communal contenders, culturally distinct groups seeking to retain or augment their share of the national pie, who may be subcoded as advantaged or disadvantaged (Tutsi of Burundi, Chinese of Malaysia, respectively). The boundaries of both groups and categories are admittedly "fuzzy," and several groups (notably, all but 8 of the 43militant sects) are coded under more than one category.

Specialists will properly quibble about coding and terminology: Why should the Masai be more "indigenous" than the other peoples of Kenya and Tanzania? Is it not at least a misnomer to apply the label "militant" to the Baha'is of Iran? Still, the categories are firm enough to yield useful empirical statements when plotted against indices of cultural differences, discrimination, and types and frequency of political action. As Gurr shows, overall, every form of ethnopolitical conflict has increased since the 1950s, but trends differ widely among regions. For example, within Western democracies, conflict peaked sharply in the 1970s, then declined, while in Eastern Europe, it increased sharply in the 1980s. Cultural differences have no effect on inequalities for ethnoclasses or communal contenders, but they are associated with discrimination against ethnonationalists. In general, cultural differences have a greater effect on economic than on political disadvantage. Religious cleavages are seldom at the bottom of communal conflicts, though they may help exacerbate such conflicts. While Gurr's categories are not ideally set up for this purpose, little in his findings would provide empirical comfort for a Huntingtonian vision of a world about to be riven by a "clash of civilizations."

Four of the book's 11 chapters provide regional context and exemplification for the global generalizations. Monty G. Marshall deals with Eastern Europe and the former USSR, Barbara Harff with North Africa and the Middle East, and James R. Scarritt with sub-Saharan Africa, while Gurr himself takes on the Western democ-

racies and Japan. Useful as these chapters are, none has the space to provide the detail and the qualifications one would like. The sub-Saharan Africa chapter strikes the most graceful balance between reverence for the comparative framework and the specific demands of local experience, though too much weight is placed on the colonial experience in accounting for the ethnic diversity of African states. The East European chapter spends disproportionate space on discussing the differences between Lenin's and Stalin's views of the "national question." The North Africa and Middle East chapter's shaky discussion of Berber-Arab relations in Morocco should be bolstered by a more systematic comparison with such relations in Algeria. And who let slip by the howler assigning the Turkish language an "Indo-Germanic origin?" (p. 221).

Although Minorities at Risk does not provide a major conceptual or methodological breakthrough, it provides a wealth of useful empirical material and a model of large-scale cross-national comparative work on complex and important subject matter. The book should quickly find its way onto the required reading list for courses on nationalism and ethnic conflict, where it would provide stimulating intellectual counterpoint for more speculative exercises of historical interpretation like Anderson's Imagined Communities (1983) and Moynihan's Pandaemonium (1993). It also should provide those immersed in regional studies with a comparative context for their more parochial discussions of communal conflict. In an era when support for large-scale empirical work is hard to come by, the U.S. Institute of Peace is to be congratulated for having sponsored and sustained this ambitious project.

Yale University

WILLIAM J. FOLTZ

The Politics of Economic Adjustment. Edited by Stephan Haggard and Robert R. Kaufman. Princeton: Princeton University Press, 1992. 356p. \$49.50 cloth, \$16.95, paper.

Since the early 1980s, the challenge of adjustment has dominated economic policy throughout the developing world. In many countries, economic reform has been concurrent with democratization and regime change. Neoclassical reform measures, urged upon indebted countries by multilateral lenders and G-7 governments, pose significant challenges to the sovereignty, ideology and domestic prerogatives of state elites. Stabilization and adjustment programs demand substantial administrative capacity and political skill, while the powerful distributive conflicts unleashed by economic change threaten the legitimacy and stability of incumbent regimes. Advocates of neoclassical reform suggest its compatibility with democratization, yet the relationship between political and economic liberalization remains ambiguous.

The Politics of Economic Adjustment represents an impressive effort to summarize the political legacy of economic reform, and to clarify the elusive relationship between political transition and economic change. In its comparative breadth and theoretical depth, this volume has few peers in the contemporary literature in comparative political economy. The chapters offer important contributions to current debates on economic change and regime transition, and the book provides a wealth of

policy-relevant insights which might also be applied to the transition economies of Central and Eastern Europe.

The Politics of Economic Adjustment is the third volume in a series of studies on the politics of economic reform. The companion works, Fragile Coalitions: The Politics of Economic Adjustment (1989) and Economic Crisis and Policy Choice (1990), both edited by Joan Nelson, were respectively devoted to the policy dynamics of reform and a series of comparative case studies of economic adjustment in Latin America, Africa and Asia. The volume under review seeks to synthesize the conclusions of the earlier efforts, and to offer a number of middle-range propositions about the political context of adjustment. The book's seven extended chapters are organized around four broad themes: the relative salience of international influences and domestic actors in the process of economic adjustment; the role of the state in economic restructuring and the scheme of state reform; the distributive implications of stabilization and adjustment strategies; and the relationship of economic adjustment and democracy.

These are controversial issues, and the analyses in this volume offer critical revisions of many common premises regarding politics and economic reform. One such notion is that orthodox programs have been successfully imposed by a monolithic creditors' cartel, led by the IMF and the World Bank. In the opening section, Barbara Stallings and Miles Kahler provide discerning evaluations of the international context of economic adjustment. Stallings considers the distinct roles of international markets, policy networks and northern-dominated regimes in promoting reform among developing countries. While external structural constraints and transnational networks provided complementary channels influencing an initial shift toward orthodox adjustment, Stallings notes that international factors are less compelling in accounting for policy implementation and outcomes.

Échoing this caveat, Miles Kahler acknowledges the importance of international factors in agenda-setting, but observes that international financial institutions and northern commercial banks had relatively weak leverage over wide realms of debtor-country policy choice and implementation. In fact, the global regime governing debt and adjustment weakened throughout the 1980s, as the "conditionality bargain" offered by creditors failed to offer substantial incentives for imposing politically costly austerity measures. Eventually non-conditional resources were renewed, and the unity of creditor groups frayed as they pursued divergent strategies. Many developing countries were able to evade conditionality or to pursue heterodox strategies.

The limits of international-level explanations point to a greater stress on the domestic politics of reform, and succeeding chapters offer both state-level and society-centered analyses. A second perspective challenged in this volume is the neoclassical assumption that state intervention is inherently detrimental to economic growth. In the book's strongest theoretical chapter, Peter Evans reassesses debates about the developmental state. States confront basic collective action problems, observes Evans, and neither arguments about the rent-seeking "predatory" state, nor depictions of a coherent, autonomous "developmental" regime adequately capture the political qualities leading to economic success. Evans offers the compelling notion of "embedded autonomy" to portray the character of developmental

states. Such regimes embody internal coherence, discipline and autonomy from fragmentary societal interests, yet they also maintain diverse linkages with society through personal networks and corporatist arrangements, which serve to garner information and foster compliance.

John Waterbury's comparative analysis of public sector reform in Egypt, India, Mexico, and Turkey provides a laudable attempt to portray the scheme of state reform in more empirical fashion. Waterbury notes that heavily statist economies give rise to similar forms of property rights and interest group coalitions, implying comparablity in the dynamics of reform. He emphasizes the importance of technocratic "change teams" within the state, endowed with a mandate to implement a reformist project. Impelled by growing fiscal constraints, reformminded state elites may surmount the scattered opposition of a statist coalition, and privatize or restructure state enterprises.

A third controversy confronted here is the impact of stabilization and adjustment on the poor, and effects of distributive tensions on social conflict and political stability. Joan Nelson analyses discrepancies between the differential costs of adjustment and the relative political salience of affected groups. While urban, formal sector groups experience greater losses under adjustment, the urban and rural poor are more vulnerable, since they possess fewer strategies for coping with price changes and income reductions. However, privileged urban constituencies exercise preeminent political influence, and are likely to block redistributive policies which could reduce the vulnerability of the very poor. Nonetheless, organized labor and urban dwellers are more likely to engage in sporadic, disparate protests against particular effects of reform rather than concerted protest against adjustment per se. Government strategies for dealing with affected groups, and general popular perceptions about economic management, may significantly assuage protest and opposition.

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In a comparative study of the politics of inflation and stabilization, Stephan Haggard and Robert Kaufman also stress regime factors rather than the strength of distributive coalitions. They find that inflationary tendencies are attributable to the relative security of elite tenure and the capacity of political institutions to resist distributive pressures. While recognizing the contribution of external shocks and devaluations to inflation, they emphasize that rates of inflation and proclivities toward stabilization are more closely related to regime type. They also observe that authoritarian governments evince few generic advantages over democracies in pursuing stabilization, although relatively cohesive authoritarian governments reflect a stronger record of limiting high inflations.

The focus on distributive pressures and regime response raises the central issue of political transition. Is democracy compatible with economic adjustment? Or, do the political costs of adjustment weaken transitional governments and foster authoritarian management? Haggard and Kaufman conclude with a chapter assessing the impact of economic crisis on regime transition. They refute the impression that economic decline in the 1980s prompted extensive regime change, and they observe that weak economic performance has not produced a widespread breakdown in transitional democracies since the early 1980s. They relate the stability of regimes, both authoritarian and democratic, to relative levels of elite cohesion and political institutionalization. The sequencing of political and economic change, and the configuration of executives and party structures, are crucial determinants of democratic transition and con-

There are few qualms one might raise about this collection. While the volume generally avoids the eclecticism of many edited works, the authors naturally hold divergent views over central questions. Certain authors seem too ready to accept the orthodox agenda as a framework for evaluating policy choice and performance, even developments prior to the 1980s. Moreover, the comparative analyses sometimes tend toward a welter of idiosyncratic qualifications, thus blurring the strengths of the comparative framework and obscuring important conclusions. These are largely stylistic weaknesses, however, and they should not detract from the commendable strengths of this book. The Politics of Economic Adjustment represents the most comprehensive comparative work in this area, and it is a collection of impressive analytical rigor and theoretical depth. It will provide a lasting touchstone for the study of economic and political reform.

American University

Peter M. Lewis

The United States and Latin America in the 1990s: Beyond the Cold War. Edited by Jonathan Hartlyn, Lars Schoultz, and Augusto Varas. Chapel Hill: University of North Carolina Press, 1992. 328p. \$39.95 cloth, \$15.95 paper.

The end of the Cold War marked a turning-point in inter-American relations. From the early nineteenth century through the late 1980s, the U.S. policy toward Latin America primarily focused on real, potential, or imagined threats to its national security from extrahemispheric powers operating in or through Latin American states. In particular, for much of the period from the late 1940s through the 1980s, U.S. decisionmakers molded military, economic, human rights, immigration, and other policies to counter the strategic and ideological challenge posed by the Soviet Union. Superpower rivalry strongly influenced both the foreign policies and the domestic politics of many Latin American countries. Thus, the end of the Cold War, in combination with important political changes in the United States and in major Latin American countries creates new opportunities for U.S.-Latin American cooperation.

This outstanding collection of essays examines key trends in U.S.-Latin American relations and the consequences of recent international and domestic political developments for hemispheric affairs. The editors and the 12 Latin American and U.S. contributors are among the foremost authorities in their fields. Together, their essays provide a comprehensive examination of both unresolved tensions in inter-American relations and the specific problems facing U.S. and Latin American poli-

cymakers in the 1990s.

The book begins with the editors' introductory essay and three overview chapters by Alberto van Klaveren, Augusto Varas, and Abraham F. Lowenthal. The authors of these essays concur that the end of U.S.-Soviet strategic rivalry in the Western Hemisphere, global economic restructuring, the democratization of foreign policymaking in the United States, and a widening consensus in major Latin American countries in support of political democracy and market-led economic development have significantly transformed U.S.—Latin American relations. Among other things, these changes have further eroded old foreign policy assumptions and produced greater heterogeneity in the bilateral foreign policies of hemispheric states.

Van Klaveren, for example, discusses Latin American countries' efforts since the 1970s to strengthen diplomatic and economic ties with West European countries and Japan, as well as the prospects for collective Latin American action on economic and security problems. Varas's chapter examines shifts over time between "hegemonic" and "coercive" paradigms of hemispheric order, noting that the coincidence of particularly favorable international and domestic conditions may lay the basis for "cooperation through partnership" in the 1990s. Varas argues that the "coercive continuities" in U.S. policy (most recently manifested in the invasion of Panama in December 1989 and the assumption that we retain the right to intervene in the affairs of Latin American states to protect its interests) can most effectively be constrained—and bipartisan consensus promoted-by issue-specific security regimes that reduce confusion between U.S. political goals and Latin American countries' security concerns. Lowenthal's essay convincingly demonstrates why, even after the disappearance of "the Soviet threat" in the Western Hemisphere, Latin America remains of vital importance to the United States. Among other reasons, hemispheric states are bound by issue agenda (migration, drug trafficking, environmental problems) that increasingly link different countries' domestic and foreign policies.

The introductory essays offer both useful historical background on inter-American political and economic relations and an assessment of the specific challenges that will confront Latin American and U.S. policymakers during the 1990s. However, they could have made an even stronger contribution by identifying more clearly the general principles that should guide U.S. and Latin American foreign policymakers now that the Cold War is over. The question whether existing inter-American institutions are capable of promoting cooperation and limiting conflict in this new era also merits further

The second part of this volume consists of essays examining domestic political trends in Latin America and specific policy areas in inter-American relations. The chapters by Rosario Espinal and Marcelo Cavarozzi analyze, in Latin America the evolution of the political Right and Left, respectively. Attention to changes in Latin American countries' domestic political situation is especially welcome in a book on U.S.—Latin American relations, and these essays offer far-ranging assessments of recent developments on the Right and the Left. Cavarozzi is, moreover, particularly effective at capturing the interplay between international conditions and domestic political change in the region since the 1930s.

Other authors assess the evolution of the Latin American debt problem in the 1980s (Riordan Roett), U.S.—Latin American trade relations (Roberto Bouzas), the issue of military involvement in Latin American politics and U.S. policies concerning democracy and human rights (J. Samuel Fitch), U.S. and Latin American policies on drug trafficking (Bruce Bagley and Juan Tokatlian), environmental problems (Steven Sanderson), and migration issues (Robert Bach). In addition to

providing comprehensive analyses of the main issues in these different areas, these chapters identify many of the unresolved dilemmas that will confront U.S. and Latin American decision makers during this decade. Bagley and Tokatlian, for example, show that despite a growing regional consensus in the 1980s that drug smuggling constitutes a major societal and national security problem, U.S. policy has lacked legitimacy and credibility because it is based on an inappropriate conceptual definition of the problem and a counterproductive U.S. preference for unilateral, often military, actions to resolve it. Bach demonstrates how domestic politics has influenced U.S. immigration policies over time, how these policies and other U.S. foreign policy goals are linked, and how U.S. policies have shaped migratory flows from Latin America.

In sum, the editors and contributors succeed admirably in assessing the foreign policy consequences of major changes in the international arena and in the domestic politics of hemispheric states, new sources of complexity in U.S.–Latin American relations, and potential sources of future cooperation and conflict. The book is a significant addition to the scholarly literature on inter-American relations. It is also an important resource for graduate and undergraduate teaching.

University of California, San Diego KEVIN J. MIDDLEBROOK

The New Cold War? Religious Nationalism Confronts the Secular State. By Mark Juergensmeyer. Berkeley: University of California Press, 1993. 292p. \$25.00.

This is an interesting book on a topic of contemporary concern. The rise of various ethnic and religious movements, which are challenging traditional authorities in many countries of the Second and Third Worlds, has led to the fear that the West faces a new confrontation with various forms of religious fundamentalism. The author does not like the term fundamentalism because it is pejorative, "is an imprecise category for making comparisons across cultures," and does not carry any political meaning (p. 5). Instead, he contrasts what he prefers to call religious nationalism with secular nationalism and, in a series of chapters, shows how the former has challenged the latter in a number of countries in the Middle East, South Asia, and the formerly communist countries in Central Asia and Eastern Europe. Then, in successive chapters, he addresses the questions, Why does religious nationalism turn violent? Is a religious state compatible with democracy and the maintenance of human rights? Can the West live with religious

The author sees the rise of religious nationalism as a reaction to the perceived failure of secular nationalism to deliver on "its promises of political freedom, economic prosperity, and social justice" and to the moral corruption secularism is claimed to have engendered (p. 23). He attempts to set the conflict between secular and religious nationalism in the perspective of the history of ideas, as well as Western history. This is the least satisfactory part of the book and seems jejune by contrast with the discussions of nationalism by Berlin in Against the Current, and Plamenatz in Kamenka's collection, Nationalism, and with the different strands of thought that have contended historically in the forma-

tion of the European state discussed by Oakeshott in his On Human Conduct, and, more recently, his Harvard Lectures, Morality and Politics in Modern Europe.

The most interesting chapter is the one in which he examines the reasons for violent religious confrontations and argues: "One can think of religious warfare as a blend of sacrifice and martyrdom . . . But behind the gruesome litany is an idea that encompasses both: . . . the dichotomy between the sacred and profane. This great encounter between cosmic forces—an ultimate good and evil, a divine truth and falsehood—is a war that worldly struggles only mimic" (p. 155). He also argues that the modern religious state need be neither antidemocratic nor against "the minimum definition of human rights—the notion that people should be able to reside peacefully alongside each other in dignity and with personal security" (p. 186). He makes a distinction between modernity, which religious nationalists are happy to embrace, and modernism ("the ideology of individualism and a relativist view of moral values, 191), which they are not. The religious nationalists are unlikely to support the "libertarian version of individual rights. . . That difference is so deep and abiding that it will not be easily resolved" (p. 189). But ultimately he thinks religion and reason, even if embodied in different nation-states, can coexist, and hence does not foresee any real danger of a new Cold War.

The main competitor of this volume is the second volume of the monumental study of fundamentalism organized by the American Academy of Arts and Sciences (AAAS), Marty and Appelby's collection, Fundamentalism and the State. Though the volume under review does have the merit of the greater coherence of a volume written by a single hand, it loses somewhat in terms of the richness of the AAAS volume. Also, it misses some important nuances. For instance, an important theme in the politics part of the AAAS volume is a distinction between the sectarian state and the nation-state, which leads to a finding that religious fundamentalists are much more likely to be successful in capturing the former. Moreover, the author virtually ignores the economic failures and successes that have distinguished states where fundamentalists (a term I still prefer to nationalists) have been resurgent from those where they have not. For instance, why have East Asia and the more successful countries in Latin America not seen a rise in religious fundamentalism? There is a storyline that can tie up all these loose ends. But even if Jurgensmeyer has not written the definitive book on religious fundamentalism, it is good to have some of the hysteria that has surrounded the subject in official circles and the media effectively dispelled.

University of California, Los Angeles

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DEEPAK LAL

Protecting Markets: U.S. Policy and the World Grain Trade. By Ronald T. Libby. Ithaca: Cornell University Press, 1992. 152p. \$26.50.

Regulating Unfair Trade. By Pietro S. Nivola. Washington: Brookings Institution, 1993. 190p. \$31.95 cloth, \$12.95 paper.

These two short books provide considered, scholarly examinations of U.S. efforts during the 1980s to counteract objectionable trading practices of other countries and ensure a "level playing field" for U.S. firms. Al-

though both hedge their bets, their conclusions are at odds.

The Nivola volume is broader and more comprehensive and carries its analysis closer to the present. It provides a partial answer to a key question: If trade negotiations under the GATT were so successful, why is the world slipping toward increased protectionism and perhaps even a trade war? Nivola's basic thrust is that trade regulation is being asked to do to much. Policymakers are unlikely to be able to fix broad macroeconomic failings simply by stiffening U.S. trade regulations. In effect, Nivola urges that trade policy actions ought to be aimed at remedying trade problems. He offers policy suggestions that the United States might adopt to refocus attention on open markets in reality, not just in rhetoric.

Sequential rounds of GATT trade negotiations made it politically unfashionable to maintain high tariff barriers. So to protect and encourage domestic industries, many countries started relying more on nontariff barriers. Frustrated, U.S. legislators demanded that in the name of equity, fair trade take precedence over free trade as the central U.S. trade objective. Their challenge was to help their beleaguered constituents without resorting to overt protectionism. To accomplish this task, the United States filed a growing number of antidumping suits and countervailing duties, negotiated many new orderly marketing arrangements and voluntary export restraint programs, and experimented with new approaches such as the Super 301 program and the Structural Impediments Initiative.

Legislators did not want to be portrayed as protectionists, so they instead tried to aid firms and workers injured by unfair competition. Nivola reviews both congressional and presidential initiatives. On the legislative side, the march from the watershed Trade Act of 1974, to the Trade Act of 1979, the Trade and Tariff Act of 1984, and (finally) the Omnibus Trade and Competitiveness Act of 1988 are summarized. At the same time, the increasingly activist intervention by the executive branch (sometimes in collaboration with Congress) is described. Still, by the mid-1980s it was clear that the international competitiveness of the U.S. automobile, steel, footwear, and textile industries was still poor.

What is largely absent from this analysis is a serious examination of the areas in which the United States experimented with new ways of encouraging freer trade, particularly with regard to so-called new issues and in smaller forums. As this volume makes abundantly clear, there is no such thing as a coherent trade policy. Thus, greater attention to U.S. efforts to liberalize trade in services and develop new understandings covering the protection of intellectual property and the handling of trade-related investment measures would have complemented the analysis. And consideration of the efforts by the Reagan, Bush, and Clinton administrations to achieve free-trade agreements with Israel, Canada, and Mexico are absent.

Still, there is an elegant power in Nivola's argument that builds with simplicity, logic, balance, and ultimately deliberateness and reasonableness. Nivola sets out to make his point and moves step by step through his argument. Nothing is rash, nothing is surprising. There are no bombshells as the weight of the argument builds steadily with each chapter. Nivola shows how the U.S. executive and legislative branches set about to remedy the perception of foreign unfairness and bolster flagging

American confidence (most often without requiring U.S. firms to correct their wayward habits). One result was a proliferation of moves to try to remedy unfair trade practices. Another result was the politicizing of U.S. trade policy. Indeed, the full-court political press, in the light of tremendous public scrutiny, that was necessary to persuade Congress to ratify the North American Free Trade Agreement is a perfect illustration. Until recently, such an agreement would have passed Congress easily without fanfare.

The recommendations offered by Nivola are modest and sensible, if unsurprising. He dismisses the idea that the United States should simply abandon efforts against unfair trade as politically naive and unrealistic. But he worries that crafting a consistently successful strategic trade policy is beyond the ability of government officials. He concludes rather tamely that the United States needs less dead-end moralism and that it should go back to basics. It "ought to keep pressing for equitable rules of commerce abroad but also carry out economic changes needed to make government a better partner of industry at home" (p. 142).

By contrast, Ronald Libby's Protecting Markets is a narrow case study that examines whether the Export Enhancement Program (EEP) adopted by the Reagan administration in 1985 to subsidize U.S. agricultural exports could be counted as a successful experiment in mercantilist trade policy. Libby states that the EEP was designed to restore U.S. grain markets that had been lost to unfairly subsidized European Community grain exports and to increase Europe's financial burden under its Common Agricultural Policy (CAP), thus diminishing its ability to engage in a subsidy war with the United States. The idea was to win back markets and to create such high levels of tensions within the European Community-particularly between the United Kingdom and France—that the Community would agree to negotiate reduced agricultural subsidies within the context of the Uruguay Round of multilateral trade negotiations in the GATT. Libby concludes that this goal was met-that Europe agreed to negotiate to reduce trade barriers under the pressure of discriminatory trade measures imposed by the United States.

This is a curious, slim volume with much to commend it, but its specific conclusions are ultimately unconvincing. This makes it hard to generalize from them. The book provides a careful, detailed analysis of political economy of U.S.–French (more than European) rivalry to capture cereal export markets for their producers. It shows how European (particularly, French) exporters aided by huge subsidies under the CAP displaced U.S. farm exports in the two decades preceding 1985. The interactions among France, Germany, and the United Kingdom with regard to agricultural trade are examined in detail.

In the 1980s, the United States started to act more forcefully to counter the loss of agricultural markets: "From the EEP's inception until October 1991, 119 export initiatives were approved by the USDA involving 80 countries and twelve commodities" (p. 53). The description and analysis of the continuing tussle between the United States and Europe in the mid-1980s, just before the designation of agriculture as a central agenda item of the Uruguay Round negotiations, is the best part of the book. Sadly, there is meager examination of the intricacies of negotiations on agriculture during the trade round itself.

Libby's claim that the EEP was an effective mercantile policy depends on whether or not you are a U.S. agricultural exporter. The United States did at least temporarily regain some markets for U.S. exporters, in part because of the EEP; but American taxpayers footed the bill for almost \$3.8 billion from 1985 to late 1991. Ironically, Cargill, the largest single U.S. beneficiary, consistently opposed the program but was nonetheless awarded nearly \$700 million in bonuses between 1985 and 1991. Some citydwellers might consider this a high price to pay to U.S. agricultural interests just to get the European Community to talk about agricultural subsidies, particularly if larger supplies in the United States might have kept prices lower. Moreover, farmers could well conclude that it was better to keep the subsidies provided under the EEP than to try to compete in an unsubsidized world. Even the Blair House compromise, which may help conclude the Uruguay Round talks (which took place after this book was completed), involves only small concessions by the French and ones that they would like to revisit.

Indeed, Libby undercuts the life of his own case for mercantilist intervention when he cautiously (and probably correctly) concludes, "Although the EEP was effective as a diplomatic bargaining tool only when U.S. grain supplies were large, world prices low, competition keen, the value of the U.S. dollar low, and most crucially political tension over the burgeoning CAP budget high, a U.S. recovery from the drought and renewed political tensions in the EC undoubtedly will make the Export Enhancement Program an effective lever in future GATT negotiations" (p. 130). That is a thin reed on which to advocate renewed mercantilism. Overall, score one for Nivola.

University of Southern California

JONATHAN D. ARONSON

Gendered States: Feminist (Re)Visions of International Relations Theory. Edited by V. Spike Peterson. Boulder: Lynne Rienner, 1992. 225p. \$32.00 cloth, \$16.95 paper.

Gendered States provides an important critique of international relations theory by problematizing one of the theory's most central and privileged concepts, the state. The book provides critical analysis and deconstruction of the relationship between gender and a number of global issues, including security, sovereignty, and revolution. The contributors uncover the centrality of the too-littlequestioned conceptualization of the state to traditional international relations theory and unmask the gendered privileges that states accord select men. Many problems associated with international-relations theory's conceptualization of the state-including how the state oppresses, degrades, and even coopts women to maintain and reproduce itself and expand its will around the world-are highlighted. The contributors, mostly feminist international-relations scholars, argue that a feminist analysis of the state and international-relations theory is central to an understanding and recreation of the world system. Consequently, the authors attempt to go beyond critique by furnishing a discussion of what they envision as significant aspects of a feminist international-relations theory

The volume's strength lies in its analysis of the impact

of the gendered nature of state formation, maintenance, and reproduction. This is done through three analyses: one focuses on the parallels between early Athenian and modern state formation, highlighting the differential roles (and corresponding power positions) accorded to men and women; one, on the continual oppression of women after modern revolutions; and one, on the lack of a feminist consciousness in the testimonies of contemporary military women coopted into the masculine culture of military service. These analyses convincingly show the state's historical and continued dependence on gender differentiations and the centrality of women and women's roles to global politics.

While the chapters make strong arguments for the importance of "taking gender seriously," the volume itself fails to take race, class, culture, sexuality, and other intragender differences seriously. Instead, the contributors privilege the history and experiences of select European women; and most chapters fail to include any serious analysis of the experiences of women of diverse backgrounds within and across states. At the same time that the volume challenges the masculinist account of political identity, autonomy, and community grounded in international relations theory, it replaces that account with accounts primarily based on the experiences of historically privileged European women. Most of the contributors assume-and at times suggest-that the developmental logic of the state and its corresponding subjugation are the same for all women, people of color, people of the lower classes, and people with alternative sexualities. This assumption results in the denial of differences in the oppression experiences of diverse peoples and obfuscates the role that states may accord some privileged women in the oppression of nonprivileged women, people of color, and poor people. The volume perpetuates the myth of the comprehensiveness of Western feminist analysis—similar to the myth of the comprehensiveness of realist analysis in internationalrelations theory.

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The volume raises questions whose answers may lead to a feminist revision of international-relations theory. The contributors (re)vision goes beyond simply adding women to the study of international relations. It contests many of the theory's foundations—including theoretical justifications for, and the practice of, utilitarian autonomy—and argues, instead, for "relational autonomy" between parties in the conduct of world politics. It argues for selectivity, by the exclusion of coopted female experiences, in the collection of women experiences on which to build its theory. It argues for the acknowledgement and analysis of the interconnections of gender, family, states, and revolution. It argues for the ecofeminist lenses of "fractious holism" that reject the appropriation and exploitation of women and nature. It argues for the rejection of notions of political identity as sacrifice. Most intriguingly, it argues for the maintenance of a much-revised liberal state where difference is not shunned and minorities are accorded political voice.

Even with these revisions, one wonders what a feminist international-relations theory inclusive of race, class, and sexuality would entail. Clearly, such a theory will have not only to uncover the multifaceted array of privilege, power, and hierarchy within and across states but also to explain how these oppressive structures of the world system can be dismantled and changed. Gendered States provides a sneak preview of what the underlying basis of a revised theory of international

relations should include. What the field may well need, however, is a new, not a revised, theory of international relations. In a time when very few of the immense global problems facing the world's peoples (over 50% of which are a diverse group of women) have gone unanticipated by international-relations scholars, movements to refine and redefine international-relations theory are necessary. Gendered States provides an initial blueprint of the kinds of questions a new theory of international relations must answer if we are to make any sense of the global village we live in.

Purdue University

PATRICIA T. MORRIS

Waiting for the Millennium: The United Nations and the Future of World Order. By J. Martin Rochester. Columbia: University of South Carolina Press, 1993. 347p. \$39.95.

There are several paradoxes associated with the United Nations. The organization has always been the object of contradictory attitudes: at the same time that many pin their hopes for world peace and security on the United Nations, others discard it as irrelevant and superfluous or even harmful and dysfunctional. Whereas summary judgments about the merits or demerits of the United Nations tend to be based on the organization's activity in the peace and security field, three-fourths of its resources are used in economic, social, and technical areas. Moreover, there is a wide gap between expectations and resources: the organization that is supposed to be the guarantor of peace and world order has an annual regular budget smaller than that of the New York City Police Department.

While noting such contradictions, Martin Rochester's study proceeds from another paradox. At a time when profound global changes have revived optimism about the future of the United Nations, international-relations theory is pervaded by an unprecedented skepticism toward the prospects of international organization. The regime literature is the specific target of Rochester's dispute with contemporary scholarship. His criticism is sometimes overstated, and his claim that scholars studying the United Nations have risked "pariah status in the international relations fraternity" is clearly exaggerated (p. 245). At any rate, Rochester proffers an alternative institutionalist and globalist approach to international order.

Trying to steer a course between "bad idealism" and "bad realism," the author launches an inquiry into the growth potential of global organization as one component of the quest for world order. He urges us to think of international system transformation as political development, understood as the cultivation of a world order that combines stability with adaptability. Adopting Karl Deutsch's terminology, he envisions a progression toward a global, pluralistic security-community. Rochester argues that the prerequisites of global institution building—a systemwide crisis and a critical mass of actors with the will and power to move the system—are present today. The main problem, in his view, is not a power vacuum but a leadership vacuum.

Rochester's assessment of the UN system starts with an overview of prominent shortcomings, including the lack of coordination between the specialized agencies, the fact that the politics of agenda setting tends to be divorced from the rest of the policy process, and the discrepancy between egalitarian voting rules and power inequalities among UN member states. This is followed by a thorough account of various reform proposals through the years, which shows a pattern of recycled ideas skirting the edges of institutional change.

Finally, Rochester offers a series of prescriptions based on a model of "dirigible pluralism" featuring subsystem autonomy within a central guidance system built around the United Nations. His reform proposals lean more toward "tinkering" than "rethinking" (to use the author's own terms). They can be realized with only marginal revisions of the UN Charter. First, Rochester recommends a number of measures to professionalize and depoliticize the UN Secretariat, including improvement of the secretary general selection process, more systematic use of competitive exams in recruiting personnel, centralization to a smaller number of superdepartments headed by Deputy Secretaries-General, and better intelligence capability through the creation of global-watch structures. Other prescriptions concern improved interagency coordination through the appointment of lead agencies and operations boards for specific functional area, budget process reform, and the restitution of ECOSOC to a pivotal role in bringing the programming and financing tracks closer together.

How, then, can such reforms be implemented, given the dismal track record of UN reform? Rochester envisions a trade-off—or enlightened, concert-of-powers approach—whereby major donors convert voluntary fundings into assessed funding in return for greater control

over the budget process.

Waiting for the Millennium is an informative, cogently reasoned, and (in many respects) impressive book. Its greatest merit is that it makes a conscious effort to embed the study of the United Nations in international relations and organization theory. In his ambition to recapture for international organization the high ground now held by regime theory, Rochester tends to spread his forces somewhat thin. His analysis ranges from broad macro levels (system transformation, world order) to narrow micro levels (organizational engineering); and he has descriptive, as well as normative, aspirations. The frequent quotes and references to an array of more or less relevant sets of literature underscore the eclectic character of his endeavor.

One limitation of Rochester's analysis is his preoccupation with formal organization. It has often been said that the great value of the United Nations has less to do with what goes on in formal sessions than with the fact that the UN headquarters is a unique conglomeration of diplomats from virtually every country in the world, who can meet informally in various constellations without publicity. Similarly, interagency coordination is often facilitated by the emergence of informal networks, whereas formal designations of a specific lead agency (one of Rochester's prescriptions) tend to aggravate organizational jealousies and turf battles.

By training his searchlights on the intergovernmental organizations (IGOs) of the UN system, Rochester leaves nongovernmental organizations (NGOs) in the shade. This also means that he underestimates the importance of IGO–NGO links in international cooperation. To produce action—to implement policies—NGOs are often needed. They are particularly significant when dealing with issues that span international and community levels and where reaching into societies is of utmost importance. (Population and AIDS are examples

in point.) Moreover, NGOs are not as easily paralyzed by claims to state sovereignty as are IGOs. Unfortunately, IGO-NGO links are mostly weak, and much remains to be done to overcome the gap of mutual distrust separating IGOs and NGOs in various issue areas. To establish arenas and channels for IGO-NGO coordination should therefore be part of the reform agenda.

In sum, Waiting for the Millennium is a useful antidote to the atheoretical, descriptive works on the United Nations whose ranks will no doubt swell further as the organization approaches its fiftieth anniversary. With its rare combination of theoretical anchoring and operative proposals, this book will be of interest to scholars, as well as practitioners, of international organization.

University of Lund

CHRISTER JÖNSSON

The Elusive Transformation: Science, Technology, and the Evolution of International Politics. By Eugene B. Skolnikoff. Princeton: Princeton University Press, 1993. 322p. \$39.50.

This book offers a very well written and thoughtful overview of the likely impacts of science and technology on international politics. Written as an extensive personal reflection by someone who has studied these issues for more than three decades, it bounces off of—and avoids—many of the more glib and startling predictions that have been made about such impact, concluding that science will probably not induce revolutionary changes in the international system of separate national sovereignties.

Skolnikoff surveys just about all the possible areas normally collected into "science policy," including broad issues of arms control and information management and, more generally, international economics. It includes discussions of demographic issues and of the possibilities of ecological damage, including, in particular, global warming. While not written as a text, the book would serve admirably for courses on science and public policy as an antidote to the exaggeration and hyperbole one often finds in this field. The author definitely sounds a warning that current political arrangements may be deficient in handling the complexities of rapidly evolving technologies; but at the same time, he stops short of concluding that such political institutions will respond by accepting major changes.

The book is very rich in citations of other literature and of relevant aggregate data. In its discussions of domestic economics and international economics, it wanders, at points, beyond what one might have regarded as science and technology policy more narrowly and precisely defined. The author endorses the general proposition that the pace of scientific development is much more rapid than in the past, thus confronting us with very different phenomena and problems. Yet the coverage is again a little too broad to fit entirely with this characterization. For example, Skolnikoff cites the AIDS epidemic as one problem that might, indeed, require more revolutionary responses but at the same times notes the analogy to the plague and other epidemics that hit the world a long time ago. Similarly cited is the possibility of an asteroid's striking the earth, which, again, may have happened with dramatic consequences in the past (though long ago).

The authors' sweep is thus broad enough to include somewhat more than what is new and different for the late twentieth century. Yet his general conclusion still seems very thoughtful and well grounded, namely, that there have been too many predictions by which what is indeed new and different about our science and technology will require—and will cause—very new and different political arrangements.

Skolnikoff presents a very issue-driven analysis of politics—as science and technology are the independent variables, and politics the dependent. A reader looking for extended discussions of the internal processes of government agencies handling these issues may thus be slightly disappointed. The book offers some wise observations about the roles and limitations of intergovernmental and nongovernmental organizations and of the future of collective security, set against the continuing debate between "liberal" and "realist" analyses of international relations. It is very sensible and clear-headed in its discussion of military issues and arms control.

The most salient conclusions that Skolnikoff offers are not so very startling, namely, that command economies are doomed in being the least able to handle the complexities of modern science and technology and that the flow of information across boundaries will make it increasingly difficult for *all* governments to constrict such information. Skolnikoff advocates, (but not with excessive vehemence) policies of "targeted research and development."

Yet the overall thrust of the book is, indeed, that the most startling conclusions are likely to be wrong here and that this work, indeed, catalogs a great number of such conclusions that seem to be more wrong than right.

University of Maryland

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George H. Quester

Coping with Complexity in the International System. Edited by Jack Snyder and Robert Jervis. Boulder: Westview, 1993. 366p. \$60.50.

In the 1960s, academic students of international relations frequently spoke and wrote about "the international system" and "systems theory." In the reader that virtually defined the field at the end of that decade, James Rosenau, declared that "of all the advances that have occurred in the study of international phenomena, perhaps none is more important than the ever-growing tendency to regard the world as the international system" (International Politics and Foreign Policy [1969], p. 71; original emphasis). Students of world politics became familiar with concepts such as system dominance, feedback, and equilibrium.

Although the language of systems became a conventional way of describing international politics, none of this work led to general explanatory theory. Indeed, during the 1970s, behavioral systems theory became unfashionable, overshadowed by Kenneth N. Waltz's powerfully argued reformulation of realist theory (*Theory of International Politics*, 1979). Discussions of general systems theory and cybernetic approaches to international politics were replaced on graduate reading lists with arguments for and against Waltz's neorealism.

For the last decade or so, increasing discontent has been expressed with neorealist theory. Elegant though it surely is, it seems to explain little of what puzzles us about world politics—both because strategic interaction among small numbers of actors has a high inherent degree of indeterminacy and because neorealism omits so much of what is significant in world politics (e.g., variation in information, institutions, patterns of economic interdependence, and domestic politics). That neorealism is "too parsimonious" and underspecified has become a constant refrain in the field.

Coping with Complexity adds the voices of 13 authors mostly associated with Columbia University-to that chorus. (Unfortunately, the volume contains no information about their affiliations or other accomplishments, except for the editors, who are well known.) Jack Snyder's introduction writes of "new thinking" about the international system, which sounds remarkably like the thinking of the 1960s: the world is complex, and many variables matter. Snyder does make an interesting point about complexity: Waltz's system is one of "disorganized complexity" with a small number of actors, but the complexity of world politics is actually largely "organized complexity," because states are "part of a hierarchical, organized system when facing their own societies" (p. 11). In the essay that provides the theme for the volume, Robert Jervis emphasizes the frequency with which action in world politics, as in other systems, has unintended consequences and provides numerous fascinating examples to support this undoubtedly correct proposition. Thomas Christensen, in the conclusion to the volume, uses some of the contributions in it to comment once again (as Eurocentric, historically oriented political scientists of the 1960s did) on the failure of such policies as those of the allies toward Germany after World War I. Except for the references to Waltz's theory, all of these ideas would have been familiar to students in international-politics seminars during the early 1960s.

Although this book hardly puts forward a theory, its contributions all purport to be informed by the systems perspective or what Paul Schroeder describes as "the general theme of this book, linear versus systemic thinking in international politics" (p. 48). The thread connecting the chapters is provided by Robert Jervis's essay, which focuses on the "perverse effects" of actions within a systemic context. Jervis does not provide a systematic statement of what he means by systems effects; but from his discussion and examples, it seems clear that he wishes to emphasize unintended and unwanted consequences resulting particularly from strategic adjustments. For instance, a state's purposive behavior may be thwarted by reallocation of others' resources or by the formation of new coalitions; that is, Jervis is concerned with the ways in which systems can produce countervailing actions. Unlike linear thinking, systemic thinking anticipates these countervailing actions and leads to more appropriate behavior.

Unfortunately for the coherence of this volume, its contributors have several different conceptions of what constitutes systemic thinking or, more broadly, systemic effects. A variety of interaction effects nestle beneath the systemic umbrella. Schroeder's fascinating essay on eighteenth- and nineteenth-century diplomacy does follow Jervis's theme; Kathleen McNamara's discussion of the European Community's Common Agricultural Program emphasizes unintended effects; and Randall Schweller's chapter on Hitler's strategy focuses on coalitional dynamics. But Jonathan Mercer's essay defends linear thinking against too great a preoccupation with reputation, and Susan Peterson focuses on bilateral

diplomacy between Britain and Russia before the Crimean War; her systemic effects refer to links between international and domestic politics, not countervailing strategic action. William McNeil focuses on the *cognitive* complexity of the war debt–reparations–loans–domestic politics system of the 1920s, which be believes Weimar's leaders failed to understand.

William Daugherty, Jeffry Frieden, and McNamara describe self-reinforcing systems that display the opposite property of that discussed by Jervis: reinforcing spirals of cooperation. (The Concert of Europe, the classical gold standard, and the Single European Act are the cases in point.) On the other hand, Richard Betts-in an updating of the critiques of collective security a generation ago by Inis L. Claude and Arnold Wolfersagainst easy expectations that maintenance of collective security under benign conditions will contribute to success when real threats develop. Anne Uchitel's chapter misinterprets Keohane and Nye's Power and Interdependence (1977) as claiming that interdependence enhances systemic stability and, in proceeding to argue that dependence can create instability, entirely ignores the theory of lateral pressure of Nazli Choucri and Robert C. North (Nations in Conflict, 1975).

Clearly all of the dynamics discussed in this book are systemic in some sense (they depend on interaction effects that would not be present were only two unitary actors involved), but to recognize this similarity is to suggest how uninformative a mere focus on systems is. If it is a platitude that world politics constitutes a system, we should hardly be surprised when a book resting on this recognition does not yield new insights. There are worthy contributions in this volume (the chapters by Schroeder, Mercer, Frieden, Betts, and McNamara are particularly interesting); but the editors' framework, with its systems approach, is not sufficiently precise to give the book much unity. As Jervis and other contributors admit, the systems approach is more an approach than a theory; it does not yield clear hypotheses, although decision makers' awareness of systems dynamics may "reduce the incidence of perverse effects somewhat" (p. 43). Coping with Complexity is more valuable as a reminder of what we have long known but are perhaps prone to forget than it is as an innovative contribution to theory and research.

Harvard University

ROBERT O. KEOHANE

The Elusive Balance: Power and Perceptions during the Cold War. By William Curti Wohlforth. Ithaca: Cornell University Press, 1993. 317p. \$39.50 cloth, \$16.95 paper.

Wohlforth argues that general international-relations theory has failed to progress because of ambiguities in the central concept of power. It is difficult, for example, to test the hypothesis that the system automatically returns to equilibrium when international-relations specialists make contradictory estimates of the distribution of power for the same time period. Even quantitative measures of power contain so many disparate elements such as military, technical, economic, population, and national resources that the analyst can achieve practically any desired result merely by selecting the correct indicators. Wohlforth comes heretically close to suggesting that there is no objective underlying distribution of

power—hence his title. Even a major war does not reveal states' relative capabilities, because wars are often ended by negotiation; and the relative influence of particular states on the postwar settlement may have more to do with expectations about the postwar hierarchy of prestige than about reality.

He reasons that if power influences international politics, it must do so mainly through the perceptions of people who make decisions on behalf of states. For each major historical era between 1945 and 1989, Wohlforth discusses Soviet views of the elements of power, the distribution of power, the mechanics of the balance of power, and the hierarchy of prestige. He also examines United States perceptions, though rather superficially.

The book provides a very useful and lucid synthesis of the many arcane yet consequential debates among Soviet ideologists and theorists over international relations. It should become a standard reference source for historians and political scientists, because it brings together the insights of many different Kremlinologists, shows where they disagree, and presents the most recent consensus of opinion.

Wohlforth is also more tangentially interested in evaluating various explanations of Soviet foreign policy learning. How could the Soviets pursue a set of foreign policy ideas that isolated them from the rest of the world and directed them to seek additional territory and influence, at the cost of provoking a hostile balancing coalition? He presents a fascinating account of the incubation of *perestroika*, making a good case that Soviet learning was not inevitable. New ideas about foreign relations were available from academics from 1980 to 1985, but among the Soviet political elites, only Mikhail Gorbachev was listening.

Experts on the former Soviet Union are engaged in a debate, going back to Barrington Moore and George F. Kennan, over whether Soviet foreign policy was primarily motivated by external influences, such as a hostile international environment, or by internal causes, such as factional maneuvering within the Kremlin or the need to build a coalition among various elite groups. Wohlforth presents a more sophisticated interactionist view, whereby foreign policy beliefs are a response to environmental challenges and opportunities but may also serve domestic political purposes. That particular ideas were useful to Stalin in his struggle with political opponents does not mean that the Soviet leader did not believe them. Wohlforth points out that the feedback from their policies was irregular or ambiguous or seemed to confirm erroneous Soviet beliefs, at least in the short term.

In flowing prose, Wohlforth presents many creative and insightful interpretations of Soviet rhetoric and the disjuncture between self-presentation and reality. Particularly interesting is his account of how Gorbachev coped with the decline of the Soviet empire and the collapse of its superpower status by trading in Soviet military chips to achieve arms control agreements, stressing global interdependence and the irrelevance of military power, and by claiming that world stability required a strong, democratic, united Soviet Union.

Wohlforth does not always give sufficient weight to the possibility that the Soviets used public estimates of U.S.-Soviet power relationships for strategic purposes. When the Soviets were vulnerable to a U.S. nuclear strike in the Stalin and Khrushchev eras, how likely is it that Soviet leaders would publicly admit their military inferiority to the United States, particularly when the Eisenhower administration maintained a declaratory policy of responding to communist aggression with "massive retaliation?"

He is too inclined to accept the realist view of the Cold War as a power struggle between the challenger Soviet Union and the dominant United States. There is little reliable evidence, for example, that Stalin was dissatisfied with the postwar settlement or felt that Soviet influence on diplomatic arrangements was incommensurate with their contribution to winning the war against Germany. Stalin achieved just about everything he wanted in Europe. Confronted with the fait accomplis of the Soviet Army, Winston Churchill and Harry Truman found it difficult to oppose Soviet territorial objectives, absorption of the Baltic states, and demands for "friendly governments."

Rather than seeking prestige commensurate with their military power, the Soviets often took apparently belligerent actions in response to such threatening Western actions as the establishment of a West German state, the creation of a peacetime military alliance, and the rearmament of Germany. By focusing solely on perceptions of power, rather than state interactions, Wohlforth overlooks the role played by mutual mistrust, the security dilemma, and conflict spirals in exacerbating Cold War tensions.

He reveals many anomalies in the balance of power that are either ignored or deliberately slighted by hardnosed realists. For example, states do not always balance against would-be challengers. The Soviets' experience in the 1950s and 1960s supported the correlation-of-forces model, which held that overwhelming military power would compel capitalist states to seek a détente. More controversially, Wohlforth concludes that there is frequently a disjuncture between leaders' perceptions of relative state power and material reality in the form of measurable resources.

Neorealists will not be convinced by his analysis, because many argue that the objective distribution of power constrains and channels the behavior of states regardless of the beliefs and perceptions of national leaders. For example, Stalin may have depreciated the importance of nuclear weapons, but he refrained from interfering with the U.S. airlift and discontinued the Berlin blockade despite his failure to block the establishment of a West German state. Similarly, for all his boasts about Soviet missile capabilities, Khrushchev did not carry out his ultimata in two Berlin crises. The debate over the extent to which power is an objective constraint or a manipulable image continues.

University of California, Los Angeles

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DEBORAH WELCH LARSON

Engaging the Enemy: Organization Theory and Soviet Military Innovation, 1955–1991. By Kimberly Marten Zisk. Princeton: Princeton University Press, 1993. 286p. \$39.50.

Kimberly Zisk examines the conditions under which military organizations are likely to change military doctrines. She argues that "the timing and content of military doctrine innovations are a result of the interaction of international and domestic political factors, of foreign doctrine changes and of domestic civilians' political strategies" (p. 5). The argument is complex and is summarized in its hypotheses: military organizations "develop innovative doctrines on their own, in the absence of civilian intervention," in response to foreign doctrinal shifts that threaten their war plans; before reacting to these foreign threats, however, military officers attend to "domestic threats to their organizational health and autonomy first"; military officers are likely to offer innovations that augment military resources and autonomy, yet these innovations provoke debate within the expert policy community and the military only slowly institutionalizes them; changes in the composition of the policy community can change the nature of the doctrinal innovations (pp. 26–28).

Zisk presents a well-crafted project that draws on a rich theoretical tradition in order to derive hypotheses and then test these in cases that permit controlled comparison. The book's three case studies examine Soviet responses to the United States' doctrinal innovations, including reactions to flexible response, the Schlesinger doctrine, and air-land battle (or follow-on forces attack). The richest and most rewarding of these studies is the last: Zisk demonstrates how public discussion of national security issues changed as civilian analysts from the Academy of Sciences' institutes joined the policy community. She also illustrates the ingenious strategies used by these analysts to develop expertise and build bridges with professional military officers, despite the resistance of some in the military.

Zisk's work is part of the effort to apply social science theory to the analysis of military doctrines—an effort that began anew in 1984 with publication of Barry Posen's Sources of Military Doctrine and Jack Snyder's The Ideology of the Offensive. Zisk's foil is the proposition, inspired by Graham Allison's work on bureaucratic politics, that military organizations are conservative bureaucracies limited by their standard operating procedures and likely to innovate only in the face of defeat on the battlefield or under pressure from civilians (p. 3). She contends that an improvement over the bureaucratic politics model for understanding the military role in doctrinal change is the "policy community" model, in which "policy ideas percolate fairly freely" among experts "who interact with each other regularly to exchange ideas" and have access to decision makers (pp. 21-22). Zisk concludes by arguing that her findings support a new theoretical approach that rests on three central propositions: (1) that the influence of institutional and organizational interests on the beliefs or behavior of individuals is complicated by idiosyncratic factors; (2) that both international and domestic politics constrain the behavior of individuals and organizations; and (3) that policy outcomes are determined not only by interest groups but also by policy communities (p. 184).

Zisk's argument might be strengthened if she could give more conceptual and operational precision to her dependent variable, military doctrine. Zisk seems torn between arguing that doctrine is formulated by the state on the basis of information provided by the General Staff and that "doctrine formulation was controlled by the General Staff" (pp. 34, 43). Although the latter view is highly doubtful in the Soviet case, it is necessary for the author's major thesis that the military will introduce innovative doctrines on its own without civilian intervention. In the empirical studies, the author does not always identify either the official act that adopted a new doctrine or the content of that doctrine, so that what

should be the most important empirical finding in each case study is often hidden in a larger discussion of a debate among military professionals. For example, the author asserts that in 1965 Soviet military doctrine "apparently shifted," that by the mid-1970s it "appears" to have shifted again, and that in the early 1980s there were statements "probably indicating that some sort of doctrinal decision had been made"; but the evidence of official doctrinal pronouncements is not presented. Instead, the author appears to let the debate among military professionals and *institutchiki* stand as evidence of innovation in official doctrine.

Zisk's argument might be further strengthened if she could substantiate empirically or specify theoretically the causal link between doctrinal developments in the United States and the innovations by Soviet military officers. She does not give us standards by which we can judge whether Soviet doctrinal developments were, in fact, responses to American ideas, rather than the autonomous development of autochthonous doctrines. Zisk offers circumstantial evidence that the Soviet debates took place around the time of an American doctrinal shift (although some Soviet debates actually began before the American shift) and that some Soviet military writers, whether part of these debates or not, had taken note of the American shift. The case for a causal link might be strengthened by hypotheses that predict the type of response we should have expected from the Soviets (e.g., symmetrical vs. asymmetrical responses, compensatory vs. exploitative responses). Drawing on the work of Roman Kolkowicz (1970) and James McConnell (1985), such hypotheses could provide theoretical reasons for believing that observed doctrinal developments in the Soviet Union were reactions to U.S. innovations.

Zisk's work might be strengthened by some further attention to the larger domestic political arena in which these military debates took place. Throughout this study, the important invisible hand (invisible from the perspective of the hypotheses) is the Communist party leadership that made doctrine based on the technical information and military science provided by the General Staff (p. 34). It was these leaders who imposed doctrinal innovations in 1960 and in the early 1980s and then "snatch[ed] the reins of doctrinal initiation away from the Soviet military" in 1987 (p. 163). It was the politicians who decided to empower civilian analysts in the late 1980s and thus expand the policy community (p. 23). The hypotheses of this study might ask why the Communist party leadership adopted innovative military doctrines. Indeed, this work raises the question whether in societies with traditions of strong civilian control over the military, one can study doctrinal innovation by limiting hypotheses to the behavior of professional military officers or even policy communities.

These opportunities to strengthen or build on this project should not detract from the significant contribution that it makes to an important debate. Zisk's work reflects the maturing of our field of Soviet and post-Soviet studies, as scholars not only ask how the theories of political science's mainstream might sharpen area studies but also how insights from our case might advance theory. Zisk's work should be taken seriously both by those who seek keener understanding of Soviet military debates in its last three decades and by those who seek to sharpen the organization theory of military behavior.

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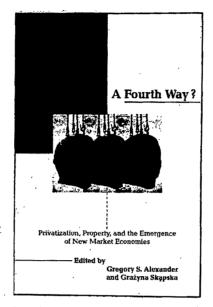
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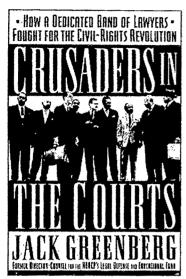
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ISSUES, CANDIDATE IMAGE, AND PRIMING: THE USE OF PRIVATE POLLS IN KENNEDY'S 1960 PRESIDENTIAL CAMPAIGN

LAWRENCE R. JACOBS University of Minnesota ROBERT Y. SHAPIRO Columbia University

Interpretations of electoral campaigns have pointed to two mutually exclusive strategies: candidates are expected to focus either on policy issues or on personal image. We argue, however, that social psychologists' notion of priming offers an empirically grounded and theoretically plausible campaign strategy for treating image and issues as interconnected strategic concerns. Based on both quantitative and historical analysis of John F. Kennedy's 1960 presidential campaign, we find that the candidate's policy positions were related to results from his private public opinion polls. Archival and interview evidence suggests that Kennedy deliberately used these popular issues to shape the electorate's standards for evaluating his personal attributes (rather than to win over utility-maximizing voters). We conclude that the study of priming offers one important approach to reintegrating research on candidate strategy and voter behavior.

nalysis of election campaigns typically assumes that rational candidates avoid taking divisive issue positions and instead craft ambiguous policy stances and attractive personal images of strength, boldness, competence, honesty, and trustworthiness (see Nie, Verba, and Petrocik 1979; Page 1978). Projecting favorable personal attributes and images is believed to offer a risk-averse strategy for holding together the party base and attracting wavering voters (Sniderman, Glaser, and Griffin 1990).

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This conventional wisdom developed in a reaction against the argument-often associated with the work of Anthony Downs (1957)—that officeholders should appeal to a self-interested electorate by concentrating on policy issues and, specifically, on minimizing the distance between their distinct issue positions and voters' policy preferences.1 Research by the personal attribute school has identified at least four reasons why rational candidates are expected to refrain from issue-based strategies: (1) the existence of multiple issue dimensions defies any office seeker's attempt to locate a position that will appeal to the median voter in all directions (but see Davis, Hinich, and Ordeshook 1970); (2) candidates are uncertain about their information concerning the electorate's preferences and their own ideal positions in relation to those preferences (Erikson and Romero 1990); (3) in this environment of uncertainty, candidates that announce definitive policy positions on conflictual issues risk alienating centrist voters; and (4) candidates are constrained in their position taking by ideologically oriented activists within the political parties (Kessel 1992; Page 1978).

However, empirical research, as well as recent economic and psychological theorizing suggest that candidates do not focus on developing a favorable personal image to the exclusion of position taking: policy stances and a candidate's personal image are not mutually exclusive bases for campaign strategy.

We draw on the social psychological process of priming as a way to understand the interconnectedness of image and issues in campaign strategy: this process suggests that candidates use popular policy issues to influence (or to "prime") the electorate's standards for evaluating their personal attributes. In particular, we focus on the case of John F. Kennedy's 1960 campaign and suggest that Louis Harris's innovative public opinion surveys heightened the Kennedy campaign's interest and skill in using position taking to shape the candidate's image. Harris's polls, which were privately conducted for Kennedy, offset a major deterrent of candidate position taking—limited information about voters' policy preferences. Our purpose is to use Kennedy's campaign to outline a candidate strategy that incorporates the public's policy preferences without ignoring the well established limitations on issue-based voting.

THE INTERCONNECTEDNESS OF ISSUES AND IMAGES

The study of campaigns can usefully draw on social psychologists' analysis of individual perception and, especially, on their distinction between the *availability* and the *accessibility* of public attitudes and information. This analysis holds that individuals have a set of enduring attitudes stored in their memories. Whether these attitudes are influential when a judgment must be made is determined by whether and how they are accessed or retrieved from memory (see Aldrich, Sullivan, and Borgida 1989; Iyengar 1990; Iyengar and Kinder 1987; Krosnick and Kinder 1990; Lavine et al. 1992). While some attitudes are frequently and consistently activated from memory, others are temporarily accessed because of immediate environmental stimuli.

Research has typically focused on the mass media

because it provides individuals with much of their information and therefore may be a primary stimulus for accessing attitudes. Indeed, studies of individuals have found that emphasis on a particular policy issue in the media may "prime" or temporarily increase the accessibility of these attitudes. For instance, as media coverage and discussion of health care reform increases the salience of the issue, the greater will be the probability that an individual's attitudes toward that issue (and perhaps related policies) will be activated and will become the standard by which he or she evaluates other attitude objects like candidates or related issues.

The process of priming, then, does not depend on altering an individual's policy preferences (e.g. converting supporters of health reform into opponents). Indeed, changing the public's policy preferences (especially during such short periods as an election campaign) is quite unlikely (see Campbell et al. 1960; Finkel 1993; Page and Shapiro 1992). Rather, priming involves a twofold process: (1) it sets the agenda by focusing public attention on certain topics, and (2) it provides the main basis for evaluation. Thus the messages communicated through and by the media during an election significantly influence which attitudes and information are likely to be retrieved or accessed from memory and incorporated into voters' judgments about, and ultimate choices of, candidates.

We propose to extend the application of priming from its original focus (the analysis of how individuals form attitudes and make decisions) to the study of candidate behavior. This new use changes the analytic focus from unintentional priming (i.e., the inadvertent impact of journalistic practices on voters) to intentional priming, namely, the deliberate strategies that candidates pursue to influence voters.³

The study of priming contributes to understanding candidate behavior by addressing three analytic obstacles to incorporating policy issues into campaign strategy: multiple issue dimensions, incomplete or imperfect information, and image-oriented campaign strategy.

First, research in the fields of psychology and political communication suggests that priming largely offsets the problem of multiple issue dimensions. Previous studies report that the media's attention to a specific issue activates existing attitudes on that policy or closely related attitudes on different policies; the effect is to crowd out other issues (Iyengar 1990; Iyengar and Kinder 1987). Ample studies of issue voting and campaign advertising confirm that focusing voters' attention on a few particular issues influences the importance that the electorate as a whole attaches to them (Aldrich and Alvarez 1992; Aldrich, Sullivan, and Borgida 1989; Berelson, Lazarsfeld, and McPhee 1954; Finkel 1993; Geer 1992; Nie, Verba, and Petrocik 1979; Petrocik 1991; Popkin 1991; West 1991).

A priming strategy is compatible, then, with a simpler and more realistic model of issue voting: when the electorate evaluates candidates, it neither possesses full information nor makes full use of the multidimensional space of issues. Rather, some issues are more important than others because priming prompts voters to gather readily available information on a few issues and to access a small subset of attitudes. The implication for campaign strategy is that candidates need not adopt issue positions that appeal in all directions: candidate appeals that activate existing attitudes on one policy issue suffocate or distract interest in other issues. In other words, campaign strategy involves the manageable task of influencing the weights voters attach to a few issues.

The second major obstacle to incorporating policy issues into campaign strategy is the risk associated with position taking in an environment of incomplete or imperfect information. One fundamental difference in candidate priming as opposed to media priming is that campaigns intentionally develop strategies to minimize risk and maximize benefits. The campaign's ability to prime voters to access stored information is constrained by several strategic (as well as real-world) considerations, including the risk that a candidate's position will generate new information and messages that alienate centrist voters.

Public opinion surveys, however, have bolstered candidates' confidence and sophistication in pursuing a priming strategy; the availability of measures of voters' concerns have mitigated candidate uncertainty over voters preferences and lowered the risk in basing a campaign on policy issues (Geer 1991). While candidates and presidents since Franklin Roosevelt have used scientific opinion surveys, John F. Kennedy's 1960 campaign introduced major innovations in terms of the number, structure, and political use of opinion polls (For the campaign's interest in simulation analysis, see Jacobs 1992b, 1993; Pool, Abelson, and Popkin 1964.)5 Interviews and archival evidence suggest that the Kennedy campaign commissioned and regularly used private, state-level opinion surveys conducted by Louis Harris. The polls included responses to an open-ended question regarding the most important problem facing the country. 6 Kennedy's private surveys of the public's policy goals were ideally suited to tracking the salience and accessibility of policy issues. Indeed, responses to the "most important problem" question are widely used in contemporary analysis of voters and priming's impact on them (e.g., Abramson, Aldrich, and Rohde 1991, chap. 6; Aldrich, Sullivan, and Borgida 1989).

Campaign records and interviews indicate that the Kennedy campaign sought to diminish its uncertainty regarding voters' preferences by adopting two innovations proposed by Harris: to poll in separate states and forgo national surveys and to avoid "separat[ing] our polling from the other problems of strategy." The political task of winning primaries and electoral college votes was closely related to polling that would, Harris explained to Kennedy, "tell you how . . . [a] critical state can be won." Harris's position within Kennedy's inner circle of advisers insured that polling information was incorporated into the campaign's strategy. In short, the Kennedy campaign committed scarce resources to

Harris's polls because they provided strategically oriented information on the policy issues that were salient and accessible to the average voter. The implication of Kennedy's campaign is that the priming concept is consistent with the information available to modern campaign organizations: extensive and sophisticated polling lowers a campaign's risk and enhances its skill in taking issue positions.

The third analytic payoff is that priming offers a theoretical framework for treating policy issues and candidate image as interconnected (rather than as conceptually distinct). When following a priming strategy, campaigns adopt a limited set of spatially distinct and popular issue positions. These policy stances are used by the campaign not as the exclusive basis of its strategy but as an instrument for emphasizing and thereby influencing the *standards* for evaluating a candidate's personal characteristics. A candidate's decision, then, to advocate a few popular issues adjusts the standards by which their leadership qualities will be perceived, evaluated, and judged. The strategic objective is to modify the public's evaluation of the candidate's image in order to widen the candidate's appeal.

A priming strategy does not require candidates to advocate positions the candidate actually opposes. Rather, it suggests that campaigns select a relatively few issues out of a large pool of acceptable possibilities for particular emphasis. In other words, candidate strategy involves strategic adjustments: highlighting a few policies by increasing the frequency, strength, and extensiveness of the candidate's statements.

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Research confirms that campaigns face compelling reasons to combine strategic position taking with image building. A singular focus on policy issues runs into the well-known limitations of a strict Downsian strategy. But an exclusive focus on image is likely to frustrate voters who are quite interested in salient issues; the electorate's perception of candidate image does not occur separate from, or at the expense of, its policy preferences. Empirical studies have found that voters' perception of candidate competence or trustworthiness is associated with their policy preferences (Kessel 1992; Page 1978, chap. 8; Petrocik 1991; Popkin 1991; Rahn et al. 1990; Sniderman et al. 1990). In addition, recent models of campaign strategy suggest that when office seekers announce their positions, they "signal" or increase the perception of favorable nonpolicy characteristics (Banks 1991; Cameron and Enelow 1992). Priming, then, suggests an empirically grounded and theoretically plausible campaign strategy for integrating the public's policy preferences with the well-established limitations on issue-based voting.

We use John Kennedy's 1960 campaign to examine candidate strategy and behavior in modern presidential elections. We ask, Does the direct study of Kennedy's strategy suggest that the campaign depended exclusively either on the spatial positioning of its policy stances or on the shaping of the candidate's image, or did Kennedy (as we suspect) attach

strategic advantage to using issues to activate or prime voters' policy attitudes?

If priming is a useful way to understand Kennedy's 1960 strategy, his campaign should exhibit behavior consistent with two theoretical expectations. First, to cut down on uncertainty concerning voters' positions on policy issues, the candidate should respond to reported polling data indicating which particular policy issues were highly salient and accessible. In other words, the specific policy areas and policy directions that Kennedy advocated should correspond with polling information regarding what most concerned Americans. Kennedy is expected to follow public opinion by making tactical adjustments both on which specific issues to support (out of a large pool of acceptable choices) and on the frequency and strength of his statements promoting a particular issue. This theoretical expectation contradicts the conventional wisdom that the importance of the public's policy preferences (as conceptualized in spatial terms) is severely mitigated by the multidimensionality of the issue space, the uncertainty surrounding the interpretation of public preferences, and the pressure of party activists.

Second, if Kennedy's position taking did respond to reported opinion, we would expect archival and interview evidence to indicate that his stances were not premised on winning over (in straightforward fashion) the Downsian utility-maximizing voter. Rather, the campaign's positions should have been developed to selectively prime voters in order to influence the electorate's standards for evaluating and judging competing office seekers. We would anticipate that the Kennedy campaign used salient, accessible issues in order to construct an appealing image of the candidate as competent and caring, an image that diverged quite noticeably (and favorably) from that of his opponent.

METHODS AND MEASURES

Our analysis of Kennedy's campaign behavior in 1960 is based on primary evidence drawn from archival records and interviews, as well as on a combination of interpretive and quantitative analysis. Typically, campaign analysis has relied either on inferences regarding statistical relationships between candidate behavior and public opinion or on formal theoretical models in order to speculate about expected strategies. However, questions concerning how and why real politicians use and are influenced by polling results have been neglected.

To incorporate the political motivations and calculations of individual office seekers, we analyze the Kennedy campaign's archival records, along with interviews with key participants. These sources provide intensive, in-depth evidence to explore, through a few specific policies such as health care reform, the extent to which polling results cause a campaign to advocate specific policies. Moreover, archival evidence and interviews provide the basis for investigat-

ing the focus of our second theoretical expectation, namely, the campaign's strategic intentions and calculations for using opinion polls.

We supplement this interpretive study of archival records and interviews with quantitative analysis of the relationship between public opinion and Kennedy's policy positions. In particular, quantitative analysis enables us to investigate further the extent of Kennedy's responsiveness to reported public opinion.

For the statistical analysis, we collected data on public opinion and Kennedy's policy positions. Our public opinion data are new and better than those used in previous studies of candidate behavior. Past research on the public's impact on candidate behavior has relied on polling results that were published and widely circulated (e.g., Page 1978; Young 1991). The critical assumption has been that the published survey results found their way into the campaign's decision-making process (or at least reflected comparable information that was used by the campaign).

The question whether relying on published surveys is fully appropriate has been generally downplayed, given the common problem of identifying available opinion data. Clearly, however, polling results assembled and actually used by a candidate are precisely what is required for analyzing how far candidate decisions were influenced by public opinion. As far as we know, ours is the first to study systematically campaign data assembled by and for a major presidential candidate. In particular, Harris was an "integral part" of the campaign organization and regularly reported his polling results to Kennedy through both written accounts and frequent meetings with the candidate. ¹⁰

Specifically, we use the policy goals that the public cited as important in Louis Harris's surveys for the Kennedy campaign; both campaign documents and our interview with Harris indicate that this body of data had "great importance" in the campaign. Harris's polls were based on personal interviews with large representative samples in separate states. Many of the states were polled on multiple occasions. According to records in the Kennedy Presidential Library, Harris conducted 66 polls during the primary and general election campaign, 26 of them between September and November 1960. Our analysis of Kennedy's campaign against Nixon focuses on the 26 opinion surveys conducted during the general election. Rather than being evenly distributed, these polls were bunched into six weeks: they were completed during the first half of September, the second week of October, and the contest's final weeks.

We studied Kennedy's most consistent source of data on public opinion toward policy issues, namely, Harris's "most important problem" question. These data have two important characteristics: (1) they measures the salience of an issue, telling us how a particular policy area (e.g., Social Security) was ranked compared to other issues; (2) because Harris structured his surveys to ask respondents, "What do you think can be done about [a national problem]," his polls also tracked the public's preferred policy

direction (e.g., increasing Social Security payments). 11 (Racial integration offers another illustration of how issues were defined by Harris: he reported "prointegration" as one issue and "anti-integration" as a separate issue.) The unique feature of Harris's "most important problem" question, as compared to the typical salience measures, is that it reports directionality. Harris's data include respondents' judgments about direction and salience. These polling results are well suited for our study of candidate priming precisely because they identify the specific issues and policy directions that were salient and accessible to voters.

Using Harris's polling reports to Kennedy, we constructed weekly opinion measures by averaging each state's poll results for weekly intervals. For instance, for the five separate state surveys that Harris conducted during the week of 23 October, we averaged the percentage of respondents who ranked increasing Social Security as an important problem.¹²

Data on Kennedy's position taking were obtained by content-analyzing two sources of Kennedy's publicly stated positions. One source of Kennedy's statements was the four debates with Vice President Nixon.¹³ We treated each debate as a discrete event (i.e., we analyzed four distinct sets of policy statements). The debates seem to warrant this separate attention because of the importance that Kennedy (and history) attached to these first televised exchanges between presidential candidates. Moreover, polling results were, according to Harris and others, an important consideration in briefing Kennedy and plotting the campaign's strategy toward the debates. 14 The debates, however, are not a direct measure of Kennedy's strategic position taking: because journalists asked the questions and therefore controlled the agenda, the debates are at best an indirect, mediated avenue for activating voters' attitude struc-

A better and more direct indicator of Kennedy's statements is contained in a comprehensive collection of the candidates' day-to-day speeches and other statements, which was compiled by the U.S. Senate. ¹⁵ The Senate studies offer a record of the public statements of both candidates during the entire general election campaign.

Kennedy's policy stances in the debates and in his other public statements during the entire campaign were coded on a five-point scale from explicitly supportive (+2) to explicitly opposed (-2), with the midpoint (0) representing a neutral or unclear position (+1 and -1 indicate probable support and opposition, respectively). We then summed these results to produce weekly measures for the content of Kennedy's statements on an issue. 17

Our data, then, consist of 55 separate policy issues that were cited in Harris's poll reports and in Kennedy's public policy statements during the entire campaign, each issue being defined in a directional sense (e.g., increasing Social Security).¹⁸ Because we collected data on 55 separate issues, we are able to conduct separate analyses of specific issues and pol-

icy directions. Our analysis is not confined to studying such broad policy categories as "social welfare" or foreign policy.

For each of the 55 issues, we created weekly measures of Kennedy's reported opinion poll results and his policy statements (as recorded by the Senate study) for each of the 10 weeks between 1 September and 8 November. The number of cases, then, is the sum total of both policy stances on, and polling data for, each of the 55 issues during the 10 weeks of the campaign, the maximum number being 550. Because Harris's opinion surveys were clustered during six weeks, however, the effective number of "issueweek" cases falls below this total. Technically, then, we have 55 (abbreviated) pooled time series, making it possible to compare many issues and time periods.

We created the weekly measures in order to reflect the campaign's operations and use of Harris's surveys. When it came to incorporating polling results, the campaign did not view each day as a discrete unit. Instead, Harris's surveys were conducted in clusters and the campaign drew on these blocks of results until the next batch was forwarded. Weekly aggregation of the opinion and policy data approximates this process of bundling together intermittent polling results in order to evaluate campaign strategy.

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Our concern to capture the campaign's operations also led us to use time lags in analyzing the debates and the candidate's public statements (as compiled by the Senate). We dated Harris's results according to the completion of the surveys. Obviously, it took significant time both for Harris to enter and analyze the survey results and for the campaign to consider altering the candidate's speeches and other statements. According to campaign documents, Harris's data on the public's policy goals took on average two weeks to reach Kennedy for much of the campaign. For instance, we related Harris's results during the campaign's first week to Kennedy's public statements during the third week of September. For the campaign's closing week, however, the evidence suggests that a one-week lag is more appropriate: Harris shortened the report time as the urgency and demand for results increased. 19 (Indeed, more than a quarter of Harris's polls were completed with less than two weeks remaining in the campaign; these results were obviously used in much less than 14 days, and the last polls within a week.) The substantive reasons for using two-week lags during the campaign's first six weeks and one-week lags during its final four weeks is supported by our results; we examined a variety of lag structures and the proposed lag structure produced the clearest and strongest pattern of results.20

ISSUES, CANDIDATE IMAGE, AND CAMPAIGN STRATEGY

Interviews and archival records suggest that the Kennedy campaign analyzed the candidate's policy

positions and the public's perception of his personal image as interconnected (rather than mutually exclusive) strategic concerns. Indeed, soon after the 1956 election, Kennedy was wondering, "What am I going to be for?" By the time the campaign gathered steam in 1959, his clear identification with a few policy positions (that were acceptable to the candidate) was seen as a way to adjust "the so-called Kennedy image."21 Throughout the primary and general election campaigns, Kennedy's aides carefully tracked their candidate's image and attempted to pinpoint his perceived personal characteristics that were considered unfavorable. (Harris devoted a separate section of his surveys to tracking and analyzing the public's image of Kennedy's personality and his job performance.) Thus, when aides identified public perceptions of Kennedy as "too slick" and "lack[ing] depth," they repeatedly recommended that the campaign develop a "Kennedy-identified program" that would appeal to two distinct groups of voters: ideologically oriented party activists and more moderate centrist voters.

In response to criticism from the left wing of the Democratic party, an aide urged Kennedy in 1959 to address in "clear and unmistakable terms" the "prevailing doubt... that Kennedy is a bonafide liberal." The campaign, he proposed, needed to develop an image of Kennedy that would "enable people to be for something [and someone] that excites their imagination." Similar anxieties about Kennedy's image emerged immediately following the presidential conventions, with aides agreeing that the campaign had reached its "low point." In response, Schlesinger suggested that "the present lethargy of the campaign" be reversed by "giv[ing] the enthusiasts something to believe in": "Once the issue-minded Democrats catch fire, then the campaign will gather steam." Kennedy's issue positions, then, were understood by campaign officials as a mechanism, at least in part, to charge the party faithful with "crusading urgency."

But campaign officials sought to identify a "package" of issues that both appealed to party activists and responded to the concerns of the centrist voter. They expected that party activists would support (rather than resist) popular positions. Thus campaign officials decided to fashion a "move-ahead" image for Kennedy that would demonstrate a large difference between the Republican and Democratic candidates. The objective, in particular, was to identify Kennedy with new and dynamic approaches to the country's problems, while tying Nixon to eight years of Republican "do-nothingism." Campaign officials calculated that emphasizing Kennedy's proposals for addressing several highly salient issues, which were accessible to the average voter, would serve as a "campaign tool" for creating an appealing image. Thus, in the final phase of the campaign, Harris pressed Kennedy to "go on the offensive" to press his "basic theme on all the issues: to get America on the move at home and abroad."²⁴

While an overarching, "move-ahead" image for

Kennedy could reasonably be expected to appeal to reform-oriented party activists, it was the perceptions of the centrist (or median) voter that primarily concerned the campaign. Two criteria, then, were especially critical in the campaign's decisions: (1) identifying issues that were highly salient and accessible to the public and (2) using the activation of voters' attitude structures regarding these issues to demonstrate large differences between the candidates. The first criteria involved responding to public opinion, the second, directing or manipulating voters' evaluations of Kennedy's personal character.

CAMPAIGN RESPONSIVENESS TO PUBLIC OPINION

Our quantitative analysis confirms that Kennedy's positions responded to public opinion: the issues that were raised in Louis Harris's polls were persistently mentioned by Kennedy in his subsequent public statements. What is significant is not just that Kennedy cited issues identified by the public but that the frequency and strength of his stance was congruent or consistent with the public's preferred direction for policy.

The following types of issues, which were salient to respondents in Harris's surveys, were also publicly advocated by Kennedy in the debates and in his public statements (as compiled by the Senate): increasing Social Security, passing Medicare legislation, reforming education, fighting unemployment, combating the high cost of living, and such foreign policy issues as bolstering America's military spending and

general prestige.

The relationship between the policy goals that Harris's respondents consistently identified and the candidate's (subsequent and congruent) positions was most clearly evident in Kennedy's public statements. The last row of Table 1 reports overall results. When we compared Harris's weekly polling results (allowing for the appropriate one- or two-week lag) with Kennedy's subsequent positions, we found that Kennedy took congruent positions on 56% of the issues cited in the polls, compared with 27% of the issues not cited in the polls—a 29% difference.

The regression results in Table 2 confirm these findings. The last line of the table indicates that the average correlation (r) between the percentage of public support for a policy goal each week and the position Kennedy staked out was .38. The slope coefficient (b) of .42 for the regression of candidate position on public opinion indicates that for each 2% or so of the public citing a particular policy during a given week, there was one additional congruent policy statement (coded 1 in our content analysis) during the subsequent weekly interval.

These overall findings, however, mask critical variations over the course of the campaign. The strongest relationships are especially evident during the first week in October and the closing weeks of the contest.

TABLE 1

Relationship between John Kennedy's Poll Results and Subsequent Public Statements during the 1960 Election (%)

> ISSUES ON WHICH KENNEDY TOOK POLICY STANCES DURING **GIVEN WEEK:**

WEEK OF CAMPAIGN	NOT CITED IN POLLS %(N)	CITED IN POLLS %(N)	DIF- FERENCE	
Opening ^a	A second			
September				
Week 3	26 (31)	54 (24)	28*	
Week 4	13 (31)	29 (24)	16	
October		, ,		
Week 5	41 (37)	78 (18)	37*	
Home stretch ^b	(,	()		
Week 7	22 (32)	52 (23)	30*	
Week 9	35 (31)	62 (24)	27*	
November	(,	(//		
Week 10	27 (33)	64 (22)	36*	
Average	27	56	29*	

Note: On issues cited in Kennedy's polls, we report the percentages of policy stances consistent with public opinion. Numbers of issues are in parentheses. No analysis is shown for weeks 1, 2, 6, and 8 because there were no polls in the appropriate preceding period. "Two-week lag between polls and statements.

For the first week of October, the percent difference (37%) jumped more than 20 points from the previous week's level (16%) and was 8 points higher than the

TABLE 2

Effect of Kennedy's Poll Results on the Content of His Public Statements during the 1960 Election

	_	
WEEK OF CAMPAIGN	BIVARIATE REGRESSION (b)	CORRELATION COEFFICIENTS (r)
Opening ^a September		
Week 3	.37*	(.33*)
Week 4	.08	(.15)
October		` '
Week 5	.40*	(.30*)
Home stretch ^b		
Week 7	.55*	(.54*)
Week 9	.78*	(.54*)
November		
Week 10	.32*	(.41*)
Average	.42*	(.38*)

Note: The independent variable is the average percentage of respondents in Kennedy's polls ranking an issue and policy direction as important. The dependent variable is the sum of the content scores for Kennedy's statements about the issues, where each statement was coded on a five-point scale (-2 to +2) depending on the degree of support for the policy. N = 55. No analysis is shown for weeks 1, 2, 6, and 8 because there were no polls in the appropriate preceding period.

One-week lag between polls and statements.

^{*} $p \le .05$.

Two-week lag between polls and statements. bOne-week lag between polls and statements.

^{*}p ≤ .05.

average (29%). Fully 78% of the issue positions cited in Harris's surveys two weeks earlier were echoed by Kennedy during the campaign's fifth week, compared to 41% of the issues that did not appear in the polling reports. Similarly, the regression coefficient rose to .40 (r = .30) from previous week's figure of .08 (r = .15).

The most striking results occurred during the closing weeks of the campaign. Table 1 suggests that week 10 produced a 36-point percentage difference, 9 points higher than the previous week. Table 2 confirms this overall picture of heightened responsiveness during the campaign's home stretch. In particular, the b coefficient for the campaign's ninth week was .78, nearly twice the size of the average figure (.42). It was also larger than the figure (.55) for the previous week with available data—week 7. For each 5% of the public citing a particular policy during the campaign's eighth week, there were two additional directly congruent policy statements (coded 2 in our content analysis) during the following week. Kennedy's elevated responsiveness is illustrated by his handling of unemployment during campaign's home stretch: a threefold increase in Harris's polling results during the second week in October (from 4% to 13%) was associated with a fourfold increase in Kennedy's statements the following week (from a policy statement score of 6 to a score of 26).

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Tables 1 and 2 present an interesting contrast for week 9: the percentage difference is average, whereas the b coefficient is the largest of any week. Although Kennedy was not more apt than on average to raise an issue that the public cited, the results that Harris reported to Kennedy apparently had an exceptional impact on the number and strength of his statements: Kennedy was acutely responsive to the issues that were highly salient and accessible.

Kennedy's heightened responsiveness during the campaign's final weeks is consistent with the strategic importance of this period for priming voters. The attitudes structures that have been most recently activated from the electorate's short-term memory will be especially decisive in voters' choices. Indeed, it was during the campaign's final push that Harris pressed Kennedy to "go on the offensive" by advocating salient issues.²⁵

The statistical results for Kennedy's positions during the two debates for which there were preceding opinion surveys are also revealing. The last row of Table 3 shows that in the debates Kennedy was, on average, 41% more likely to promote issues that were cited in previous polls than to raise issues that lacked public attention and support: he made congruent statements on 49% of the positions cited in Harris's reports compared to 8% for issues not cited in the polls. The last row of Table 4 presents the average regression results: the b coefficient is .18, and the correlation coefficient is .47. Overall, then, an issue's citation in Harris's reports made it quite likely that Kennedy would raise the issue, but the impact of reported opinion on the strength and frequency of

TABLE 3

Relationship between John Kennedy's Statements during the Debates and His Most Recent Poll Results (%)

DEBATE (SUBJECTS)	NOT CITED IN POLLS %(N)	CITED IN POLLS %(N)	DIF- FERENCE
Opening ^a 26 September (domestic policy) Home stretch ^b 13 October	11 (37)	56 (18)	45*
(subject open) Average	6 (32) 8	43 (23) 49	37* 41*

Note: On issues cited in Kennedy's polls, we report the percentages of policy stances consistent with public opinion. Numbers of issues are in parentheses. No analysis is shown for October 7 and October 21 because there were no polls in the appropriate preceding period.

Kennedy's statements, as estimated by the regression b coefficient, was not strong.

Kennedy's was most responsive during the first debate, which occurred on 26 September. Table 4 indicates that the b coefficient for the first debate was more than twice that for the other debate (.26 vs. .11). Moreover, the 45% difference in Table 3 is the highest figure to emerge in our analysis: Kennedy made congruent statements during the debates on 56% of the issues cited in Harris's reports, compared to 11% on issues not cited in the polls.

The strong findings for the first debate may reflect a general *trend* toward responsiveness during this period. The results for Kennedy's public statements,

TABLE 4

Effect of Kennedy's Poll Results on the Content of His Statements during the Debates

DEBATE (SUBJECT)	BIVARIATE REGRESSION (b)	CORRELATION COEFFICIENTS (r)
Opening ^a 26 September (domestic policy) Home stretch ^b	.26*	(.48*)
13 October (subject open) Average	.11* .18*	(.46*) (.47*)

Note: The independent variable is the average percentage of respondents in Kennedy's polls ranking an issue and policy direction as important. The dependent variable is the sum of the content scores for Kennedy's statements about the issues, where each statement was coded on a five-point scale (-2 to +2) depending on the degree of support for the policy. N = 55. No analysis is shown for October 7 and October 21 because there were no polls in the appropriate preceding period.

[&]quot;Two-week lag between polls and statements.

bOne-week lag between polls and statements.

 $p \leq .0$

^aTwo-week lag between polls and statements. ^bOne-week lag between polls and statements.

 $p \leq .05$.

which are presented in Tables 1 and 2, also found that public opinion became more influential during and after the debate (compare weeks 4 and 5 in Tables 1 and 2). However, the statistical results for the debates should not be overstated: analysis of the first debate is limited to a relatively few statements during a short period—one evening. In addition, reported opinion had only moderate impact at best on the frequency or strength of Kennedy's statements. The media's role may explain why reported opinion had a more limited impact on the debates than on Kennedy's public statements: because journalists asked the questions, they (not Kennedy) controlled the agenda and the set of issues that the candidates addressed. Kennedy's reduced responsiveness during the debates may reflect the media's intervention.

The general pattern of our findings suggests a striking and consistent relationship between Harris's surveys identifying a policy issue as important and Kennedy taking a subsequent and corresponding position on it in his public statements and in the debates.

Kennedy, however, did take positions on such issues as civil rights and foreign aid that the public either failed to fully support or openly opposed. On the issue of civil rights, Harris reported responses to the following three related issues: antiintegration, prointegration, and states' rights. These three issues were identified in nearly each of the 26 surveys conducted.

Public opinion toward civil rights clearly divided on regional grounds. Respondents in critical north-eastern states like New York and Pennsylvania consistently ranked prointegration among their top 10 concerns; segregationist positions (opposing integration and favoring states' rights) were not identified as major concerns. States in the Democrats' traditional stronghold of the South held nearly opposite preferences: states like South and North Carolina identified antiintegration as their top policy concern.

Kennedy cautiously reacted to these regional divisions in public opinion (and in the Democratic party). On the one hand, he spoke in favor of expanded civil rights, a position that went against the antiintegration attitudes in the South. On the other hand, he had sufficient information about southern opinion to appreciate the benefit of crafting an ambiguous position and minimizing the issue's exposure during the campaign. Thus, when the issue was raised, Kennedy emphasized his narrow, legalistic concerns and endorsed civil rights based on constitutional (rather than moral) grounds. Kennedy did his best, though, to downplay the issue and its salience: over the course of three months of continuous daily statements, he only made statements on the issue a couple dozen times. One of the only wrinkles in the campaign's avoidance of the potent issue of race was caused by Kennedy's call to Coretta Scott King on the eve of the election. Robert Kennedy's initial reaction was that contacting King had "lost" or "screwed the whole election up." He feared that the call had attracted attention to the issue of civil rights as one of the standards on which the candidates would be judged (according to Sargent Shriver, quoted in Jackson and Riddlesperger 1991, 5). In other words, the fear was that the candidate's action had activated voters' memories about a set of issues that could only hurt their evaluation of John Kennedy: his understated position would satisfy neither southerners nor integrationists.

Too much foreign aid was another position with which Kennedy did not publicly agree or follow public opinion: this position was persistently cited by a relatively high proportion of Harris's survey respondents, but Kennedy repeatedly staked out an opposing position in the debates and in his public statements. As Harris emphasized in our interview with him, foreign aid was an area about which Kennedy deliberately "took a stand in spite of public opinion" in order to assure other elites of his worthiness to be president. Instead of deferring to public opinion, he devoted an exceptional amount of time (especially during the October debates) attempting to educate the American public about a foreign policy that advanced the country's national interests.²⁶

Issues like foreign aid and civil rights, however, are clearly exceptions. In general, Kennedy was much more likely to take stances on issues that Harris identified as salient and popular than on policies that lacked this kind of support.

Archival records of Kennedy's primary and general election campaigns corroborate the statistical evidence that the campaign consistently selected issue areas and particular policy directions that were salient and accessible to Americans. The issue of passing Medicare legislation, which would help finance the elderly's health care, illustrates this sensitivity to public opinion (Jacobs 1993, 1992a).

The tireless reformer, Wilbur Cohen, deliberately capitalized on the campaign's concern with public opinion when he pressed Kennedy to promote Medicare: Cohen assembled recent polls in order to be able to tell "Kennedy and his people: this proves . . . we've got public opinion." Moreover, Harris also stressed that he found strong polling results favoring Kennedy's medical care position: he repeatedly identified it as among "the most dominant overall-all problem[s] of concern to voters and the sharpest cutting edge[s]" between the candidates. In his polling reports and verbal advise to Kennedy, he concluded that "there is no situation, no local or national speech, in which the medical care issue should be ignored." 28

Kennedy's recognition of this "public interest and apparent support" prompted him to select Medicare for particular emphasis. As one campaign official recalled, Kennedy believed that it was, "of all the issues on which he campaigned, . . . the one that constantly provoked the most interest." Kennedy's perception of Medicare's salience (one shared by other campaign officials) prompted him to select it from a "roster" of issues that he already supported for particularly frequent and strong statements.²⁹ Indeed, a careful look at Harris's findings and Ken-

nedy's statements suggests a close relationship. For instance, a more than twofold increase in public support for Medicare during mid-September (from 9% to 22%) coincided—two weeks later—with both a sevenfold increase in Kennedy's daily attention to the issue (from a policy statement score of 2 to a score of 14) and a decision to select Medicare for exceptionally frequent, strong, and extensive statements during the first debate. In short, Kennedy's polling information prompted him to make tactical adjustments, modifying the extensiveness and forcefulness of his positions.

USING SALIENT POSITIONS TO BUILD CANDIDATE IMAGE

Evidence of Kennedy's responsiveness—that he consistently took positions on specific policy areas and options that were salient and accessible to Americans—suggests that information about public opinion from his private polls was strategically attractive to the candidate because it diminished his uncertainty regarding voter preferences (Geer 1991). The responsiveness to the public's concerns about policy issues contradicts the conventional assumption in campaign analysis that rational candidates avoid position taking. What remains unanswered, however, is how Kennedy's information regarding the public's policy concerns was understood and used in strategic terms by the campaign. Conceivably, Kennedy's responsiveness could have been motivated by Downsiantype calculations: he may have positioned his issue stands in close proximity to the public's policy concerns, expecting voters to make a prospective decision that his election would secure desired benefits. Alternatively, the campaign's calculations may have implicitly reflected a strategy of priming. That is, the strategic objective of adopting positions on popular issues may be to emphasize standards for evaluating a candidate's personal characteristics, which encourages voters to perceive large-and advantageousdifferences in the candidates' images.

The Kennedy campaign viewed position taking on salient, accessible issues as the means by which to project the candidate's "move-ahead" image. 30 It was "through the issues" and by "hammer[ing] home" his positions on medical care, education, and other policies that the campaign attempted starkly to contrast Kennedy's image (as "caring" and willing to offer new approaches) with Nixon's identification with the Republican's "drift and inaction." "The most fundamental job," Harris advised, was to "increase Kennedy's identification . . . with the problems important to voters" and thereby to demonstrate that Kennedy, unlike Nixon, "really cares" about addressing the problems facing the country and individual voters. Thus, although medical care was expected to have special meaning for liberals and the aged, the medical care issue was also seen as a

way to shape Kennedy's image to "many voters other than the old people themselves." ³³

The Kennedy campaign's decision to use popular policy issues as part of a priming strategy raised two strategic concerns. First, the campaign struggled to identify Kennedy visibly with these popular issues. It used a range of strategies (including Senate activities³⁴ and a "TV spot campaign")³⁵ to cement Kennedy's identification with popular issues. For example, in preparation for the debates, Kennedy's advisers analyzed the most effective issues, with Harris identifying "leading domestic issues as they emerge from our surveys" (medical care and education topped his list). As part of this effort to make sure that Kennedy impressed voters with his concern about salient issues, the campaign monitored whether and with what frequency Kennedy discussed these issues during the debates.³⁶

Second, as the general election campaign developed, Kennedy's advisers endeavored to protect Kennedy's identification with popular issues from Nixon's efforts to (as Clark Clifford put it) "fuzz up" the differences.³⁷ For example, Nixon's concern with the Democrats' use of the popular Medicare issue led him to reverse the Eisenhower administration's previous opposition to new legislation and to "resort to me-tooism." Nixon stressed that despite the differences in the approaches favored by the candidates, he shared Kennedy's aim of helping the aged finance their health care (David 1985; Jacobs 1993, chap. 5; Page 1978, 73–74).

In other words, Nixon was attempting to counter Kennedy's strategy of priming salient issues in order to portray large differences in each candidate's character. The Republicans intended to neutralize the impression that Medicare illustrated Nixon's listless character as opposed to Kennedy's forward-looking approach. Nixon's actions are consistent with theoretical models about rational candidate behavior: he used information following Kennedy's announcement to better estimate the true position of voters and decided to modify his position accordingly.³⁸

Alarmed at Nixon's attempt to create the "illusion" of similarity between the candidates' positions, Kennedy's aides repeatedly urged their candidate to emphasize the differences on the issues. ³⁹ If popular issues were to be an effective "tool" in shaping public evaluations, the campaign felt compelled to protect and reinforce Kennedy's identification with those issues. Thus, as election day approached, Harris frequently warned Kennedy to resist Nixon's "fuzzup" effort and his own campaign's tendency to "move over to new issues" by "dramatizing" and "pressing home" the candidates' differences. ⁴⁰

DISCUSSION

We modified social psychologists' study of priming by shifting its focus from individuals to candidate behavior and by accounting for the tactical considerations and intentional strategies of modern campaigns. Partly because position taking risks alienating centrist voters, candidate priming is expected to respond to salient and accessible issues: campaigns emphasize these issues in order to influence voters' evaluations of their candidate's personal image and to demonstrate large appealing differences from their competitors.

Our analysis of Kennedy's strategy in the 1960 race confirms two critical aspects of a candidate priming strategy. First, we found that Kennedy responded to private polling information that identified specific policy areas and policy directions that were highly salient and accessible to Americans. Statistical evidence concerning the impact of reported opinion was reinforced by interviews and archival evidence. In its efforts to fashion a winnable election strategy, the Kennedy campaign used its polling results to select and emphasize a limited number of policy issues from a large pool of possible choices.

Some of Kennedy's positions (e.g., his support for health insurance for the aged) were enduring "party cleavage" issues, over which Democrats had been battling Republicans for decades. It could naturally be anticipated that the relationship between Kennedy's positions and Harris's polling results was spurious: it is possible that the candidate's positions were driven not by polls but by longstanding divisions between the political parties and their supporters. In other words, party activists (rather than the electorate) may have been the driving force.

Although partisan cleavages obviously played a role in the 1960 elections, the archival and statistical evidence strongly suggests a causal connection between Kennedy's polls and the positions he took. While partisan division might explain why various issues would be included in the pool of conceivable election issues, the evidence indicates that Kennedy's campaign was quite selective regarding which policies it chose from this pool of party cleavage issues. Moreover, some of Kennedy's positions (such as his foreign policy stance on the new issue of rebuilding American prestige) were not issues that had persistently divided the parties.

Kennedy's concern with responsiveness to public opinion was also reflected in his decision to commit scarce financial and organizational resources to polling and to incorporating these assessments of public opinion into the campaign's decisions. Indeed, the campaign's organizational and strategic interest in assembling private poll data was a watershed event in the evolution of modern campaigning. It is well known that past politicians conducted polls; but the number, quality, and strategic use of Harris's surveys represent significant innovations. Indeed, Kennedy and his aides believed that their recognition of the "importance and value [of polling results] on the American scene" and on the designing of campaign strategy were historic firsts. 42 The campaign's pioneering use of polling to improve its information about voters' concerns had an effect beyond the 1960 race: the campaign prompted Kennedy to inaugurate

private polling as an enduring, institutional commitment of the White House (Jacobs 1992b; 1993). 43

Our second finding is that Kennedy's responsiveness coexisted with his attempt to manipulate voters' evaluations of his personal character. Kennedy's responsiveness to the issues that the public ranked as important was driven neither by an exclusively image-based strategy nor by a straightforward Downsian calculation that the proximity of his issue stances to voter's preferences would win over the electorate. (Of course, Kennedy's campaign did not use or consciously test these competing theories.) Instead, the campaign viewed Kennedy's advocacy of a few popular policy issues as a political resource for highlighting the candidate's bold, competent proposals for "moving ahead." By emphasizing a few salient, accessible issues in voters' memories, the campaign intended to shape the electorate's standards for evaluating and judging competing office seekers. The objective was to use priming to construct an image that would be appealing and noticeably divergent from Nixon's.

Although comparable research on other presidential elections has not been conducted, Kennedy's use of salient issues to shape candidate image may provide insights into subsequent campaigns—even those in which candidate behavior apparently followed Downsian expectations. For instance, the 1964 and 1972 presidential contests appear to be classic illustrations of candidates offering divergent policy stands. Goldwater and McGovern presumably lost because their opponent's centrist positions promised greater benefits to the median voter than did their own extreme proposals. Nevertheless, further analysis of each candidate's issue stands may well find the voters' decisions in 1964 and 1972 resulted from the victorious candidate's use of a priming strategy. By focusing the electorate's attention on Goldwater's and McGovern's initial, isolated, and often retracted "extreme" issue stances, the eventual victor in each race was able to mobilize (with the media's assistance) widely shared beliefs that cast their opponents in a bad light (Kelley 1983; Page 1978; Popkin 1991).44

CONCLUSION

Our findings have two broad implications for the future study of elections. A fuller assessment of these implications than is possible here, however, requires that our empirical analysis of one campaign be supplemented with research on additional campaigns and on voting behavior. First, the process of priming may provide an analytic basis for recoupling research in the two subfields of voters' motivation and campaign strategy. For decades, the two interrelated components of electoral analysis—research on voters and on candidates—were held together by a shared emphasis on the influence of candidates' personal image and on the limited role played by policy issues. The limits on Downsian expectations regarding issue-oriented campaigns were compounded by a number

of stable obstacles to issue-guided vote choice: voters' incomplete or inaccurate information, their tendency to make retrospective judgments of government performance, and their party loyalties (Campbell et al. 1960; Fiorina 1981; Keith et al. 1992; Smith 1989).

The conventional wisdom, however, that rational campaigns adopt nonissue strategies that emphasize favorable personal images has been undermined by the recent revisionist interpretation of the American electorate (e.g., Aldrich, Sullivan, Borgida 1989; Popkin 1991; on policy preferences more generally, see Page and Shapiro 1992). The finding that voters are influenced (perhaps decisively) by policy issues has effectively decoupled current analysis of voters from the study of campaigns; voting studies consider issues quite consequential for voters, whereas the analysis of campaigns treats candidate strategy and behavior as non-issue oriented.

If analysis of campaigns is to be reintegrated with the study of voting, new approaches that recognize policy issues as an important basis for vote choice must be developed for investigating candidate behavior. Research on priming offers one approach for conceptually linking the analyses of campaign behavior and vote choice. Incorporating candidate behavior in the study of voters would mean treating the influences on vote choices as endogenous to the campaign (i.e., the impact of deliberate strategies that candidates pursue in order to win over voters). On the campaign side, priming is useful for refocusing campaign analysis on candidates' treatment of issues. Kennedy's 1960 candidacy illustrates how one campaign combined image building with a candidate's position taking on issues that responded to perceived public opinion.

Second, the Kennedy campaign's collection and use of polling information raises complex normative issues that defy simple categorization. On the one hand, the campaign's sophisticated use of opinion surveys might be considered consistent with a conception of liberal democracy in which political leaders both respond to and direct public opinion. In terms of democratic responsiveness, Kennedy was systematically guided by polling on voters' concerns. Moreover, liberal democracy is also premised on politicians directing or educating the mass public on critical issues of national importance. Kennedy's determined efforts to move public opinion toward embracing his position of backing foreign aid suggest that on this issue, he did alter the balance between leading and following public opinion. In addition, Kennedy's success in using a strategy of this sort may confirm the role of competitive elections in leadership selection. The Democrat's victory could be interpreted as foreshadowing his effectiveness as a national leader.

On the other hand, Kennedy's campaign strategy does have a disturbing implication for integrating elections into the governing process of America's liberal democracy. 45 In an ideal situation, competing candidates would respond to public opinion (and to pressure from party activists) by offering important and constructive policy positions. Voters' choices in the electoral process could then identify clear directions for subsequent government decisions. In effect, this style of competitive elections would fulfill the long-denied objectives of responsible party advocates: elections would not only serve as formal processes for determining succession but would be linked to choices over basic policy direction (Lowi 1967, 1985; Schattschneider 1948).

Kennedy's 1960 campaign devoted enormous resources to identifying the public's policy goals, but this information was used to influence evaluations of Kennedy's personal qualities. For responsible party advocates, then, the recoupling of voters and campaign strategy in this way is not promising: candidates incorporate issues into their strategy but they do not bank on substantive (and popular) policy positions to win their contests. Kennedy's selective use of issues meant that no clear meaning could be attached to his electoral victory; there was no mandate or popular confirmation of a policy platform to pursue in governing once he took office.

The emergence of this form of priming is consistent with a political regime that has often failed over much of the last two centuries to incorporate policy decisions into the electoral process (see Ginsberg 1972, 1976; Lowi 1967, 1985). Candidates' efforts to prime voters in order to manipulate their evaluations is but one more reason that Americans' choices of officeholders rarely settle policy issues.

Notes

We would like to acknowledge the able assistance of Scott Adler, Scott Allard, Angela Hernandez, Sandy Johnstone, Paul Kramer, Harpreet Mahajan, Sara Offenhartz, Edward Rodriguez, and Eli Schulman. Kelly Patterson generously shared data. Julie Schumacher provided her usual goodnatured assistance. Special thanks are owed to Mary DeFlorio and the rescue mission of Bob's mother-in-law, Bernice De-Florio. The staff at the John F. Kennedy Library provided archival assistance, and the Center for the Social Sciences (Columbia University) offered research assistance. Charles Cameron, Michael Delli Carpini, George Edwards III, William Flanigan, John G. Geer, Benjamin Page, Richard Pious, Steven Smith, James Stimson, and John Sullivan offered valuable comments. This project has also benefited from research grants from the University of Minnesota Graduate School, the University of Minnesota Land Grant-McKnight Professorship, the John F. Kennedy Foundation, and the National Science Foundation (SES-9122440)

1. Analysis of strategic position taking differs from Downs's own work in several critical respects. In contrast to what is often considered Downsian analysis, Downs himself emphasized the role of nonissue considerations (1957, esp. pt. 2) and, moreover, focused on the behavior of political parties,

rather than individual candidates.

2. It is not necessary to pinpoint the process by which attitudes become accessible—whether retrieved via chronic or temporary accessibility. It is reasonable to assume that even if attitudes are chronically accessible, the retrieval of such attitudes is enhanced by campaign priming (Aldrich, Sullivan, and Borgida 1989).

3. While we focus on candidate priming strategies, we recognize that voters face multiple sources of priming.

4. In fact, spatial models have taken this into account

mathematically by including a set of weights, which accounts for the relative importance of issues (e.g., the "a-matrix"; see Davis, Hinich, and Ordeshook 1970). One complication, however, is that different issues may be important to different individuals: the weights theoretically can vary across individuals. In addition—and most important—the weights are variables. They are not given.

5. The simulation project's input into the Kennedy campaign was largely limited to the period before the general election campaign. But, Pool, Abelson, and Popkin concede that their studies were not viewed as useful by Kennedy's aides, who relied on Louis Harris's studies (1964, 18)

6. We generally focus on responses to the following type of open-ended question: "Now, what do you feel are the two or three biggest problems facing people like yourself that you feel the national government should do something about in the next four years? Any others?" As we shall discuss, Harris reported not only the ranking of a policy's importance but also the policy direction that respondents preferred.

7. Harris to JFK, 23 July 1959, box 24, JFK Theodore Sorensen Papers, Kennedy Library; Harris to JFK, memo, 5 July 1960, box 39, Political, Pre-Administration, JFK/RFK Papers,

Kennedy Library.

- 8. JFK to Harris, memo, 5 July 1960, box 39, Political, Pre-Administration, RFK Papers, Kennedy Library; interview with Harris by authors, 17 June 1991.
 - 9. Interview with Harris by authors, 17 June 1991.
 - 10. Ibid.
 - 11. Ibid.
- 12. Archival records and our interview with Harris suggest that Kennedy treated the state public opinion data as representative of national opinion. It is on this basis that we compare the responses of Harris's state samples with Kennedy's policy statements.
- 13. The debates occurred on 26 September, and 7, 13, and 21 October, with the first focusing on domestic policy and the last on foreign policy (the subjects of the other two debates
- 14. Interview with Harris by authors, 17 June 1991; interview with Helen Lempart by N. Aldrich, March 1966, Kennedy Library; "Memorandum on the First Kennedy-Nixon Debate on Domestic Issues 22 September 1960, box 36" Political, Pre-Administration, JFK/RFK Papers; "Index to topics discussed and list of topics not discussed by Senator

Kennedy during the first four debates."

- 15. A subcommittee of the Senate Commerce Committee assembled a four-volume set of the public positions taken by Kennedy and Nixon as part of its investigation into the dissemination of political information during the 1960 general election campaign. We coded what were clearly Kennedy's main substantive policy statements in part 1 (speeches, remarks, press conferences, and statements) and part 3 (radio and television appearances) (United States, Congress, Senate 1961). Further information is available from the authors.
- 16. We prepared detailed written instructions and held frequent discussions with two graduate research assistants concerning coding rules and treatment of problematic cases. We independently coded random samples of Kennedy's statements and the content of the debates to verify that the instructions were followed. Coding discrepancies of more than one unit of our five-point scale occurred on approximately 5% of the assistants' and authors' coding. Finally, the coders were unaware of the polling results and therefore were unaffected by prior knowledge concerning our opinion mea-
- 17. For instance, if Kennedy took 3 positions within one week on the issue of civil rights and these statements were coded +2, -2, and +1, the weekly measure for the content would be +1.
- 18. Any issues omitted from the debates, the Senate material on daily statements, and Harris's polls (and thus excluded from our data) would have led to underestimates of the correlation between the poll reports and Kennedy's visible positions. Moreover, on opposing Communist China and issues related to Catholicism, Kennedy actually had addi-

tional opinion data that we did not incorporate in our statistical analysis. On the issue of Catholicism, Harris provided substantial evidence separate from his "important problem" series; on the issue of China, Harris reported general opposition to communism, which may have influenced Kennedy's more specific policy toward China. (We coded the candidate's position toward China as a separate issue.)

19. Harris did telephone Kennedy with preliminary results. Although horse-race reports on Kennedy's standing against Nixon could easily be conveyed verbally, the more cumbersome data on policy issues were reported last and took the longest time to incorporate into campaign strategy. Inter-

view with Harris by authors, 17 June 1991.

20. The use of one-week lags during September produced statistically insignificant results, which is consistent with the expectation that Kennedy's strategists did not receive and incorporate Harris's results until some two weeks after the survey's completion.

21. J. Miller, "Some Modest Realignments in the Kennedy Image," Memo, 20 October 1959, box 39, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; interview with Wilbur Cohen by Jacobs, 1 April 1987; interview with Cohen by David McComb, 8 December 1968.

22. Miller, "Some Modest Realignments."

23. Schlesinger to JFK, 26 and 30 August 1960, Special Correspondence, box 32, JFK/Presidential Office Files, Kennedy Library; "Memorandum on the Last Nine Days of Campaigning," confidential memo, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

24. Louis Harris and Associates, "An Analysis of the Third Kennedy-Nixon Debate," 19 October 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; inter-

- view with Harris by authors, 17 June 1991.
 25. Louis Harris and Associates, "An Analysis of the Third Kennedy-Nixon Debate," 19 October 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.
 - 26. Interview with Harris by authors, 17 June 1991
 - 27. Interview with Wilbur Cohen by Jacobs, 1 April 1987.
- 28. Interview with Harris by authors, 17 June 1991; Louis Harris and Associates, "A Study of Presidential Election in California" and "A Report of the Presidential Race in Maryland," September 1960, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; Louis Harris and Associates, "A Report on the Presidential Election in Maine," box 44, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library
- 29. Interview with Harris by authors, 17 June 1991; interview with Ivan Nestingen, 1966, Social History Research Office, Columbia University. Leaders of the Kennedy campaign in the states during both the primaries and the general election recalled support for the medical care issue; these leaders included Nestingen in Wisconsin; Anthony Akers in New York; and Charles Love, Jr. in West Virginia. In addition, close advisers also reported the same favorable reaction. Interview with Akers by William Moss, 17 July 1971, Kennedy Library; interview with Love by William Young, 14 July 1964, Kennedy Library; Abraham Ribicoff to RFK, 15 October 1960, box 25, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; "Memorandum on the Last Nine Days of Campaigning," confidential memo, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

30. "Memorandum on the Last Nine Days of Campaigning," confidential memo, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library. This theme is also

prevalent in Harris's polling reports.

31. Louis Harris and Associates, "A study of Presidential Election in Ohio, Wave I," 6 September 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; "Memorandum on the First Kennedy-Nixon Debate on Domestic Issues," 22 September 1960, box 36, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; Louis Harris and Associates, "A Study of Presidential Election in California, Wave III," 31 October 1960, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.
32. "A Report of the Presidential Race in Maryland," Louis

Harris and Associates, Inc. September 1960. This is a constant theme in Harris's reports.

33. Louis Harris and Associates, "A Study of Presidential Election in Florida," 13 September 1960, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; Louis Harris and Associates, "A Study of Presidential Election in Maryland, Wave I," 23 September 1960, box 44, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; idem, "A Survey of the Presidential Election in South Dakota," 19 September 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; "Simulatics, Report #2," Memo, 25 August 1960, box 48, Political, Pre-Administration, JFK/RFK Papers, Kennedy Libraries.

34. For instance, in preparation for his presidential candidacy, Kennedy presented to the Senate, in August 1958, a 10-point program to establish a "Bill of Rights for the Elderly, in which health insurance was one of the top priorities. In addition to formal statements, Kennedy cosponsored a Medicare bill in the Senate during the campaign in spring 1960; by the fall, he cosponsored an important amendment that would establish a health insurance program for the elderly.

35. Steve Smith to Campaign Coordinators, memo, 19 October 1960, box 46, Political, Pre-Administration, JFK/RFK

Papers, Kennedy Library.

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36. "Index to topics discussed and list of topics not discussed by Senator Kennedy during the first four debates" and "Memorandum on the First Kennedy-Nixon Debate on Domestic Issues," 22 September 1960, box 46, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; interview with Helen Lempart by N. Aldrich, March 1966, Kennedy

37. Clark Clifford to JFK, memo, 27 September 1960, box 36, Political, Pre-Administration JFK/RFK Papers, Kennedy Library

38. The interrelationship between Kennedy's and Nixon's behavior underscores the fact that campaigns involve strategic interactions, rather than a one-player game. Neither space nor comparable data enable us to explore here this important

component of campaign strategy.

39. Interview with Clinton Anderson by John Stewart, 14 April 1967, Kennedy Library. "Memorandum on the last Nine Days of Campaigning," confidential memo, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; George Belknap, "Most Americans Are Basically 'New Deal-June-August 1960, Box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; Abraham Ribicoff to RFK, 15 October 1960, box 25, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

40. Louis Harris and Associates, "A Study of Presidential Election in Ohio, Wave II," 19 October 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

41. For instance, while health insurance is an obvious issue on which the political parties disagreed, Kennedy's proposal differed in important respects from the last major Democratic

plan, Truman's universal health insurance proposal.
42. JFK to George Gallup, 24 July 1959, box 24, JFK/ Theodore Sorensen Papers, Kennedy Library; Harris to JFK, 23 July 1959, box 24, JFK/Theodore Sorensen Papers, Kennedy Library; Harris to JFK, 5 July 1960, box 39, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; interview with Harris by authors, 17 June 1991.

43. Johnson significantly expanded the White House's poll-

ing operation (Jacobs and Shapiro 1992).

44. Future research may find that the campaign responsiveness to public opinion can lead to strategies that are quite different from that which Kennedy pursued. For instance, reported opinion led the 1988 Bush campaign to avoid economic issues, which were the top public concerns, and instead to focus attention on the previously quiet issues of crime and patriotism (Moore 1992).

45. Lying or misleading the public would be another serious threat to liberal democracy; evidence of this type of manipulation occurred in the case of the alleged U.S.-Soviet "missile gap" (see Key 1961; Page and Shapiro 1992, chaps. 6,

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ENHANCING DEMOCRACY THROUGH LEGISLATIVE REDISTRICTING

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The demonstrate the surprising benefits of legislative redistricting (including partisan gerrymandering) for American representative democracy. In so doing, our analysis resolves two long-standing controversies in American politics. First, whereas some scholars believe that redistricting reduces electoral responsiveness by protecting incumbents, others, that the relationship is spurious, we demonstrate that both sides are wrong: redistricting increases responsiveness. Second, while some researchers believe that gerrymandering dramatically increases partisan bias and others deny this effect, we show both sides are in a sense correct. Gerrymandering biases electoral systems in favor of the party that controls the redistricting as compared to what would have happened if the other party controlled it, but any type of redistricting reduces partisan bias as compared to an electoral system without redistricting. Incorrect conclusions in both literatures resulted from misjudging the enormous uncertainties present during redistricting periods, making simplified assumptions about the redistricters' goals, and using inferior statistical methods.

'n 1982, the Michigan Supreme Court imposed a redistricting plan on their state that was generally believed to favor the Republicans. The Democrats' alternative measure had to pass the legislature with a two-thirds vote, which was difficult even though they had a majority in both houses. Democratic leaders tried to sneak through the legislation by gutting the contents, but not the title, of an irrelevant bill at the last minute and inserting redistricting legislation. The Republicans discovered this ploy, making the situation extremely tense. In the heat of the long debate during this midnight session, a Democratic senator collapsed. Paramedics were called in, but he refused to leave the Senate floor before the vote. In a classic case of political hardball, a Republican senator used parliamentary procedure to delay the vote by insisting that the legal description of all 148 legislative districts and their boundaries be read into the record. Despite his failing health, the Democratic senator stayed through the entire reading, and his party won the vote.1

During the Illinois redistricting process,

Republican state Senator Mark Rhoads believed he had the Democratic votes needed to pass a GOP map in the Senate. In a rare Sunday legislative session Rhoads became outraged over the parliamentary tactics employed by [Senator] Rock to delay a vote on reapportionment. Unable to control his anger, Rhoads attempted to charge the podium and get at Rock. However, before he reached the burly Senate president, Democratic downstate Senator Sam Vadalabene sucker-punched Rhoads with a right to the jaw. According to eyewitness Al Manning of the State Journal Register, "for a moment it looked as though both benches were going to empty," but, with the television cameras grinding, the combatants were pulled apart. Later in the day, Rock eventually called the remap bill and with total party unity the Democrats passed out their own bill, thus assuring a reapportionment deadlock. (Green 1982, 32)

These are among the most colorful recent redistricting stories, but they accurately portray the intensity of the partisan conflict in many such processes throughout the United States. From George Washington's first presidential veto to the present day, redistricting issues have been extremely controversial at every level of government. Most redistrictings are contested in state and federal court cases heard so late that there is insufficient time to follow the usual rules of discovery, evidence, or due process. In total, legislative redistricting is one of the most conflictual forms of regular politics in the United States short of violence.

While partisan and bipartisan redistricting plans can protect incumbents, they only protect some of those who survive the redistricting process—and many do not survive. Indeed, most incumbent politicians would give an awful lot to avoid redistricting altogether. After all, they are fighting over the fundamental rules of the game (fights that might well have been concluded at the founding of the republic) and for their own political survival. As a result, redistricting creates enormous levels of uncertainty, an extremely undesirable situation for any sitting politician. Indeed, because the costs of the political fight frequently outweigh the benefits of government service during redistricting, incumbents disproportionately choose to retire at this time.²

Some scholars assume that those who draw the district lines are motivated by incumbent protection, whereas others believe the motivation is partisan advantage, but even the briefest discussion with participants in the process indicates that redistricters are concerned with both. Indeed, these are often competing goals: incumbents are often forced to give up votes (hence electoral safety) in order to increase the number of legislative seats their party is likely to capture. The tension between the goals of individual and partisan advantage creates yet additional uncer-

tainty about the outcome of a redistricting. Since political party gain is the most predictable common ground for otherwise competing incumbents, party advantage will often take precedence over individual incumbents' advantage in the ultimate political compromise represented by a redistricting plan.

Moreover, not only do redistricters attempt to maximize the competing goals of incumbency protection and partisan advantage, but incumbency protection is itself composed of competing goals: winning the general election and winning (or avoiding) the primary election. These goals conflict because adding too many of a legislator's political party members to his or her district (hence piling up expected votes in the general election) might leave the incumbent vulnerable to a now larger opposition faction within his or her party primary.³

In addition to the high levels of political conflict and uncertainty and the conflicting goals of those who draw the district lines, the entire process includes several severe legal and political constraints. These include the requirements of equal population, contiguity, compactness, minority representation, maintaining communities of interest, not splitting local subdivisions, and especially protecting some incumbents, all within the context of complicated local geography. Other constraints are much less widely recognized but no less important to incumbents, such as the inclusion of the right political contributors, the exclusion of prospective challengers, and the keeping of each favored incumbent's several district offices within the district.⁴

Thus, in our view, the key to understanding the effects of redistricting is to view redistricters as trying to achieve consensus among—or impose a solution on—incumbents who are operating in an extremely uncertain environment and attempting to reconcile at least three competing goals: to maximize their probability of winning or avoiding a party primary, to win a general election (conditional on winning the primary), and to increase their political party's seat advantage. The resulting redistricting plan is usually a compromise, heavily influenced by numerous formal and informal constraints, which generally weights the political party's overall seat advantage most heavily.⁵

We shall evaluate, and then resolve, two important scholarly disagreements about the effects of legislative redistricting on two features of American democratic electoral systems: electoral responsiveness and partisan bias. Both sides in each debate are inconsistent with part of the substance of redistricting as just portrayed. The results of our analyses define and establish new positions. They do not fully support either side in what were previously portrayed as either/or debates but are consistent with the political substance of legislative redistricting discussed here. Our empirical analysis also succeeds by using more powerful methods, more accurate information about more redistricting plans, and dozens of times more data than have heretofore been brought to bear on these problems. Our empirical results have important counterintuitive policy implications, since, in total, they imply that the existence of legislative redistricting—and even partisan-controlled gerrymandering—has beneficial effects on American electoral systems, increasing electoral responsiveness and reducing partisan bias.

THE SCHOLARLY DEBATE AND PROPOSED RESOLUTIONS

We shall begin by introducing the scholarly debates, proposing resolutions, and overviewing our empirical results.

Electoral Responsiveness

Electoral responsiveness is the degree to which the partisan composition of the legislature responds to changes in voter preferences. Although closely related concepts exist—including the competitiveness of the electoral system, the probability that an incumbent will lose a reelection bid, the frequency of marginal seats, and the swing ratio—we find electoral responsiveness (which we shall define precisely later) to be the most direct representation of the relevant theoretical concept of interest.⁶

Political scientists have typically taken two contradictory positions about the effect of redistricting on the responsiveness of an electoral system. One set of scholars maintain that partisan and bipartisan redistricting plans reduce electoral responsiveness. For example, Cain writes, "Because incumbents tend to be risk averse—no margin of safety is too much—the result [of a bipartisan redistricting plan] is greater electoral inefficiency and more noncompetitive seats" (1985, 321). Mayhew (1971) and Tufte (1973) also argue that bipartisan redistricting plans are primarily incumbent protection, hence reducing responsiveness of legislative seats to citizen votes. Owen and Grofman (1988) show theoretically that optimal partisan redistricting plans should also produce a less responsive electoral system. A different position has been argued by another group of scholars: "Redistricting has no influence at all on the swing ratio" (Ferejohn 1977; see also Burnham 1974).

This is an important scholarly debate, but neither position is fully consistent with our prior qualitative knowledge. For example, although some incumbents benefit from redistricting, all (or even most) do not. Many of the incumbents of the party not in control of the process will lose support even if they are not actually paired into the same districts. Some will intentionally reduce their general election support in order to avoid a primary. Moreover, because of geographic constraints, redistricting even hurts some incumbents of the party in control. Improving the partisan composition of a district for one incumbent requires modifying the neighboring district boundaries, and neighboring districts are not always conveniently open seats or held by opposition party

members. As a result, in addition to interparty competition, redistricting frequently creates intraparty competition among rational officeholders seeking to maximize their probability of reelection: "[The] scrambling of incumbents can have momentous importance for the election that follows the redistrict-

ing" (Cain 1985, 331).

How could redistricting have no effect on—or even reduce—electoral responsiveness when it loosens the hold of many incumbents of both parties on their electoral constituencies and reduces their chances of reelection? In fact, our empirical results indicate that both prevailing positions in the literature are incorrect. Redistricting (whether partisan or bipartisan) tends, on average, to increase electoral responsiveness (see also Campagna and Grofman 1990; King 1989). Redistricting does this by shaking up the political system and creating high levels of uncertainty for all participants. Moreover, when redistricters draw lines by jointly maximizing the advantages to their party and their incumbents, they create additional uncertainty and also produce a direct increase in responsiveness by attempting to gain partisan advantage by creating more districts with smaller likely victory margins.

Partisan Bias

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Partisan bias is the degree to which an electoral system unfairly favors one political party in the translation of statewide (or nationwide) votes into the partisan division of the legislature. Politicians, journalists, some judges, and many political scientists believe that political parties in control of redistricting produce sizable effects on the degree of partisan bias in the electoral system (see Abramowitz 1983; Born 1985; Cranor, Crawley, and Sheele 1989; Erikson 1972; Gopoian and West 1984; Hacker 1963; Niemi and Winsky 1992). This results in important political consequences. For example, Robert Dixon insists, "Apportionment and districting decisions are primary determinants of the quality of representative

seat advantage and have a large and lasting effect, while the other argues that whatever gerrymanderers maximize, they have only a small or transitory effect. Paradoxically, we find that both sides in this debate are correct. The disagreement appears to lie in a difference over the precise causal question asked. From the perspective of a close observer of the process and the first group of scholars, redistricting certainly has a partisan "effect", but this effect is defined (implicitly) as the consequence of Democraticcontrolled versus bipartisan or Republican-controlled redistricting. The causal effect of interest to this first group is the difference between bias in the electoral system when redistricting is controlled by Democrats versus Republicans (although obviously only one kind of redistricting is observed at any one time). Any good politician knows the consequences of letting the opposition party draw the district boundaries. We find that the difference here is as predicted: on average, redistricting favors the party that draws the lines more than if the other party were to draw the lines. In fact, the effect is substantial and fades only very gradually over the following 10 years.

The second group of scholars in this debate finds no "effects," or else finds effects that disappear quickly over time. It appears that the causal question asked by this group is distinct from the first, focusing not on the difference between Democratic- and Republican-controlled redistricting but on the difference between the consequences of redistricting versus no redistricting. We find that on average, redistricting (either partisan or bipartisan) actually reduces the degree of bias as compared to no redistricting. Most of the especially effective partisan gerrymanders take a political system severely biased in favor of one party and make it slightly biased in favor of the other, hence reducing the overall bias. This result does not contradict the position of the first group, since partisan plans do favor the party in control compared to bipartisan plans, but they all reduce the overall degree of bias compared to what would have been if no redistricting had occurred. We now turn to the

fortunately, adequate information about state redistricting processes have never been compiled. Sources such as Hardy, Heslop, and Anderson 1981 and Cortner 1970 and numerous court cases provide valuable but insufficient information. Previous studies of political gerrymandering either analyze a single case or a few cases in depth (e.g., Cain 1984; King 1989; Scarrow 1982) or use only indirect evidence of partisan control of redistricting (see Morrill 1990 for a partial exception). For example, Erikson (1972), Born (1985), and King and Browning (1987) infer control of redistricting processes indirectly by noting the party that controlled the state legislature and governorship, with special rules to deal with court challenges and other exceptions. (If one party controlled all three, the plan was assumed to have been gerrymandered by that party; if control was split, they concluded the plan was bipartisan.) This inferential procedure has the advantage of being easy to implement and is often correct, but it is misleading in many cases. For example, some state constitutions give control of redistricting to bipartisan commissions, regardless of who controls the government. In other states, the courts have at times implemented the minority party's redistricting plan (on grounds other than political gerrymandering but presumably with the same effect). And in all states, creative maneuvers by politicians, using techniques such as court challenges and legislative impasses, can cause redistricting to occur at times other than immediately following the decennial censuses. Using these indirect methods causes many redistrictings to be missed and many of those not missed to be misclassified.

To avoid problems with existing measures, we conducted an in-depth study of each redistricting process in every state. We mailed a questionnaire to every state legislature, requesting the names and party affiliations of all individuals who participated in the redistricting process, the official and unofficial rules of the apportionment and districting process, copies of the final redistricting bills, and certain district maps. We then interviewed state election officials, state court justices, commission members, attorneys, academics, legislators, and political party officials, as well as looking at many state newspapers and scholarly literature. Throughout, the goal was to gauge the intention, rather than the perceived effect, or publicly stated goal, of a particular redistricting plan. Regardless of whether the redistricting was implemented by a legislature, a governor, a commission, or a court, we categorized each plan by its partisan intention. We finished collecting the redistricting data before calculating any estimates from our electoral data to eliminate possible coder-induced endogeneity (in fact, an early version of our data were used almost three years ago; see Niemi and Jackman 1991). From this information, we identified 60 redistrictings and classified each as Democraticcontrolled, Republican-controlled, or bipartisan. ⁹ The states, years, and classifications of the redistrictings appear in Appendix B.

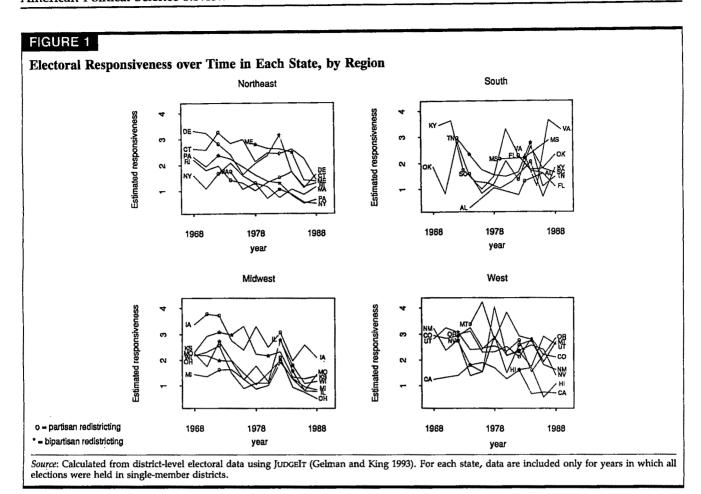
DEFINING AND ESTIMATING ELECTORAL RESPONSIVENESS AND PARTISAN BIAS

We estimate electoral responsiveness and partisan bias in each state legislature, for each of the 267 election years, using the model described by Gelman and King (1994) and the associated computer program. Although we developed this statistical model to estimate bias and responsiveness in legislative data, it has numerous other applications. This methodology is briefly summarized in Appendix A. For each state and election year, we calculate a point estimate and standard error for electoral responsiveness and partisan bias and the same quantities in the counterfactual situation in which all incumbents suddenly retire; we use these for all subsequent analyses. Results from our numerous auxiliary analyses not reported here, with alternative measures of these and related concepts, strongly support our substantive conclusions described. For each state election in the data set, our estimates of electoral responsiveness and partisan bias, along with the number of seats in the legislative house, appear in Appendix B.

In order to define these concepts more precisely, we define \bar{v} to be the average Democratic proportion of the two-party vote across districts in the state (corrected for uncontested seats; see Gelman and King 1994 for details) and \bar{s} to be the Democratic proportion of the seats in the legislature. We also account for the effects of differential turnout. To each state's electoral system in each election year, we estimate electoral responsiveness and partisan bias. (We take our definition of these concepts from King and Gelman 1991, which generalized the definitions introduced in King and Browning 1987 and King 1989.)

Electoral Responsiveness

We define *electoral responsiveness* as the change in the expected seat proportion given a small change in the vote proportion, from slightly more Democratic than the average district vote to slightly more Republican (see King and Browning 1987; Gelman and King 1994). For present purposes, we use a swing of 1% in each direction from the election outcome: responsiveness is the average difference, $[E(\bar{s}|\bar{v}+.01)-E(\bar{s}|\bar{v}-.01)]$, divided by the vote swing, .02.¹¹ For example, a value of responsiveness of 1.0 is (in the absence of bias) de facto proportional representation. A value of 2.0 (approximately the average value across all the data we analyze) indicates that a 1% increase in the average district vote share for Democratic candidates statewide will produce a 2% increase in the Democratic share of the state legislature. Scholars of American politics almost uniformly take the normative position that higher values of responsiveness indicate a healthier democracy (e.g., Ferejohn 1977). (In stark contrast, scholars from most other countries prefer proportional representation and therefore lower values of responsiveness nearer 1.0; a valuable



topic for future research would be to work out the conditions under which each normative standard is most appropriate.) Figure 1 presents a descriptive view of electoral responsiveness over time in each state in our data set. Most states have responsiveness values between 1.0 and 3.0. Except for the South, responsiveness has gradually dropped over time, just as in the U.S. Congress (King and Gelman, 1991). As the Republicans have gained strength in the South, the legislatures of southern states have become more competitive with increasing electoral responsiveness.

Partisan Bias

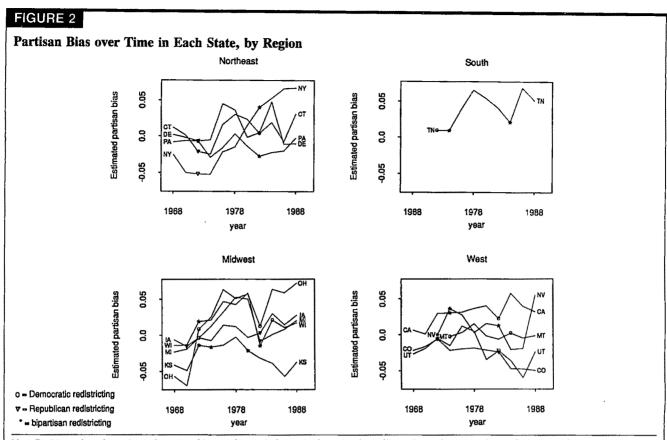
Partisan bias is the deviation from partisan symmetry when the average district vote is between $\bar{v}=.45$ and $\bar{v}=.55$. For example, if one party is able to translate 55% of the average district vote into 75% of the seats in the legislature, then it would be symmetric for the other party, too, when it receives 55% of the average district vote, to receive 75% of the seats. We define partisan bias as the proportion of the seats in the legislature that the Democrats receive over and above what is fair according to this symmetry criterion. A positive bias favors the Democrats, whereas a negative value for bias indicates that the electoral system favors the Republicans. For example, if partisan bias is -.05, then the Democrats receive 5% fewer seats in

the legislature than they should under the symmetry standard (and the Republicans receive 5% more seats than they should).

Unlike electoral responsiveness, estimating partisan bias requires imagining what would happen if the minority party were to become the majority party in some future election. Although this is obviously possible in any state, it is extremely unlikely in some. Forecasting what would happen if, for example, the Republicans suddenly won a majority of the Alabama legislature is beyond the scope of any empirical analysis. We therefore limit our analysis to "competitive electoral systems," which we define as states in which each political party managed to garner a majority of seats or votes in at least one election between 1968 to 1988. For the analysis of bias, this reduces our data set to 16 states and 164 elections. Less restrictive definitions of competitive do not materially change our substantive results. 12

Figure 2 displays our estimates of partisan bias over time in each state (only for "competitive electoral systems"). Most of the bias figures are between 5% favoring the Democrats to 5% favoring the Republicans. Partisan bias in these states seems to be trending from favoring the Republicans to favoring the Democrats. This is also true of the U.S. House (see King and Gelman 1991).

Finally, we also estimate what values each of these three quantities would be if, at the start of each



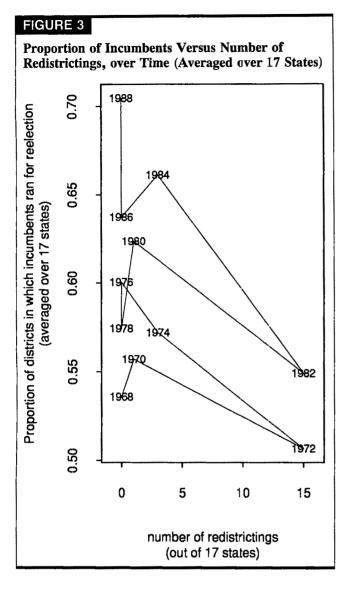
Note: Positive values for estimated partisan bias on the vertical axes in these graphs indicate electoral systems that unfairly favor the Democratic Party; negative values indicate bias in favor of the Republican Party. Calculated from district-level electoral data using JUEGEIT (Gelman and King 1993). For each state, data are included only for years in which all elections were held in single-member districts. Only states with competitive electoral systems are included.

election campaign, all incumbents decided not to run. This simulated universal term limitation helps us ascertain the role of incumbents in redistricting. We omit the analogous time series plots of these figures, but to gain an understanding of the changes in incumbency over time, we present Figure 3. For each year in the data set, this figure displays the number of redistrictings in that year by the proportion of incumbents running for election. 13 A line connects the points so one can easily trace the path of the electoral system over time. Two patterns are clearly evident in this graph. First, from 1968 to 1988, the proportion of incumbents has increased from about 50% to nearly 70%, as portrayed in the graph by the line headed steadily upward. Second, the proportion of incumbents clearly decreases during redistricting years. Figure 3 also portrays an important effect of redistricting. Some incumbents are undoubtedly "retired" involuntarily by redistricting plans that eliminate their political base or pair them in districts, forcing them to run against each other. However, many other incumbents probably also retire to avoid the huge political fight of redistricting itself. If a state legislator were thinking of retiring (or running for another office) at some time over the next few elections, then planning an exit to coincide with a redistricting would save a lot of grief, if not political defeat.

A REGRESSION MODEL

We conduct parallel analyses of electoral responsiveness and partisan bias. Each variable has the state election year as the unit of analysis (n = 267 for responsiveness and n = 164 for bias). The core analysis involves a straightforward regression model using each of these dependent variables in turn. We present the regression analysis of each numerically and then provide a further interpretation of these regression results with two forms of graphical analysis.

We denote Y_{it} as the dependent variable (electoral responsiveness or partisan bias), minus its mean, for election year t and state i. ¹⁴ In addition, we define the following explanatory variables: 15 redistricting, R_{it} , is 1 if a redistricting occurred immediately before election year t in state i, and 0 otherwise; partisan redistricting, P_{it} , is .5 if a redistricting occurred immediately before election year t in state i and was exclusively controlled by one of the major political parties, -.5 if the redistricting was bipartisan (controlled by both parties), and 0 otherwise; democraticcontrolled redistricting, Dit, is 1 if the Democratic party controlled the redistricting immediately before election year t in state i, -1 if the redistricting was Republican-controlled, and 0 otherwise; and state effects, S_{ii} , are a set of 0/1 dummy variables representing the states.



Regressions for Electoral Responsiveness. We do not use all the explanatory variables for each regression. To assess the effects of redistricting on electoral responsiveness, we estimate the following linear regression:

$$E(Y_{it}) = \beta_1 R_{it} + \beta_2 (Y_{i,t-1} R_{it})$$

$$+ \beta_3 P_{it} + \beta_4 Y_{i,t-1} + \gamma S_{it}$$
, (1)

where β_1 , β_2 , β_3 , and β_4 are regression coefficients, γ is a vector of regression coefficients, and $(Y_{i,t-1}R_{it})$ is an interaction term calculated by taking the product of the lagged dependent and redistricting variables.¹⁶

Inclusion of the state variables, S_{it} , is a standard procedure recommended in time series cross-sectional literature, where it is called a "fixed effects model" (Hsiao 1986; Stimson 1985). These variables enable us to pool different states safely, guaranteeing that we are comparing (for example) New York in one year with New York in another, rather than New York in one year with Rhode Island in another. (Including all the state variables also makes the constant term in the regression unnecessary.) From a

theoretical perspective, a random effects model might be preferred (e.g., Dempster, Rubin, and Tsutakawa 1981). We repeated all our regressions with random state effects and found no major substantive changes in the results.¹⁷

As such, estimating the coefficients in γ is important, but the values they take on are not of direct interest; we therefore omit these from our tables. The remaining coefficients are of interest and are interpreted as follows: β_1 is the average effect of any redistricting in increasing Y (responsiveness or bias); β_2 is generally negative, indicating how much larger the effect of redistricting is for small previous values of Y and how much smaller it is for larger values of Y (β_2 is, equivalently, the drop in the persistence in the level of responsiveness between two elections due to a redistricting intervening); β_3 is the additional constant effect of partisan versus bipartisan redistricting over and above the average effect; and β_4 indicates the persistence of Y over time in the absence of redistricting.18

Regressions for Partisan Bias. To estimate the effects of redistricting on partisan bias, we change equation 1 only by substituting P_{it} with D_{it} . This changes the interpretation of β_3 to the constant effect of Democratic-versus Republican-controlled redistricting over and above the average effect. The interpretation of the other coefficients does not change.

It should be possible to improve our model if additional data become available, but in the many alternative models and diagnostic tests we tried, we found no evidence to contradict the model in equation 1. We also found the error distribution and autocorrelation structure of the data to be consistent with the time series behavior of the model.¹⁹

Although we found no evidence of nonlinearities, the empirical independence of the lagged dependent variable and the redistricting variables makes our specification unreliant upon the linearity assumption in estimating our key causal effects. Finally, the substantive results we are about to present were robust across all reasonable specifications that were consistent with the data.

EVIDENCE

We present our empirical results first for electoral responsiveness and then for partisan bias. We conduct the analyses in each of these sections in analogous fashion and explain our procedures in most detail in the first.

Electoral Responsiveness

Our regressions explaining electoral responsiveness appear in Table 1. Column 1 reports the estimated regression effects (with standard errors in parentheses) on the actual level of responsiveness. Column 2 reports estimates for a regression with the same explanatory variables but with a forecast of what

TABLE 1

Effects of Redistricting on Electoral Responsiveness: Regression Estimates

EXPLANATORY VARIABLE (PARAMETER)	ACTUAL	NO INCUMBENTS RUNNING
Redistricting (\hat{eta}_1)	.47 (.08)	.98 (.11)
Redistricting interaction $(\hat{\beta}_2)$	- 17 (.10)	20 (.11)
Partisan redistricting (\hat{eta}_3)	−.23 (.16)	32 (.21)
Lagged responsiveness $(\hat{\beta}_4)$.38 (.06)	.17 (.06)
Residual standard deviation ($\hat{\sigma}$)	.51	.64

Note: Entries are coefficients from a weighted least squares regression; standard errors appear in parentheses. N=237 election years (we dropped the first case in each state so that we could regress on the lagged variable). The state effects, S_{tt} , are also included in the regressions but are omitted from the table because their coefficients are not of direct interest.

electoral responsiveness would be under the situation with no incumbents running as the dependent variable. We discuss these coefficients here, followed by a more detailed interpretation in several figures.

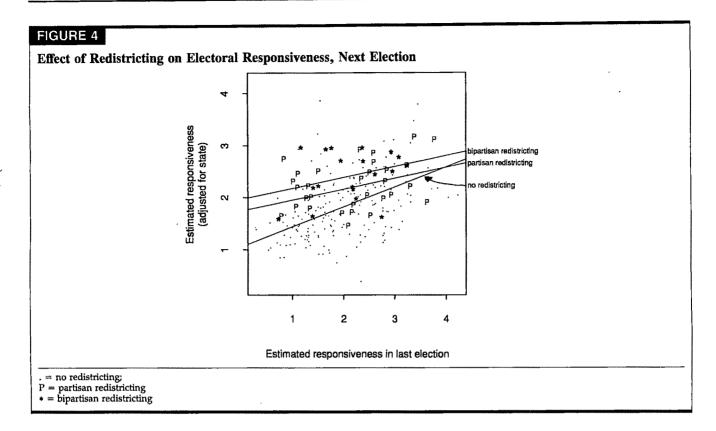
The primary effect indicates that on average, redistricting increases responsiveness by about half a point (.47 plus or minus the .08 standard error). This is a substantial effect, consistent with our qualitative understanding that redistricting creates uncertainty by shaking up the political system and that redistricters are maximizing competing goals. Table 1 also indicates that the effect on responsiveness in a hypothetical electoral system without incumbents is even larger. Put differently, by adding incumbents into the electoral system and redistricting process, the increase in responsiveness that results from redistricting is lessened. We attribute this effect to incumbents trying to maximize their probability of winning the general election, since our analysis holds constant all the other constraints we have observed.

The "redistricting interaction" coefficient indicates that the effect of redistricting is slightly larger when responsiveness is low, and smaller for higher values of responsiveness. This is a "regression to the mean" that pushes the effects of all types of redistricting toward a common level. The effect is not much larger than its standard error, but our many experiments with auxiliary regressions testing this hypothesis convinces us that this effect, although small, is real. As expected, the effect of partisan versus bipartisan redistricting is small and imprecisely measured. The lag of responsiveness indicates that in nonredistricting years, redistricting has a moderate level of persistence, meaning that it is possible to forecast responsiveness from its previous values (and the state coefficients). Put differently, in the absence of redistricting, electoral responsiveness is moderately persistent across elections. However, when a redistricting occurs between these two elections, most of this persistence vanishes (since $\hat{\beta}_4 + \hat{\beta}_2$ is small). Moreover, virtually all of the persistence of responsiveness across a redistricting is due to the presence of incumbents (since $\hat{\beta}_4 + \hat{\beta}_2$ is approximately 0 for Table 1, column 2).

That the persistence of responsiveness across a redistricting is due largely to the incumbents and their districts is consistent with information we gathered in interviews with district-map-makers. That is, those nominally in charge of redistricting, as well as the people who sit at the computers, drawing lines on maps, almost uniformly report that finding satisfactory districts for incumbents is their most important constraint. They also report that the constraint posed by the presence of incumbents is the one of the biggest factors in making their job difficult.

Immediate Effects of Redistricting on Responsiveness. Figures 4 and 5 portray the effects of redistricting on responsiveness graphically, displaying both the data and the regression results in Table 1. Figure 4 illustrates the immediate effects of redistricting by plotting the lagged value of responsiveness by the current value (adjusted for the state effects by subtracting from each value the estimated coefficient for the corresponding state dummy variable). This figure highlights every data point as well as all the regression coefficients; it also emphasizes exactly how the regression summarizes the data in this case. Each of the 237 state elections appears in this plot as a dot (for elections without redistrictings), an asterisk (for years preceded by bipartisan redistrictings), or the letter P (for years preceded by partisan redistrictings). For each point, the responsiveness estimated from that statewide election appears on the vertical axis (after being corrected for the appropriate state effect), and the estimated responsiveness in the state's previous election is shown on the horizontal axis. The most important effect in this figure is that most of the points labeled P and * are higher on the vertical axis (for a given value of the horizontal axis) than the dots. This shows directly that responsiveness is higher in years following redistricting even after controlling for responsiveness in the previous election (and adjusting for state means).

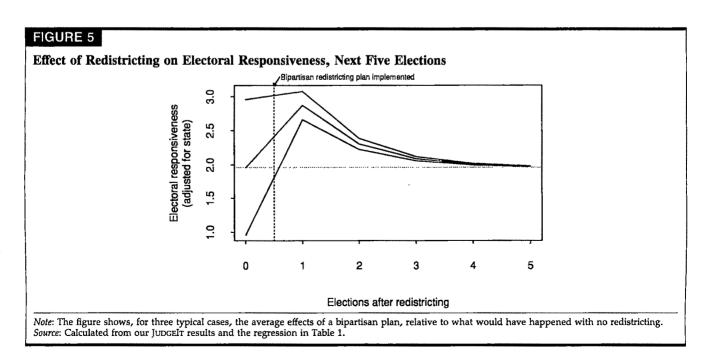
We also plot the regression lines, fitted to the three types of election years, to help highlight average effects. The coefficient of lagged responsiveness, $\hat{\beta}_4$ = .38, is the slope of the line labeled "no redistricting." The increase in responsiveness due to redistricting is portrayed by the vertical distance between the noredistricting line and the partisan or bipartisan redistricting lines. The distance between the differently sloped lines at the center of the graph is the average effect of redistricting: $\hat{\beta}_1 - \hat{\beta}_3 = .70$ for bipartisan and $\hat{\beta}_1 + \hat{\beta}_3 = .24$ for partisan redistricting. The difference between the slopes of the regression line is $\hat{\beta}_2 = -.17$. As can be plainly seen, the effect of redistricting is large for small values of lagged responsiveness (at



the left). Or, equivalently, the persistence of responsiveness (as represented by the steepness of the line) is much greater in the absence of redistricting.

Figure 4 helps convey, better than the coefficients in Table 1 can alone, the appropriateness of our regression model for this problem. (For example, one can see the dots on this graph clearly clustered around the no-redistricting line and below the asterisks and *Ps.*) Perhaps the most important feature of

the graph, confirming that the assumptions of our regression model cannot be rejected by the data, is the absence of any systematic pattern in the data points except those picked up by the regression lines. Finally, Figure 4 shows the unpredictability of redistricting: even after controlling for the lagged responsiveness and state effects, the points on the graph are quite variable. The strong average effects revealed by the regression do not apply to all individual cases.



Longer-Term Effects of Redistricting on Responsiveness. Figure 5 displays the average effects on responsiveness (relative to any trend) for the five elections following a redistricting. The horizontal axis indicates the number of elections since redistricting, with the vertical dashed line indicating the implementation of a bipartisan redistricting plan. To show the differential effects of redistricting, Figure 5 gives three examples, distinguished by the level of responsiveness in the election before redistricting. All three examples show again that, on average, redistricting sharply increases responsiveness in the first election, with the largest effect occurring for states that had low responsiveness before the redistricting. The effects in the graph are calculated from the regression in Table 1 and the implied AR(1) time series model.

Figure 5 shows that the average redistricting effect is very large in the first year, moderate in the second, much smaller in the third, and nonexistent for the fourth and last years. The effect of redistricting in increasing responsiveness does not last until the fifth election, implying that redistricting is unlikely to contribute to longer-term trends. Hence, the existence of many redistrictings increasing responsiveness is consistent with a secular decline in responsiveness over the two decades in our study (see Figure 1). However, this pattern does not mean that the effect of redistricting on responsiveness is unimportant. On the contrary, in the typical state, redistricting occurs after every fifth election, and, as a result, its electoral system benefits from higher levels of responsiveness for roughly half of all elections solely because of redistricting. Redistricting thus boosts the responsiveness of a state electoral system significantly higher than it would otherwise be for about half of all elections. Although any single redistricting does not have permanent effects, the decennial redistricting process is a permanent part of every state's electoral system. As a result, redistricting continually and fundamentally alters the character of representative democracy.

Partisan Bias

In addition to its effect on responsiveness, legislative redistricting has an important effect on the relative fortunes of the political parties. Redistricting can affect the proportion of seats that a party controls in the legislature in two ways. The first, which has been the subject of speculation in the literature but only rarely of empirical analyses, is the effect of redistricting on the average district vote. The second is what we call partisan bias—the effect of redistricting on the allocation of seats between the parties given their average district votes. The ultimate effect of redistricting on the division of parties in the legislature is the sum of these two effects. One can use the seat proportion as the dependent variable in a separate regression to estimate this sum directly. Like most researchers, we prefer a separate estimate of the effects on partisan bias so that we can judge the fairness of the electoral system.

TABLE 2

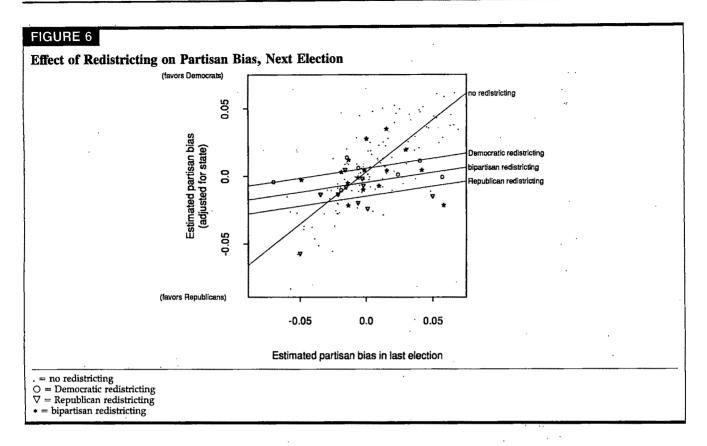
Effects of Redistricting on Partisan Bias: Regression Estimates

EXPLANATORY VARIABLE (PARAMETER)	ACTUAL	NO INCUMBENTS RUNNING
Redistricting $(\hat{\beta}_1)$	012 (.004)	012 (.004)
Redistricting interaction $(\hat{\beta}_2)$	63 (.12)	77 (.16)
Democratic redistricting $(\hat{\beta}_3)$.010 (.005)	.003 (.006)
Lagged bias (\hat{eta}_4)	.77 (.07)	.55 (.09)
Residual standard deviation (ô)	.014	.015

Note: Entries are coefficients from a weighted least squares regression; standard errors appear in parentheses. The dependent variable is estimated partisan bias; as in Figure 2, where this variable is displayed, positive values indicate state electoral systems which favor the Democrats and negative values indicate bias in favor of the Republicans. N=143 election years (all but the first election in our data set for all states whose electoral systems were "competitive"). The state effects, S_{tt} , are also included in these regressions but are omitted from the table because their coefficients are not of direct interest.

Immediate Effects of Redistricting on Partisan Bias. We begin by estimating the regression with bias as the dependent variable, conducted in a manner parallel to that for responsiveness. The results appear in Table 2. Many of the key results here are easier to interpret in conjunction with the graphs in Figures 6 and 7. To begin, note that Figure 6 indicates that partisan bias is rarely greater than about 8% (on both axes) in favor of either party. We believe this is because of the numerous constraints on gerrymanderers, as described in the introduction. We turn now to the effects of redistricting on bias by looking first at the baseline of the effect of lagged bias on current bias in the absence of redistricting. This is portrayed in Figure 6 as a no-redistricting line with the steep slope of $\beta_4 = .77$ and indicates that in the absence of redistricting, the level of bias tends to persist much longer than for responsiveness. Because the slope of this line is almost 1, partisan bias changes very little in the absence of redistricting. We therefore expect that whatever effect redistricting has on bias, its effect will take a long time to dissipate.

From this baseline, we can now examine the effects of redistricting. On average, redistricting makes the typical state's electoral system fairer (closer to zero bias) than it would be if redistricting had not occurred. This effect is illustrated in Figure 6 by the asterisks, circles, and triangles (indicating that bipartisan, Democratic, or Republican redistricting, respectively, occurred between the two elections), which are generally much closer to 0 on the vertical axis than the dots (election years without redistricting). This means that state electoral systems are closer to no partisan bias following redistrictings.



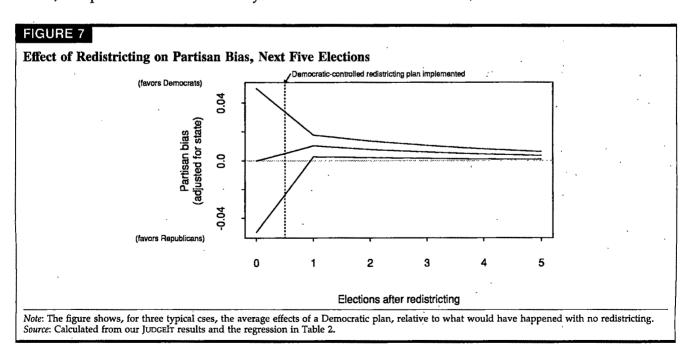
This effect is summarized by the three redistricting lines being much flatter (and closer to 0 on the vertical axis for most of the range of the horizontal axis) than the no-redistricting line. (The difference in the slopes of the redistricting and no-redistricting lines is the redistricting interaction coefficient, $\hat{\beta}_2 = -.63$.)

Thus, no matter how fair or biased the electoral system is to begin with, the typical redistricting plan, whether Democratic, Republican, or bipartisan-controlled, will produce a fairer electoral system.²¹ This

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result is consistent with evidence from individual cases in which the largest effects of redistricting change an existing huge bias in favor of one party to small bias in favor of the other.²²

The reason that any redistricting reduces bias appears to be the role that redistricting has in shaking up the political system in combination with the many constraints on the mapmakers. Shaking up a system that is effectively constrained to have partisan bias between about $\pm 8\%$ of fairness cannot have an enor-



mous effect (although much smaller effects will be quite significant to individual incumbents and political parties). Moreover, if there is already a high level of bias, due to the previous decade's redistricting (or, more likely, to demographic and mobility changes in the population over the decade), any political turmoil will have a higher probability of moving the system toward fairness since there is simply more room to move in that direction.

This powerful role redistricting has in producing fairer electoral systems does not imply that Democratic redistrictings produce the same result as Republican or bipartisan ones. To the contrary, the order of the three redistricting lines in Figure 6 (and the effect $\hat{\beta}_3$ in Table 2) indicates that Democraticcontrolled redistricting plans typically bias the electoral system toward the Democrats by about 1% (with a standard error of .5%) more than a bipartisan plan. Republican-controlled plans favor the Republicans by about the same amount. Thus, the difference between a Democratic- and Republican-controlled redistricting plan is, on average, an increase in partisan bias of about 2% for the party in control (and a corresponding decrease of about 2% for the other party).²³ The variability of individual results in Figure 6 also indicates that redistricting can have somewhat more or less powerful effects than the average results summarized by the regression lines. With fewer data, less efficient statistical methods, or less careful categorization of redistrictings by partisan control, we may not have been able to distinguish the systematic partisan effects of redistricting from the inherent variability in the system (as seemed to be the case with some existing research).

Finally, Table 2, column 2, displays the effects of redistricting on partisan bias in the hypothetical situation where no incumbents run for reelection. The partisan effect of redistricting, β_3 , is statistically (and substantively) indistinguishable from zero in this case. Since incumbents are key actors in either drawing the lines or influencing the line-drawers, this result is fully consistent with the substance of redistricting that we portrayed in the introduction. Incumbents lead the troops into most redistricting battles. That they and their vote-getting abilities would be considered as givens when drawing the district lines is just what we would expect. If, as in our counterfactual condition, many incumbents were unexpectedly defeated, the redistricting might turn out to have very different political consequences.

Thus, partisan-controlled redistricting plans produce electoral systems that favor the party in control more than the opposition party. However, the range of possible outcomes that any redistricter is able to produce, given the complicated constraints and uncertainties, is usually in the neighborhood of nearzero bias. The differences within this neighborhood are still highly significant to the partisans (as we shall further demonstrate), but the overall existence of redistricting constrains bias to within this small and comparatively fair range.

Longer-Term Effects of Redistricting on Partisan Bias. We examine the effects of redistricting on partisan bias over the next five elections with Figure 7, which is based on the regression (and the implied autoregressive time series model with interventions) in Table 2, column 1. This figure is directly analogous to Figure 5, except that the only intervention represented here is a Democratic-controlled redistricting plan. (Estimates for the effects of a Republican-controlled plan are a mirror image of these.) As can be seen clearly by the three lines converging from election year 0 to election year 1, large Republican and even Democratic biases are substantially reduced because of this redistricting. However, the Democrats still produce an electoral system biased in their favor, since all three lines are above zero. Finally, we can see that this immediate effect persists in large measure over the remaining election years before the next redis-

Effect of Redistricting on the Average District Vote

We studied the effect of redistricting on votes with the same regression model we developed for bias but using \bar{v} in place of bias as the outcome variable and also including dummy variables for each election year, to control for national swings in the vote. The results indicate that partisan redistrictings increase the proportion of votes for the candidates of the controlling party by an average of about 1% (plus or minus a standard error of .5%) as compared to a bipartisan redistricting. Because responsiveness averaged over all states and elections in our data is about 2.0, this effect on votes typically increases the seat proportion for the party controlling the redistricting by about 2% (the 1% effect on votes multiplied by the typical responsiveness of 2.0) as compared to bipartisan control. Thus, the difference in seats between a Democratic- and Republican-controlled redistricting plan is, on average, a substantial 4% of seats. The causal mechanism by which this effect works is probably as follows. A partisan redistricting produces additional districts that the party in control of redistricting is likely to win, as we have demonstrated. As a result, this party finds it easier to field better candidates, which, in turn, produces more votes for those candidates (see Cain 1985; Canon, Schousen, and Sellers 1993).²⁴

Unlike the other regression models estimated herein, the regression for votes relies very heavily on the assumption of linearity in order to establish the causal effect of redistricting. The reason is that only in the model for votes are the key causal effects (the redistricting variables) highly correlated with one of the control variables (lagged votes). That is, states with higher Democratic vote proportions in the election before redistricting are more likely to have Democratic-controlled redistricting plans implemented. The correlation with the lagged variable is much weaker in the regressions for bias and responsiveness. The dependence on the linearity assumption of our inference for the effect of redistricting on votes is

well within the standards followed throughout the social sciences where key causal variables are often highly correlated with important control variables. As such, we are confident of these estimates. However, we do not proceed with more detailed analyses of this effect (including such features as its persistence over time, as we did for responsiveness), since they would not be as certain as all other results herein, where we are in the fortunate situation of having causal inferences that meet even higher standards (and thus greater certainty) than usual.

Total Effect of Redistricting

Finally, we can add the effect of redistricting on partisan bias (seats given a fixed average district vote) to the effect of redistricting on the division of votes between the parties. This sum gives the total effect of redistricting on the division of seats in the legislature between the parties: if one party controls a redistricting plan, it can expect, on average, to receive approximately 6% of the seats that the other party would have won if it controlled the redistricting. That is, the party drawing the district lines receives, on average, about 6% more seats—and the opposition party 6% fewer seats-than if the opposition party had controlled the mapmaking. Thus, even though redistricting makes the electoral system substantially fairer overall than if there were no redistricting, the difference between Democratic and Republican control over the drawing of district maps is still one that politicians are rightfully concerned about.

We also estimated the effect of redistricting on seats directly, by regressing seats \bar{s} on redistricting type, controlling for state effects, lag seats, the lag-redistricting interaction (as usual), and election year effects. The results of this regression—an effect of 3.2% for the party controlling redistricting, with a standard error of 1.3%—are consistent with our separate analysis of votes and bias. (An effect of 3.2% for partisan redistricting corresponds to 6.4% when comparing Democratic to Republican plans.) In general, we prefer to analyze votes and bias separately, because each is an important consequence of the partisan effect of redistricting. 25 We see the direct regression of seats as a confirmation of our more important results.

CONCLUDING REMARKS ON THE BENEFITS OF REDISTRICTING

As described, our empirical results are consistent with the conflictual and uncertain process of legislative redistricting and the competing goals of redistricters. They also help resolve two important controversies in American politics over the consequences of legislative redistricting for partisan bias and electoral responsiveness. For one, our results demonstrate that contrary to all previous researchers, redistricting in state legislatures has substantially increased elec-

toral responsiveness and kept it higher than it would be otherwise for about half of all elections in each state. The effects of any one redistricting are not permanent, but the decennial redistricting process repeatedly injects the political system with a healthy dose of increased responsiveness. For partisan bias, we have identified a difference in the causal question asked by two groups of researchers, making both sides in this controversy correct to a degree. Our results indicate that partisan and bipartisan redistricting plans reduce bias overall, leading to fairer electoral systems than if there had been no redistricting, but the difference between Democratic-, bipartisan-, and Republican-controlled redistricting plans within this smaller and comparatively fairer region are still politically significant.

We now briefly organize these results from two perspectives. First, individual legislators involved in redistricting can be seen as simultaneously attempting to maximize three partly inconsistent goals: they try to increase the probability of winning or avoiding a primary, winning the general election (conditional on winning the primary), and helping their party win a majority of seats in the legislature. Those responsible for drawing the district lines, whether partisan or bipartisan, always operate in a highly constrained and uncertain environment. The final redistricting plan is usually the result of the process of achieving

consensus among incumbents and others, subject to

the formal and informal constraints; this process

usually produces a plan that weights party advantage

heavily. When incumbents give up votes in order to increase the probability of being in the majority party, responsiveness increases. It also increases when other incumbents retire to avoid the political fight altogether and due merely to changes in district lines and to wholesale increases in uncertainty. Giving up votes in this way also means that Democratic-controlled redistricting plans usually favor the Democrats more than those controlled by the Republicans. However, in order to retain their seats, they do not go too far in trying to achieve this goal. Hence, partisan bias does not favor their party as much as it could. These constraints on partisan bias during redistricting are much more substantial than during the rest of the decade, when changes in demographics, turnout, and the configuration of candidates can cause comparatively larger changes in bias and responsiveness.

A second way to organize these results is to review what they say about the benefits and costs of redistricting for states' representative democracies. The purpose of reapportionment and redistricting is to guarantee that the number of citizens in each district if roughly the same, at least at the start of each decade. Redistricting obviously accomplishes this minimal goal. However, as most political scientists recognize, population equality guarantees almost no form of fairness beyond the numerical equality of population. Even aside from issues raised by counting citizens (rather than voting-age Americans or voters), by representing ethnic minorities fairly, or

attempting to ensure that each citizen has an equal say in the policy outcome (which may be impossible to achieve, given such internal legislative rules as seniority on committees), there are the questions of what redistricting does intentionally or unintentionally to the features of our representative democracy that we have discussed here. Allowing state legislators to redistrict opens up the possibility of partisan gerrymanders, incumbent protection plans, and other apparently insidious consequences of the simple task of drawing district lines around equal-sized groups of Americans.

The vast majority of American political scientists have adopted the normative position that healthy representative democracies have low levels of partisan bias and high levels of electoral responsiveness. Our empirical results should make those who support this dominant position yearn for the next redistricting period. The political turmoil created by legislative redistricting creates political renewal. Many of the goals sought by proponents of term limitations are solved by legislative redistricting. Even the reputation of the "egregious" partisan gerrymander has been somewhat rehabilitated: not only does redistricting perform the simple task of getting the numbers right, but redistricting has tended to reduce partisan bias and increase electoral responsiveness.

It is true that bipartisan redistricting produces as high levels of responsiveness and lower levels of partisan bias than partisan-controlled redistricting plans. Moreover, Democratic- and Republican-controlled plans have very different consequences for the parties. One can also still find specific examples of substantial partisan gerrymanders that produce much more partisan bias. These results provide good reason to support a proposal to require bipartisan control of all redistricting processes. If a legislature is incapable of forging a bipartisan agreement, then alternating, or randomly assigned, control of redistricting would also accomplish many of the same benefits. Our results demonstrate that earlier objections to this proposal based on the belief that it will usually create incumbent protection plans (and hence unresponsive electoral systems) are unfounded.

Finally, our results bear directly on the role the courts might choose in resolving partisan gerrymandering claims. The U.S. Supreme Court declared partisan gerrymandering to be justiciable in *Davis* v. Bandemer (1986), but it has not yet made clear whether the standards of fairness will be set so that a plaintiff would have a chance of meeting them. On the basis of its recent decisions (e.g., Voinovich v. Quilter 1993), it seems clear that the Supreme Court would probably prefer not to be involved in partisan redistricting matters, and our results provide them with a clear public policy justification.²⁶ Individual state redistricting plans sometimes do produce very unfair electoral systems, but on average, recent state redistrictings, even when unattended by the courts, have reduced partisan bias and increased responsiveness. Far from being a scourge on the political system in need of major reforms, legislative redistricting has invigorated American representative democracy.

APPENDIX A: ESTIMATING **RESPONSIVENESS AND BIAS**

We used the method of Gelman and King (1994), implemented in our computer program called JUDGEIT (Gelman and King 1993), to estimate the electoral responsiveness and partisan bias from individual district-level data in each state legislative election. The results of these estimations are displayed in Figures 1 and 2. (JUDGEIT and the statistical model underlying our method are described in detail in Gelman and King 1994.) Our method is a straightforward and substantially improved version of our previous method (see Gelman and King 1990; King and Gelman 1991) for defining, estimating, predicting, or evaluating under counterfactual conditions concepts such as seats-votes curves, partisan bias, electoral responsiveness, the expected or predicted vote in each district in a legislature, the probability that a given party will win the seat in each district, the proportion of incumbents or others who will lose their seats, the proportion of women or minority candidates to be elected, the incumbency advantage and other causal effects, the likely effects on the electoral system and district votes of proposed electoral reforms (e.g., term limitations, campaign spending limits, and drawing majority-minority districts), and any other properties of an electoral system that can be defined in terms of vote shares in districts. The method is based on a statistical model that can be applied to virtually any legislature with two major parties and plurality rule elections in districts.

Here we shall briefly outline how our model is applied to estimating responsiveness and partisan bias. As described, we define these quantities based on the seats-votes curve, which in turn we define as the expected proportion of seats, given the average district vote. (These definitions were first given in Gelman and King 1990 and King and Gelman 1991 and are based on King and Browning 1987 and King 1989.) A large literature on the seats-votes curve has accumulated over the past half-century, and several methods have been applied to estimate the function of seats, given votes (or, more precisely, expected seats, given votes). One approach, which uses minimal modeling assumptions, is to estimate the seatsvotes curve by a regression of aggregate results from several election years; see, e.g., Tufte 1973). This approach suffers from inefficiency (using aggregate, not district-level, vote information) and, more importantly, cannot be used to measure year-to-year changes, such as we are interested in here. The other traditional approach to estimating the seats-votes curve has been based on the model of "uniform partisan swing" (see, e.g., Butler 1951), a deterministic model that, without modification, applies to no

known electoral system.

Our method can be seen as a generalization of uniform partisan swing, with two major improvements. First, we model variability in district election outcomes beyond a uniform statewide swing, thus making the model far more realistic. Second, we include explanatory variables to improve predictions, thus harnessing the full power of regression modeling for district-level data. We also verified each of these assumptions in extensive analyses of all available data on state legislative and congressional elections. We here use past election results, uncontested status, the party of the winner in the previous election, and incumbency as explanatory variables. In addition, we treat uncontested elections in a special way (see Gelman and King 1994).

Although our model applies to any electoral system with two parties or groups of parties, we use the labels "Democratic" and "Republican" to fix ideas more clearly. We assume a legislature comprising nsingle-member districts and denote v_i as the Democratic proportion of the two-party vote in each district i and v as the set of votes for all districts (v_1, v_2, \ldots, v_n) v_n). The votes v will be predicted by k explanatory variables, which can together be written as $n \times k$ matrix, X. We model the district vote outcomes with a random components linear regression of v on X, v = $X\beta + \gamma + \epsilon$, where β is a vector of k parameters that we estimate from each state election, and γ and ϵ are two vectors of independent error terms. The variable ϵ is a traditional random error term; γ is the "random component" error term, which helps correct for the fact that the X variables do not completely describe the state of the electoral system at the start of the

campaign due to the omission of relevant variables and measurement error in the variables included. For each district i, the error terms are assigned independent normal distributions, $\gamma_i \sim N(0, \sigma_\gamma^2)$, $\epsilon_i \sim N(0, \sigma_\epsilon^2)$, with variances σ_γ^2 and σ_ϵ^2 that we estimate from several years of election data for each state.

The parameters of this model to be estimated— σ_{γ}^2 , σ_{ϵ}^2 , and β —are not usually of primary interest in evaluating electoral systems and redistricting plans (although β is in some cases of interest in evaluating causal effects). Instead, we define theoretically all the quantities of interest, including the seats—votes curve, responsiveness, and partisan bias, and then compute posterior distributions of all these quantities using Bayesian simulation. The estimates and standard errors of responsiveness and partisan bias used in this study are just the posterior mean and standard deviation of these quantities, estimated separately for each state election.

APPENDIX B: ESTIMATES OF BIAS AND RESPONSIVENESS FOR STATE ELECTIONS

The posterior mean estimates, along with redistricting information, are presented in Table B-1. Posterior standard deviations, as well as the district-level data upon which the estimates were based, are available from the Inter-University Consortium for Political and Social Research in a Class V data set under the authors' names, or from the authors.

TABI	TABLE B-1										
Estim	ates of	Bias and Res	ponsivene	ss and R	edistricting	g Inform	ation f	or State Electi	ons		
		Party			Number			Party	_		Number
State	Year	Controlling Redistricting	Respon- siveness	Partisan Bias	of Districts	State	Year	Controlling Redistricting	Respon- siveness	Partisan Bias	of Districts
AL	1974	Troubillotting	0.29	0.004	105	IA	1980		2.51	-0.003	100
AL	1978		1.05	0.002	105	IA	1982	Rep.	3.08	0.003	100
AL	1982	D	0.78	0.017	105	IA	1984	•	1.98	0.030	100
AL AL	1983 1986	Dem	1.31 1.61	0.044 0.073	101 105	IA IA	1986 1988		2.60 2.12	0.014 0.028	100 100
CA	1968		1.26	0.007	78	KS	1968		2.31	-0.041	125
CA CA	1970 1972		1.34 1.41	0.001 0.030	80 79	KS KS	1970 1972	Bipartisan	2.93 3.08	-0.049 -0.014	125 125
CA	1974	Bipartisan	1.82	0.031	80	KS	1974	Bipartisan	2.98	-0.016	125
CA CA	1976 1978		1.90 1.71	0.032 0.037	78 80	KS KS	1976 1978		3.31 2.25	-0.014 -0.002	125 125
CA	1980		1.27	0.040	79	KS	1980	Bipartisan	2.18	-0.021	125
CA	1982	Dem	1.61	0.022	78 70	KS KS	1982	-	2.33 1.35	-0.032 -0.039	125 125
CA CA	1984 1986		0.70 0.76	0.057 0.040	79 80	KS	1984 1986		1.29	-0.057	125
CA	1988		0.72	0.032	80	KS	1988		1.40	-0.038	125
CO	1968 1970		2.96 2.85	-0.021 -0.016	65 65	KY KY	1969 1971		3.46 3.63	0.005 0.009	89 78
co	1972	Rep	2.93	-0.006	64	KY	1973	Dem	1.74	0.025	100
CO	1974	• •	3.25	-0.022	65 65	KY	1975 1977		1.30 1.25	0.066 0.072	100 100
CO	1976 1978		2.48 2.56	-0.019 -0.018	65 65	KY KY	1979		1.03	0.072	100
CO	1980	_	2.35	-0.021	65	KY	1981	D	1.31	0.081	100
CO	1982 1984	Rep	2.77 1.55	-0.024 -0.047	65 65	KY KY	1984 1986	Dem	2.05 0.71	0.034 0.109	100 100
čŏ	1986		2.21	-0.047	65	KY	1988		1.82	0.070	99
ço	1988		2.13	-0.050	65 177	ME ME	1978 1980	Bipartisan	2.79 2.64	0.005 0.007	150 151
CT CT	1968 1970		2.62 2.59	0.012 0.001	177 177	ME	1982		2.62	0.011	151
CT	1972	Rep	3.25	-0.021	151	ME	1984	Bipartisan	2.49	0.011	151 151
CT CT	1974 1976		2.83 2.99	-0.025 0.016	151 151	ME ME	1986 1988		1.43 1.39	0.028 0.041	151
CT	1978		2.14	0.031	151	MA	1974	Dem	1.76	0.034	237
CT	1980	Dem	2.46 2.43	0.024 0.004	151 151	MA MA	1976 1978	Dem	1.07 1.31	0.076 0.052	236 156
CT CT	1982 1984	Dem	2.43	0.019	151	MA	1980	Dom	1.17	0.076	158
CT	1986		2.26	-0.008	151	MA	1982		0.75 1.07	0.104 0.092	160 159
CT DE	1988 1968		1.45 3.31	0.032 0.002	151 39	MA MA	1984 1986		0.87	0.097	159
DE	1970	_	3.23	-0.002	39	MA	1988		1.16	0.079	160 110
DE DE	1972	Rep	2.81 2.40	-0.007 -0.005	41 41	MI MI	1968 1970		1.46 1.37	-0.023 -0.019	110
DE	1974 1976		1.65	0.045	41	MI	1972	Dem	1.63	-0.004	110
DE	1978 1980		2.07 2.37	0.037 0.001	41 41	MI MI	1974 1976	•	1.65 1.27	0.013 0.032	110 110
DE DE	1982	Bipartisan	3.15	0.005	41	Mi	1978		0.88	0.053	110
DE	1984	•	1.72	0.048	41	MI	1980 1982	Don	1.02 1.93	0.050 0.008	110 110
DE DE	1986 1988		1.13 1.65	-0.011 -0.010	41 41	MI MI	1984	Rep	1.30	0.001	110
FL	. 1982	Dem	2.28	0.017	120	MI	1986		0.96	0.008	110
FL FL	1984 1986		1.73 1.63	0.076 0.050	120 120	MI MS	1988 1979	Dem	0.85 2.16	0.020 0.001	110 121
FL	1988		1.11	0.068	120	MS	1983	Dem	2.20	0.001	119 113
HI	1982	Bipartisan	1.63 1.71	0.027 0.051	51 51	MS MO	1987 1968		2.87 2.28	0.003 0.011	113 163
HI	1984 1986		0.56	0.132	51	MO	1970		1.76	0.020	163 163
HI	1988		1.07 2.82	0.063	50	MO	1972	Bipartisan	2.73 2.00	0.004 0.026	163 162
IL IL	1982 1984	Dem	2.82 1.58	0.001 0.035	118 118	MO MO	1974 1976		1.50	0.052	162
IL	1986	20:11	0.81	0.043	118	MO	1978		1.04	0.070	162 163
IL IA	1988 1968		0.80 3.39	0.038 0.006	118 124	MO MO	1980 1982	Bipartisan	1.51 2.00	0.059 0.021	163 161
IA IA	1970	Rep	3.77	-0.015	100	MO	1984	pai iioui i	1.26	0.076	162 163
lA.	1972	Rep	3.72 2.77	-0.003 -0.007	100 100	MO MO	1986 1988		0.82 1.45	0.086 0.063	163 163
IA IA	1974 1976		2.40	0.014	100	MT	1974	Dem	3.38	0.002	100
ΙÃ	1978		3.31	0.012	100	MT	1978		2.80	0.015	100

TABL	_E B-1				.					-	
State	Year	Party Controlling Redistricting	Respon-	Partisan Bias	Number of Districts	State	Year	Party Controlling Redistricting	Respon- siveness	Partisan Bias	Number of Districts
MT	1980	-	3.84	-0.002	100 100	PA	1968 1970		2.32	-0.008 -0.006	190 196
MT MT	1982 1984	Dem	2.94 2.81	-0.006 0.002	100	PA PA	1972	Bipartisan	1.95 2.38	-0.007	197
MT	1986		1.94	-0.004	100	PA	1974 1976	· ·	2.25 1.94	-0.029 -0.016	197 192
MT NV	1988 1972	Bipartisan	2.71 2.75	-0.001 0.000	100 40	PA PA	1978		1.63	0.003	201 185
NV	1974	Bipartisan	1.80	0.036	40	PA	1980	Dinartican	1.39 1.31	-0.014 -0.028	185
NV NV	1976 1978		1.55 4.04	0.028 0.003	38 37 36	PA PA	1982 1984	Bipartisan	0.83	-0.028	203 184
NV	1980	Din autic an	2.18	0.016	36	PA	1986		0.53 0.67	-0.020 -0.003	190 189
NV NV	1982 1984	Bipartisan	2.38 2.58	0.013 -0.020	40 41	PA Ri	1988 1968		2.21	0.017	100
NV	1986		2.42	0.019	41	RI	1970		1.81 1.98	0.036 0.040	100 100
NV NM	1988 1968		1.43 3.22	0.055 0.013	42 70	RI RI	1972 1974	Dem	1.43	0.032	100
NM	1970	Dinadia	2.39	0.034	70	RI	1976	-	1.31	0.048	100 100
NM NM	1972 1974	Bipartisan	2.79 1.40	0.034 0.067	70 70	RI RI	1978 1980	• •	1.01 1.34	0.078 0.056	100
NM	1976		1.56	0.072	70 70	RI	1982	Dem	1.53 1.76	0.040	100 100
NM NM	1978 1980		2.93 1.51	0.035 0.034	70 70	RI RI	1984 1986	•	1.76	0.072 0.061	100
NM	1982	Dem	2.59	0.002	70 70	l RI	1988	Daw.	1.32	0.065	100
NM NM	1984 1986	Dem	2.77 1.82	0.025 0.045	70 70	SC	1974 1976	Dem	1.59 0.84	0.015 0.043	124 124
NM	1988	•	1.64	0.059	70 70	SC	1978		1.20	0.050	124
NY NY	1968 1970		1.61 1.10	-0.026 -0.050	150 149	SC SC SC	1980 1982	Dem	2.09 1.37	0.041 0.041	124 123
NY	1972	Rep	1.68	-0.052	150	ŠČ	1984	,	2.63	0.072	124
NY NY	1974 1976		2.09 1.60	-0.052 -0.022	150 150	SC SC	1986 1988	•	1.81 1.65	0.072 0.076	123
NY	1978		1.30	-0.015	150	TN	1972	Dem	2.96	0.010	99
NY NY	1980 1982	Bipartisan	0.72 1.07	0.015 0.041	150 150	TN	1974 1976	Bipartisan	2.34 1.76	0.009 0.040	97
NY	1984	,p-u	0.87	0.053	150	TN	1976 1978		1.56	0.066	98
NY NY	1986 1988		0.57 0.52	0.066 0.067	148 150	TN	1980 1982		1.50 1.66	0.055 0.042	98 98
OH	1968	•	2.19	-0.057	99	TN	1984	Bipartisan	2.77	0.021	124 123 123 99 97 94 98 98 99 99 69 68 75 75
OH	1970 1972	Dem	2.32 2.57	-0.070 0.009	99 99 99 99 99 99	TN	1986 1988		1.11 1.49	0.069 0.051	99 99
OH	1974		1.51	0.025	99	l UT	1968	•	2.83	0.026	69
OH OH	1976 1978		0.89 1.11	0.063 0.051	99 99	UT	1970 1972	Bipartisan	3.24 3.08	-0.019 -0.005	68 75
OH	1980	5	1.10	0.057	99	UT	1974		2.41	0.015	75
OH OH	1982 1984	Dem	2.11 0.95	0,012 0.063	99 99	UT	1976 1978	.*	2.4 4 2.88	0.012 0.005	75 75
OH	1986		0.76	0.058	99	UT	1980		2.19 2.32	-0.034	75 75
OK	1988 1968		0.51 1.87	0.072 0.050	99 99 99	UT	1982 1984	Rep	2.32 1.75	-0.022 -0.036	75 75
OK	1970	_	0.83 2.82	0.096	99	i ut	1986		2.93	-0.060	75 75 75 99 98 99 98
OK OK	1972 1974	Dem	2.82 1.51	D.017 D.067	101 101	UT VA	1988 1983		2.62 2.34	-0.024 0.033	75 99
Ŏĸ	1976		1.51 1.00 1.50 3.30	0.091 0.079	101	VA	1985 1987 1989 1968		1.11	0.080	98
OK OK OK	1978 1980		1.50 3.30	0.079 0.035	101 101	VA VA	1987 1989		3.66 3.29	0.080 0.021 0.023	9 9 98
ŏĸ	1982	Dem	2.29 1.76	0.031	101	WI	1968		2.21	-0.014	100
IOK	1984 1986	•	1.76 1.80	0.099 0.055	101 101	WI	1970 1972	Bipartisan	2.21 2.19 1.99	-0.013 0.019	100 99
OK OK	1988		1.80 2.32	0.042	101	WI	1974	- Dipui iidai i	1.97	0.021	. 99
OR OR	1972 1974	Rep	3.00 3.12 2.31	-0.007 0.009	60 60	WI	1976 1978 1980		1.25 1.79	0.047 0.043	99 99
OR	1976	,	2.31	0.030	60	Wi	1980		1.17	0.058	97
OR OR	1978 1980		2.33	0.024 0.010	60 60	WI	1982 1984	Bipartisan Dem	2.78 1.80	-0.015 0.021	99 99
OR	1982	Rep	2.53 2.13	0.017 0.008	60	WI	1986 1988		1.09	0.011	100 100 99 99 99 99 97 99 99
OR OR	1984 1988		2.85 2.86	0.008 0.017	60 60	WI	1988		1.18	0.017	99
	.500		2.00	0.017		1					

Note: Electoral responsiveness and partisan bias are estimates computed using the Judgelt program, as outlined in Appendix A. Responsiveness is the expected change in Democratic proportion of seats per change in average Democratic proportion of votes in a state; for example, a responsiveness of 2 means that a swing of 1% in average district vote will cause (on average) a 2% swing in seats. Bias is the expected average difference between the Democratic and Republican proportions of seats when their proportions of the vote range from .45 to .55; for example, a -2% bias roughly corresponds to the Democrats receiving 48% of the seats from 50% of the vote. In our main analysis, we only use the bias figures for states with competitive elections, as discussed in the text.

Notes

We thank Ken Benoit and Mike Ting for research assistance; Jim Alt, Bruce Cain, Normal Luttbeg, and Michael McDonald for helpful comments; and the National Science Foundation for research grant SBR-9223637. Gary King also thanks Nuffield College, Oxford University, for a visiting fellowship; the John Simon Guggenheim Memorial Foundation for a fellowship; and the National Science Foundation for research grant SBR-9321212. All data and information necessary to replicate the results in this article are available from the Inter-University Consortium for Political and Social Research in a Class V data set listed under our names. The computer program we used for this work is called JudgeIt and is available from the authors, the ICSPR, or on the Internet via "gopher" or "anonymous FTP" from haavelmo.harvard.edu.

1. We learned of this story from telephone interviews with justices, politicians, and civil servants in Michigan, while collecting our data.

2. In private conversations with us, members of Congress often volunteer their strong support for increasing the size of Congress so that no state would lose a member at redistricting time. However, they also believe that such a proposal would not stand a chance of passing because it seems so self-serving.

3. For example, we observed several white Democratic legislators in one state succeed in drawing many black voters out of their districts. Because blacks vote overwhelming for the Democrats, this action reduced these legislators' likely general election vote margins, if they got to the general election. However, since voters often prefer to be represented by others from their ethnic group, these new district lines may have increased these white legislators' probabilities of winning or avoiding a primary election.

4. During the redistricting process in one state, we spoke to an incumbent whose new district would have probably given him about 75% of the vote, almost exactly what he had before the redistricting. Yet he went to great extents to oppose the new plan because the opposition party, which controlled the redistricting, had, as he said, "ruined his life." It previously took him under an hour to drive to anywhere in his district, but the new district would be stretched almost all the way across the state. In addition, all four of his district offices and even his childrens' schools were drawn out of the district.

5. Even trying to improve their party's chance of winning a majority of seats is in the narrow self-interest of incumbent legislators, since majority party members in most states have more staff, are chairs of committees, gain additional visibility, and are better able to accomplish their policy goals and satisfy their constituents. As one legislator explained it to us, being in the majority party is also "a lot more fun."

6. We have applied the methods described herein to other measures of electoral competitiveness and found only trivial substantive differences from the results reported in the text

using our responsiveness measure.

7. We have checked a sample of these ICPSR data with our own data coded directly from the blue books published by several state governments. We found a few errors (and reported them to the ICPSR), but overall these data are remarkably clean and far more reliable than, for example, the ICPSR collections of U.S. House and Senate data.

8. We do not distinguish between bipartisan and nonpartisan redistricting plans. In most states, it is difficult or impossible to do so. Some previous analyses also used a separate category for court-imposed plans, but many courts (especially state courts) are widely known to be very partisan.

9. Errors in this variable, if any, are almost certainly unrelated to other variables in our analysis.

10. We studied the effects of differential turnout across districts on our estimates by repeating all analyses after substituting the statewide vote for the two parties (i.e., the total votes for the Democratic candidates cast statewide as a proportion of major party votes) in place of the average district vote, \bar{v} . (For a discussion of the role of turnout in these two definitions of statewide vote, see Ansolabehere, Brady,

and Fiorina 1988.) Although the levels and patterns of responsiveness and bias did change in some states, the effects of redistricting on these quantities were not materially different from those based on v. That is, in the ensuing analyses, Figures 1 and 2 changed in some ways, but Figures 4-7 (and Tables 1 and 2) changed in only substantively trivial ways.

11. Using average electoral responsiveness measured at \bar{v} between .45 and .55 (see Gelman and King 1994) gives very similar results when applied to the present analysis.

12. We also analyzed responsiveness for these subsets of data and found no relevant changes.

13. To maintain comparability, each point on the figure is averaged over only those 17 states that are in our data set for all 20 years.

14. Subtracting the mean from the dependent variable does not affect any substantive results, but it allows the coefficients of redistricting to be interpreted as effects for the average case.

15. The specific codes we use to represent the dummy variables make interpretation convenient but are not otherwise required or consequential.

16. To improve the estimation, we use weighted least squares with the estimated standard error of each observation (our estimated value for responsiveness), pooled within each state, to compute the weight. This is a standard procedure in general and in this literature (see King 1991; King and Browning 1987)

17. The random-effects model altered the regression results in three ways: (1) most of the standard errors decreased slightly, (2) the estimated state effects decreased, and (3) the estimated coefficients of the lagged variables increased. All these changes could be considered as improvements in the model fit, and all are consistent with the theory of randomeffects models. However, we used the simpler linear regression model, because the estimated causal effects of interest did not change materially.

18. Another way to interpret equation 1 is by noting that the effect of R_{it} (redistricting) on Y_{it} (responsiveness or bias) is $(\beta_1 + \beta_2 Y_{i,t-1})$, or, equivalently, the effect of $Y_{i,t-1}$ (last election's responsiveness or bias) on Y_{it} is $(\beta_4 + \beta_2 R_{it})$.

19. Equation 1 is a form of an AR(1) time series model. Conditional on the first lag, which is estimated, the model implies a geometric decline in the coefficients for subsequent lags. To evaluate this assumption, we estimated separate regressions with lags 1, 2, 3, and 4 of redistricting, omitting cases with intervening redistrictings. In all cases, we could not reject the assumption that the correlations followed the AR(1) pattern. For example, the theoretical and estimated lag coefficients for responsiveness are as follows:

Lags	Theoretical	Estimated (s.e.)
1	.40	.40 (.06)
2	.16	.15 (.08)
3	.06	.07 (.14)
4	.03	.22 (.33)

They decline as expected, consistent with the theoretical values based on the lag 1 coefficient of .40. (Note that the estimated coefficients at lags 3 and 4 are smaller than their standard errors.) Analysis of partisan bias gave similar results.

20. In the vast majority of states five elections, at most, are held under any redistricting plan. Since the estimated difference between the effects of bipartisan and partisan plans on responsiveness is small, we omit an analogous figures for partisan plans; it looks very similar to Figure 5.

21. Another way to look at this is with descriptive statistics: the average absolute value of bias in years following redistricting was .016, compared to an average absolute value of

.028 in nonredistricting years.

22. The biggest example of this-indeed, the largest effect of redistricting ever noted in the academic literature to our knowledge—is the Ohio State Legislature in 1972. See Figure 2 and the more detailed analyses in Gelman and King 1990.

23. We find no evidence of a difference between the aver-

age absolute size of an effect created by Democratic and Republican mapmakers.

24. We find little evidence that redistricting plans typically

have an effect by selective placement of nonvoters.

25. In addition, as noted, the causal inference for bias is especially reliable because lagged bias, unlike lagged seats and votes, does not correlate strongly with the type of redistricting.

26. The Supreme Court wrote in Voinovich v. Quilter (1993):

Time and again we have emphasized that "reapportionment is primarily the duty and responsibility of the State through its legislature or other body, rather than of a federal court." Growe v. Emison, 507 U.S. (1993), supra, (quoting Chapman v. Meier, 420 U.S. 1, 27 [1975]). Accord, Connor v. Finch, 431 U.S. 407, 414 (1977) ("We have repeatedly emphasized that 'legislative reapportionment is primarily a matter for legislative consideration and determina-tion' "[quoting Reynolds v. Sims, 377 U.S. 533, 586 [1964]].)

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POWER AND INFLUENCE IN STATE LEGISLATIVE POLICYMAKING: THE INTERACTION OF GENDER AND POSITION IN COMMITTEE HEARING DEBATES

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here is an implicit assumption behind advocating for more minorities or women in elected office, namely, that these officials will bring a new power and influence to their underrepresented groups. However, for women, this idealized viewpoint ignores the social dynamics that subordinate women's words and actions even in "well-balanced" male and female group interactions. Using transcribed verbatim transcripts of 12 state legislative committee hearings, this research analyzes the conversational dynamics of committee members, witnesses, chairs, and sponsors. Sex differences among committee members are highly significant, even after accounting for political factors and structural features of the hearing. In addition, male and female chairs do not conduct hearings in the same way, and these differences affect the behavior of witnesses and committee members. The findings suggest that as the proportion of women increases in a legislative body, men become more verbally aggressive and controlling of the hearing. Women legislators may be seriously disadvantaged and unable to participate equally in legislative policymaking in committee hearings.

The entry of more and more women into the realm of legislative politics suggests that their L increased numbers will offer women equally increased opportunities to influence policymaking. Once a group reaches a certain size (a certain "critical mass"), then its position, power, and influence will increase dramatically. For example, scholars have theorized that through numerical gains, the negative effects of tokenism, which serve to marginalize women in an institution, will fade and that feminist women elected to office will no longer hide or subsume their agendas (Carroll 1985; Kanter 1977). Recent empirical research provides some evidence that women's proportional gains translate into policy gains, as women legislators are beginning to prioritize and pursue topics of special interest to women, such as family policies and women's rights (Reingold 1992; Saint-Germain 1989; Thomas 1989, 1991; Thomas and Welch 1991). Over time, women have moved into leadership positions, and early research in this area indicates that women appear to have a different leadership style than men (Dodson and Carroll 1991; Flammang 1985; Rosener 1990).

Women, even after their 1992 electoral gains, still remain a small minority in the U.S. Congress (10.8% in the House, 7% in the Senate). However, over the last 20 years, local- and state-level offices have proved more permeable for women. In 1993, state legislatures had an average of 20.4% women, ranging from a low of 4.3% in Kentucky to a high of 39.5% in the state of Washington (Center for the American Woman and Politics [hereafter CAWP] 1993a). Given the increasing importance of policymaking at the state level and the springboard effect and policy expertise that state legislative experience provides for many congressional members (Berkman 1993; Francis and Riddlesperger 1982), examining women's partic-

ipation in policymaking and leadership experiences at the state level provides a snapshot of their current and potential impact.

Of course, as many scholars have shown, state legislatures are not monolithic. Comparisons between one state and another are complicated by political culture, length of the session, salary compensation, staff resources, size of the chamber, number of standing committees, use of subcommittees, degree of party competition, and parliamentary rules-to name but a few important differences (Elazar 1972; Francis 1989; Hedlund 1984; Jewell 1962). Despite these complications, Jewell's (1982) comparative work on all 50 states and Francis's (1989) theoretical models of the states' committee systems provide a baseline for locating where a state fits within the U.S. federal system. But, perhaps more importantly, they have also located commonalities across the diverse set of legislative bodies. For example, Francis found that state legislators spent the majority of their time working with legislation, as opposed to constituency service or legislative oversight. Specifically, committee work and sponsorship were a legislator's main focus, with the former taking more time than the latter due to the greater number of bills that pass through committees and bill introduction deadlines that are set early in the session (Francis 1989; National Conference of State Legislators [hereafter NCSL] 1992; Straayer 1990), which have the practical effect of limiting the number of bills a legislator can prepare to sponsor.

Leadership and party-caucus power notwithstanding, the important decision-making role of standing committees (both full and subcommittees) has been recognized and tested by state and congressional scholars (for recent examples, see Evans 1991a; Francis 1989; Francis and Riddlesperger 1982; Hall 1987).

While structural and personal characteristics of an individual (e.g., leadership position, party affiliation, terms in the legislative chamber, policy goals, constituency concerns, desire to make good policy, and expertise or interest in a particular bill) have been considered important determinants of participation in committee hearings (see, esp. Hall 1987), the effect of a committee member's sex has been completely ignored. With the lack of women in Congress, it is not surprising (or necessarily disappointing) that gender is absent in these models. But even the recent work of state legislative scholars has failed to take into account the role of gender, despite a substantial presence of women in state legislatures over the last decade.

The presumption among these scholars must be that gender simply does not matter. Yet an extensive body of research on gender in a variety of disciplines (e.g., management, psychology, communication, linguistics, sociology, political science) has shown that women are not equally situated with men or adequately captured through models built upon male behavior (see, e.g., Gilligan 1982; Guy 1992; Kanter 1977; Kathlene 1989; Ragins and Sundstrom 1989). Women, whether they be 10%, 20%, or 60% of an organization, work within the larger confines of gendered institutions and socially prescribed roles (Guy and Duerst-Lahti 1992; Zimmer 1988). As Janice Yoder (1991) shows, simply increasing women's presence in the workplace to combat the negative effects of tokenism and bring about gender equality ignores the pervasive sexism in society. Yoder reports that studies on tokenism have found that token men do not experience the same negative consequences as token women (i.e., sexism, rather then group size, produces inequities) and that highly masculinized occupations become more, not less, resistant to rapidly increasing numbers of women, a phenomenon Yoder labels the "intrusiveness" effect.

Yoder's assessment in itself has serious implications for women in politics generally and legislative committee studies specifically. Few social and occupational domains are more masculinized than politics. One does not need a thorough review of American history to recognize that elected offices have belonged almost solely to men (especially at the state and national levels) until just very recently—the last 20 or so years. While on one hand we may bemoan that women are making slow (but steady) increases in state legislatures (less than a 1% gain per year), women's pace, historically understood, could be classified as rapid and "intrusive" in a highly masculinized institution (women's presence in state legislatures has increased fivefold since 1969; see CAWP 1993b). If individual power and influence over policymaking occurs mainly in committees (Francis and Riddlesperger 1982; Hall 1987), women may face additional social barriers not considered within existing models of legislative committee behavior.

In the legislative setting, individuals (and therefore groups) obtain influence primarily through two methods: (1) appointments to powerful positions and (2) assignments to and participation in committees.

Theoretically, in a gender-neutral institution, all committee chairs have the power to set the agenda and guide committee discussions. If women have more of an interest in certain types of issues, as research on their legislative priorities and sponsorship of bills indicates (Saint-Germain 1989; Thomas and Welch 1991), then, in an equitable setting, female committee chairs would have opportunities to imprint their concerns while directing hearings. Similarly, if committees are composed of some critical mass of women, then the effects of tokenism and marginalization should be eliminated, allowing women to join freely in the debate on bills. Yet Yoder's work suggests that women chairs will not receive the same respect or have the same influence over committee hearings as men; and the more feminized a committee becomes, the more overt hostility will be directed at women by men.

But it is even more complicated than Yoder's work implies. Other research has found that women do not use or perceive their positions of power like their male counterparts (Blair and Stanley 1991; Dodson and Carroll 1991; Duerst-Lahti and Johnson 1990). Eagly and Johnson's (1990) metaanalysis of experimental and organization research on gender and leadership style found that women tended to lead more democratically, while men tended to be more autocratic. Similar results were found in a legislative setting, where women chairs were more likely to use their position as a facilitator or moderator of committee discussion, rather than as a way to control witness testimony, direct committee discussion, and join in the substantive debates (Kathlene 1990, 1991). Yet it is the latter approaches that are associated with the notion of positional power. That women do not use the position in the same way brings up two important questions: (1) Will gaining access to "powerful" positions result in truly equal influence? and (2) If women use their positions differently, is this due to men's negative reaction to women in power or women's freely chosen redefinition of power?

Finally, with regard to rank-and-file committee members, theoretical and empirical research previously discussed strongly indicates that women and men will not be "equal" participants in group discussions. Therefore, electing more women to legislatures will not automatically change the power balance or the influence in policymaking. Gender, as described and applied in this literature, is a complex and interacting construct representing struggles over the use and definition of power, methods of managing conflict and building consensus, paths toward implementing change, and resistance by supporters of the status quo. Gender, like class, is a fundamental category of political analysis, not simply another independent variable representing biological sex (Silverberg 1990).

This research attempts to discover where the barriers—and when the imbalances—occur in one important decision-making setting: the committee hearing. While it is possible that men always dominate verbal interactions in mixed-sex groups, other re-

search indicates that the dynamics of dominance are more complex. Such factors as the positional power of speakers plays an important role in gendered verbal behavior, and it appears that there may be important power and influence differences depending on the topic (or issue) being discussed (Craig and Pitts 1990; Kathlene 1991). Documenting precisely whether and when women are effectively kept out of committee hearing debates and under what circumstances women are successful is important for understanding how the status quo resists change despite numerical gains in elected women representatives.

RESEARCH DESIGN AND METHODOLOGY

All the data were collected from the 1989 Colorado State House during the regular legislative session. Although it was not a full-time legislature, Berkman (1993) ranked the Colorado legislature during the mid-1980s as "more professionalized" (thirteenth of 50) based on its relatively long annual session, legislators' compensation, staff size, and control over federal grants (see also Francis 1989; Jewell 1982).

Four institutional features made Colorado a good choice for examining legislators' verbal interactions in committee hearings. First, while Colorado tends to lean Republican, it has a competitive two-party system thereby making partisan comparisons meaningful (Francis 1989; Jewell 1962). In 1989, Republicans controlled both legislative chambers, 60% of the House, and 69% of the Senate, while the governor's office was Democratic. Second, legislative power is not monopolized; rather, it is shared among the majority party caucus, leadership, and standing committees (Francis 1989). However, Francis also found that legislators in Colorado both preferred and believed that standing committees were the locus of decision making for most bills, with the majority party caucus only taking an active role on bills that reached the House floor. And as of the 1989 session, leadership and caucus power were significantly decreased by the citizens of Colorado through the passage of an amendment the previous November. Dubbed GAVEL for "Give A Vote to Every Legislator," the amendment prevented (1) pocket vetoes by the speaker and committee chairs and (2) binding caucus votes. GAVEL lessened the chair's control to set the committee agenda by forcing all bills through the committee process (Lorch 1991; Straayer 1990). Building coalitions and guiding committee and witness discussions were the main avenues of control left for committee chairs. Theoretically, chairs have the ability effectively to kill bills even since GAVEL by scheduling them late in the session. However, in 1989, only a handful of bills died due to expired deadlines. Relatedly, parliamentary rules regarding speaking behavior (such as time limits, turn limits, or seniority privilege) did not exist for committee hearings.2

Rather, each chair determined the degree of formality that would be followed (Lorch 1991).

Third, Colorado's relatively small legislative body (n = 65 in the House), the restriction on the number of bills a legislator can introduce in a session, and bill introduction deadlines has helped thwart the pressure to create subcommittees (Francis and Riddlesperger 1982; NCSL 1992; Straayer 1990), thereby increasing the importance of standing committees in policymaking. Fourth, in 1989, Colorado House representatives were assigned to an average of 1.72 standing committees, well within state legislators' "satisfaction threshold" (n \leq 3) for the optimal number of committee assignments (Francis 1989). With fewer committee assignments, individual legislators are more likely to have studied the bills assigned to their committee, conversed with their committee colleagues about the legislation, and attended most of their committee's hearings; that is, legislators' scheduling conflicts are reduced with fewer committee assignments (see Francis and Riddlesperger 1982). Under these conditions, rank-and-file committee members should be more active in committee policymaking and less reliant on the chair.

All these conditions point to the importance of standing committees in Colorado state policymaking. Indeed, in the House during the 1989 session, first committee hearings proved to be crucial to the final outcome of the bill. Nearly three-quarters (n=196) of all House bills that passed out of the first committee (n=274) eventually became law. Similarly, of the 164 House bills that were killed in 1989, over 50% (n=86) died in the first House committee hearing, with the remaining bills slowly dropping out at each subsequent stage of the process.³

The Colorado legislature has long been among the states with the highest proportion of elected women. During 1989, Colorado ranked fifth among the 50 states in the number of women elected to state government, with 33% (n = 22 out of 65) seated in the House and 20% (n = 7 out of 35) in the Senate (CAWP 1989). For testing gender differences in participation rates at committee hearings, the House committees provided a variety of sex compositions. The percentage of women assigned to standing committees ranged from a low of 9% on the powerful State Affairs Committee to a high of 55% on the Education Committee. Two of the 10 committees of reference were chaired by females; 6 committees had female vicechairs. Both committees chaired by females also had a female vice-chair. All committees were proportionally representative of the legislature's party distribution, with the committee chairs and vice-chairs selected from the majority Republican party.

Sample Selection of Hearings

Twelve committee hearings were selected from the 68 taped and transcribed hearings gathered. I was interested in selecting a group of bills that were likely to bring out women's voices. All eight family/children bills that were sampled during the 1989 session were

selected for this analysis based on the presumption that women were likely to engage in discussions about family and children issues since other research has found that women are more likely to prioritize and sponsor these issues (Saint-Germain 1989; Thomas 1994; Thomas and Welch 1991). Four other nonfamily bills selected on the basis of hearing and sponsor characteristics comparable to the family/children bills (i.e., sponsor's sex, chair's sex, the proportion of women on the committee, and the first committee assigned to the bill) provide for a comparison between family and nonfamily bills.

Half of the bills were sponsored by a female; 5 of the 12 hearings were chaired by a female. It was not unusual for the vice-chair to direct the hearing or to cochair the hearing. Four of the selected hearings were chaired by more than one person. In these hearings, it was the male chair (rather than the female vice-chair) who predominated over most of the hearing (80% or more); therefore, these hearings are coded as male-chaired. The percentage of female legislators at the hearing, including the chair and the sponsor, ranged from 12% to 64%. Eight of the 12 bills passed out of their first committee (the hearing under study in this analysis). Ultimately 5 of the 12

bills became law. Table 1 lists the characteristics for each of the hearings analyzed.

Descriptive Statistics of Sample

The 12 hearings represent a total of 13.2 hours of committee discussion, with a mean length of 66.2 minutes, ranging from 26 to 138 minutes. The 12 hearings have a total of 204 speakers acting in one of four positions during the hearing (e.g., sponsor, chair, committee member, witness). Sixty-five percent of the House members (42 out of 65) were represented in one or more of the six hearings. Thirty-three percent of the selected legislators were in their first term, with 2.5 being the average number of terms in office, and there were no sex differences. The average age of the legislators was 47.0 years, but this differed significantly by sex (t = 3.69, p = .000), with men having a younger average age (mean = 44.2 years) than women (mean = 50.7 years). Sixty-five percent were Republicans (committee members and chairs combined), representing the party distribution in 1989. Two-thirds of the sponsors in this sample of bills were Republicans, which is similar to the sponsorship by party for all bills introduced in the 1989

									
TABLE 1									
Committee Descriptions and Legislative Outcomes of Selected Bills $(N = 12)$									
	ISSUE	ASSIGNED	SEX OF	SEX OF	PERCENT				
BILL AND TITLE	AREA	COMMITTEE	SPONSOR	CHAIR	FEMALE ^a	OUTCOME ^b			
Family									
1066 Disabled children									
home care program	Family	HEWI®	female	female	12	passed/law			
1089 Medical benefits for				_					
children	Family	HEWI	female	male	36	passed/law			
1193 Encourage		1 1999 4 21				umid			
opportunity for child care	Family	HEWI	female	male	25	passed/Pl ^d			
1344 Concerning custody	Camilla	ludiaine.	female	female	55	Pl			
proceedings 1071 Domestic abuse	Family	Judiciary	remale	remaie	99	PI			
restraining orders	Family	Education	male	female	56	passed/law			
1234 Enforcement of	1 carring	Laucation	male	iemaie	30	passediaw			
support obligations	Family	Judiciary	male	male	44	passed/law			
1269 Child care expenses	1 army	oddiolai y	maio	111010		passourian			
and income tax	Family	Finance	male	male	36	tabled			
1339 Reporting child abuse	Family	HEWI	male	male	27	Pl			
Other	•								
1105 School dropout									
prevention act	Education	Education	male	female	64	passed/PI			
1118 Adult high school	Ladoution	Eddodion	maio	Torridio	04	paoceani			
diplomas	Education	Education	female	female	56	passed/lost			
1263 Reparation payments						1			
and social services	S.Services	Finance	male	male	36	passed/law			
1309 Superfund						•			
implementation	Environment	HEWI	female	male	36	PI			

[&]quot;The percent female at the hearing includes all legislators (committee members, sponsor, chair). The variation shown within the same committee reflects one or more of the following changeable conditions: (1) one or more legislators assigned to the committee did not attend the hearing, (2) the sponsor could be a man or a woman, and (3) the chair of a committee could be either a man or a woman, since chairs and vice-chairs directed hearings.

*Outcome of bill in committee hearing in study/Final outcome of bill in the 1989 legislative session.

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Health, Environment, Welfare, and Institutions.

Postponed indefinitely.

House. Overall, the sample is representative of both the bills and the legislators across many important characteristics.

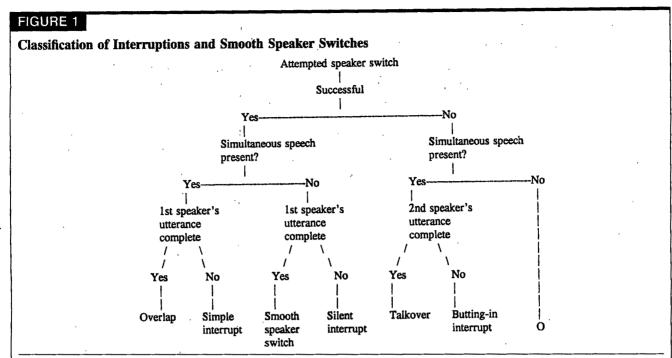
Dependent Variables

To determine "who holds the floor" in legislative committee hearings, five dependent variables were tested. The first measure, percent TIME ELAPSED, is the percentage of the hearing that has elapsed when the speaker first talks,5 calculated by dividing the number of words spoken at the hearing prior to a speaker's first utterance by the total number of words spoken at the hearing. The point in the hearing when a speaker enters the committee discussion may be an important measure of influence. Earlier comments and/or questions may direct the other committee members to consider the bill based on the concerns of early speakers. In addition, speaking up early in the hearing may influence subsequent witnesses to modify their testimony in order to address the apparent concerns of the committee at the possible expense of emphasizing other important points. Of course, amendments created in committee (as opposed to sponsor amendments brought to the committee to address perceived deficiencies in the drafted bill)6 result from discussions in the hearing so that early engagement in committee discussion is likely to both guide further discussions and result in a permanent change of the proposed legislation in the form of amendments.

The second two dependent variables, words spo-KEN and TURNS TAKEN, are discrepancy score measures that subtract the expected frequency (the sum of the speaking behavior divided by the number of people present at the hearing) from the actual frequency of a particular speaking behavior. The discrepancy score standardizes for differences in the length of the hearing time to make interhearing speaking behavior comparable. Words spoken measures the time a speaker holds the floor. This is an indicator of how vocal a person was during the hearing. Research outside of political settings has found that men talk more than women, whether in mixed- or same-sex groups (Wood 1966). Other studies have documented that males are the more talkative in mixed-sex conversations in the workplace, classroom, and in couple situations (see, e.g., Bernard 1972; Craig and Pitts 1990; Fishman 1978; Swacker 1975; Swann 1988; Woods 1988), and that males talk longer in monologue-type situations (Edelsky 1981). Turns taken measures how often speakers initiated and engaged in discussion. This is an indicator of how actively involved a person was in the hearing.7 The interpretation of activity will vary depending upon the positional role of the speakers. When committee members take many turns, it reflects their self-initiation. However, when witnesses take many turns, it reflects the engagement of the legislators (chair, committee members, and, to a lesser extent, the sponsor) through questioning the witness.

The final two dependent variables, percent INTER-RUPTIONS MADE and percent INTERRUPTIONS RECEIVED, measure the different ways committee members gain and lose their speaking turns. Most turns, even in less formal settings than a committee hearing, are gained through "smooth speaker switches," where a person waits until the speaker has completed their utterance. How a person gains their speaking turn is an important indicator of both social norms (i.e., politeness) and power or dominance. Numerous linguistic and sociological studies of mixed-sex group dynamics document the male's verbal aggressiveness. For example, one study found that males interrupting females accounted for 96% of all interruptions (Zimmerman and West 1975). Another study found that men were more likely to talk while a woman was speaking than while a man was speaking (Willis and Williams 1976), though the context and situation of the verbal interaction are important determinants that can reverse the pattern (Murray and Covelli 1988). Interrupting another person reflects the speaker's position of power and dominance, whether it be social or institutionally derived (West and Zimmerman 1977). In committee hearings, there are potentially two conflicting conditions regarding power and dominance. Research on sex differences suggests that men will do a disproportionate amount of interrupting and that women will be interrupted more often than men. But positional power (e.g., chairing the hearing) should mitigate the socially constructed power differences between the sexes. Women chairing committees should not act differently from their male counterparts—or at least, they should be more dominant (i.e., do more interrupting) than either rank-and-file committee members or witnesses (see Kollock, Blumstein, and Schwartz 1985).

An interruption can be successful or unsuccessful. Following Craig and Pitts' (1990) coding scheme, five types of interruptions were identified. Figure 1 diagrams how speaker switches were coded. The five dependent variables examine verbal behavior of legislators and witnesses in committee hearings. While the measures are naturally related to each other (e.g., we would expect that speaking early in the hearing would be positively correlated with speaking more in the hearing), it is the variation among the independent variables that expose the conversational dynamics of a hearing. For example, based on literature previously discussed, men and women may take an equal number of turns, but men may talk longer than women in any given turn. Similarly, interruptions will be related to how much a person talks simply because there is more time to interrupt the person, yet we would expect chairs to make more—but receive less—interruptions by virtue of their position of control. Most of the dependent variables used in this analysis are only moderately correlated (.25 to .50), and all covary in the expected direction. (See Appendix Table A-1.)



Source: Craig and Pitts 1990, 129, based on G. W. Beattie. 1983. Talk: An Analysis of Speech and Non-Verbal Behavior in Conversation. Milton Keynes: Open University Press.

Note: Only smooth speaker switches represent taking a turn without interrupting the speaker. Both interruption variables were created by summing together all successful and unsuccessful interruptions (Overlap + Simple Interrupt + Silent Interrupt + Talkover + Butting in) that a speaker made or received. The individual sums were then divided by the total number of turns taken in the hearing to produce percentage scores. While there are theoretically important differences in the type of interruption made, there are too few observations in the data set to analyze the five types separately.

RESULTS

Descriptive Statistics

A total of 116,051 words were spoken with a mean of 9,671 words per hearing. Across all 12 hearings there were 1,747 smooth speakers switches and 195 interruptions. Total interruptions ranged between 4.0% to 26.0% of all speaker switches, with an average of 11.2%. The interruption rate is low in comparison to research findings in conversational dynamics (see Craig and Pitts 1990). No doubt this was due to the formality of the setting. Parliamentary procedures both produce more speaker switches (the chair constantly acknowledges each speaker-55.1 percent of all turns were taken by chairs) and lessen the likelihood of interruptions (speakers generally wait to be acknowledged by the chair). Similar to findings in conversational analysis, most interruptions during the hearings resulted in a successful turn (86.2%) for the person who did the interrupting, further validating the importance of studying who interrupts whom.

Positional Role

Position at the hearing is an important determinate of speaking behavior. Three multivariate regression models of the five dependent variables that demonstrate the power of positional role and speaking behavior are shown in Table 2. In the first model, chairs, committee members, and witnesses all differed significantly from sponsors on the five dependent

dent measures. Chairs took the most turns speaking, while sponsors dominated hearings in speaking time. Sponsors and chairs both made and received more interruptions than committee members or witnesses. The significant differences in elapsed time among the roles are an institutional artifact: sponsors always had the opportunity to summarize their bill verbally at the start of committee hearings; chairs spoke next by virtue of their role as moderator of the hearing; witnesses and committee members were the last to enter into the committee discussion.

The second model adds sex to the equation. The positional patterns found in the first model remain essentially the same. In addition, except for interruptions received, men and women at the hearing also differed significantly from one another. Women entered the discussion later, spoke less, took fewer turns, and made fewer interruptions than men.

The third model includes interaction variables of sex by position. Overall, position continues to distinguish among different speaking behaviors, and female chairs are significantly different than male chairs. Women chairing committees spoke less, took fewer turns, and made fewer interruptions than their male counterparts, suggesting that men and women have different leadership styles. The same gendered pattern holds for female committee members and female witnesses in comparison to their male counterparts, although these differences are not statistically significant.

Since positional role is highly significant, in statis-

TABLE 2

MODELS AND		UNSTANDARD	IZED REGRES	SION COEFFICIE	NTS
INDEPENDENT VARIABLES ^a	% TIME ELAPSED ^b	WORDS SPOKEN°	TURNS TAKEN ^e	INTERRUPTS MADE (%) ^d	INTERRUPTS RECEIVED (%) ^d
Model 1					
Chair	.19**	-1791.28****	17.50****	.02	02
Comm. member	.54***	-2309.43****	-20.93****	20****	20****
Witness	.49****	-1749.49****	-19.95****	20****	20****
Constant	.00	1928.65****	15.92****	.23****	.24***
R ²	.25	.46	.61	.49	.38
Model 2					
Chair	.19**	-1785.30****	17.58****	.02	02
Comm. member	.54***	-2326.70****	-21.16****	21****	20****
Witness	.49***	-1759.81****	-20.09****	20****	20****
Female	.11****	-203.43****	-2.70***	04***	02
Constant	05	2030.36	17.27****	.25****	.25****
R ²	.28	.47	.62	.52	.39
Model 3					
Main effects					
Chair	.13	-1418.50****	24.29****	.12****	04
Comm. member	.46****	-2209.07****	-19.83****	18*** [*]	21****
Witness	.47***	-1619.52****	-19.71****	18****	22****
Female	.00	69.53	06	.02	04
Interactions					
Fem. chair	.12	-708.00*	12.82**	−.18*** *	.04
Fem. comm. member	.18	-227.56	-2.66	05	.01
Fem. witness	.04	-281.43	56	04	.03
Constant	.00	1893.88	15.95	.23	.25
R ²	.29	.48	.63	.55	.39

"Omitted category is the sponsor of the bill.

*Percent time elapsed measures the percentage of the hearing that has elapsed when the speaker first talks. Smaller percentages indicate early engagement in committee discussion.

Words spoken and turns taken are discrepancy scores computed as expected frequency minus actual frequency. Negative numbers reflect less activity—positive numbers, more activity—than average. Words spoken measures the time a speaker holds the floor, turns taken, how often a speaker initiates and engages in discussion.

*Interruptions made and interruptions received are calculated by summing together the number of all the types of interruptions a speaker made (or received) divided by the total number of turns in the hearing. See Figure 1 for a diagram and description of the coding scheme.

 $p \le .15$. $p \le .10$.

 $p \le .05$. $p \le .05$.

tical terms as well as substantive terms, the analysis will compare speaking behavior within each positional role across all 12 hearings.

Committee Members

Personal and Institutional Factors. A look at which group of legislators "hold the floor" in committee hearings most closely mirrors the environment found in linguistic research on conversational dynamics. While there are clear and important differences between informal groups (a group of friends, a set of couples, etc.) and the formality of a public hearing with its political constraints, the committee member plays the most "natural" role of the four types of participants studied in this research. Committee members are relatively free to engage in discussion at any point in time-unlike the witnesses who must wait for their initial turn and subsequent requests for comments. Unlike the sponsor, committee members

are not necessarily advocates of a particular bill, although they may be strongly aligned with a particular issue area. Rather than "campaigning" for the bill (as the sponsor does), the committee members' task is to examine the proposed legislation from a multitude of perspectives—personal, professional, and political—that they bring with them. In this way, committee members have the freedom to act more like individuals in a conversation than the witnesses (who are "outsiders" or merely "guests" at the legislative table), the sponsor (whose self-interest is tightly bound to the passage of the bill), or the chair (whose primary formal responsibility is to direct and coordinate the hearing).

Thomas and Welch's (1991) study of legislative activity found that men and women reported similar levels of participation in committee hearings. However, given the research discussed earlier on mixedsex conversational dynamics, we would expect gender differences between committee members, with

men dominating the hearing. Unlike an informal conversation among friends, there are several political and institutional factors that could produce differentiated patterns beyond or instead of gender. Partisan differences seem very likely. We might expect that the minority party (in this case, the Democrats) to be less vocal than the majority Republican party. Or minority party members may be particularly active in committee hearings in order to vocalize their objections to or imprint their concerns on the bill before it reaches the highly partisan dynamics of the House floor debates (Francis 1989; Hall 1987). Or there may be no discernible party differences since committees are viewed by the legislators themselves as one of the least partisan arenas in the legislature (Francis and Riddlesperger 1982).

Other factors could also be important. Hall (1987) found that Congress members' desire to make an imprint on a piece of legislation was strongly related to their participation in committee hearings. Having a personal interest or expertise in the issue area of the bill could explain differences in verbal behavior. Four measures were used to determine a legislator's interest in a bill. A legislator was considered "interested" if (1) the specific bill was mentioned in the personal interview as one of the most important in the session, (2) the issue area of the bill was mentioned among the legislator's top priorities, (3) the legislator sponsored or cosponsored a related bill during the 1989 session, or (4) the legislator's professional experience (e.g., profession, task-force work, community awards) was directly related to the issue area of the bill. Interest in the bill should increase a legislator's activity in the committee hearing.

Finally, legislative expertise may account for greater participation. The number of terms served in the legislature can be used as a general measure of legislative expertise. We would expect newly elected legislators to be inexperienced in state legislative policymaking at a number of different levels (e.g., parliamentary procedures, historical detailed knowledge about failed bills in previous years, and general insider politics) and lack the clout to be included in important off-the-record dealings with legislative leaders, state bureaucrats, and lobbyists. Because the first-term legislators in this research were in the first 60 days of their two-year term, the differences in speaking behavior between the newest members and more seasoned legislators might be very pronounced.8 Hall (1987) found that freshman status was negatively related to participation in congressional policymaking, and the same may hold true in state legislative committee hearings.

Results of Personal and Institutional Models. Table 3 reports (1) the bivariate regression results of sex on each of the dependent variables and (2) three multivariate regression models. As the bivariate model shows, sex is highly significant on all five measures of speaking behavior. Women, on average, waited until more than two-thirds of the hearing was over before they uttered their first words, while men engaged

halfway through the hearing. Men spoke longer and took more turns, and men made and received more interruptions than women committee members. The first multivariate model results indicate that sex continues to be significant across the five measures even after accounting for committee members' interest in the bill, their party affiliation, and their freshman status. In each of the measures, it was the male committee members who dominated the committee hearings.

Other variables are also important in understanding verbal behavior. As predicted, committee members who had an interest in the bill spoke longer and engaged in discussion more frequently. They were also interrupted more often, which follows from their greater verbal activity. Except for making fewer interruptions, minority party status did not affect speaking behavior in committee hearings, supporting Francis's (1989) interviews with state legislators, who reported low levels of partisan politics in committee hearings. Unlike Hall's (1987) findings on congressional representatives, freshman status did not lessen their participation in the hearings. Evans (1991a) found that newcomers to the Senate participated equally with their more senior colleagues, suggesting that there are circumstances that produce greater and lesser inequality based on tenure. Perhaps being a newcomer is more of a disadvantage in a large legislative body, such as the House of Representatives, than in a smaller chamber, such as the Colorado State House (see Evans 1991b).

Additional Committee Hearing Factors and Interactions. Based on recent research on women's legislative priorities and activities (Reingold 1992; Saint-Germain 1989; Thomas 1991; Thomas and Welch 1991), women may be more active participants in hearings that address family issues. If women have a special interest in certain types of legislation, due to their personal experience or belief that they can represent women's interests better than men (Reingold 1992), then female committee members should be more vocal in hearings on family issues.

The sex of the sponsor may have an effect on the committee members' speaking behavior. If women are bringing different views and values to policymaking based on a more needs-based, rather than individual-rights, orientation toward society (Dodson 1991; Dodson and Carroll 1991; Gilligan 1982; Kathlene 1991), then the bills that women sponsor, regardless of issue area, may embody this difference and perhaps strike a responsive cord among the other women committee members.

Chairing a committee is an important position of power because the chair can control much of the hearing dynamics, including in what order committee members will be recognized to speak, which committee members' ideas will be encouraged or cut off, and whether the bill will receive a final committee vote or be tabled (usually resulting in the death of the bill). Given findings in all-female groups that women engage in supportive and cooperative, rather than

TABLE 3 Bivariate and Multivariate Analyses of Speaking Rehavior among Comp

MODELS AND	UNSTANDARDIZED REGRESSION COEFFICIENTS							
INDEPENDENT	% TIME	WORDS	TURNS	INTERRUPTS	INTERRUPTS			
VARIABLES	ELAPSED ^a	SPOKEN ^b	TAKEN ^b	MADE (%)°	RECEIVED (%)			
Bivariate model:								
Female	.18***	-135.14***	-2.43***	04***	03***			
Constant	.46	56.64	1.00	.05	.05			
R ²	.08	.05	.05	.13	.05			
Multivariate model 1:					.00			
Female	.19****	-144,49***	-2.66***	04***	03***			
Interest	05	153.23***	2.73***	.01	.02*			
Minority party	.05	-10.53	12	02**	01			
Freshman status	.03	-63.48	-1.34	01	02			
Constant	.45	34.33	.69	05	02 .05			
R ²	.09	.11	.12	.03 .17	.10			
Multivariate model 2:	.03	.11	.12	. 1 (.10			
Speaker:								
Female	.19****	-144.54***	~2.65***	04***	04** *			
Interest	06	159.16***	2.84***	.01	.02*			
Minority party	.06	-10.88	−.13	02*	.00			
Freshman status	.02	-60.95	-1.32	01	02			
Hearing:								
Family issue	08	26.44	.44	.00	.02			
Female sponsor	~.06	-5.87	12	.00	.01			
Female chair	.11	-25.16	41	01	.01			
Female mass ^d	11**	16.12	.23	.00	.01			
Constant	.71****	-6.28	.10	.06***	.01			
R ²	.13	-0.20 .11	.12	.18	.15			
Multivariate model 3:	.10	,11	.12	.10	.10			
Speaker	00	521.55**	E 00	. 04	04			
Female	28		5.09 2.89***	04	.01			
Interest	05	163.53****		.01	.02*			
Minority party	.08	-37.04	40	02**	.00			
Freshman status	.00	-44.67	80	01	02			
Hearing								
Family	02	131.70	1.26	01	.04**			
Fem. sponsor	−.1 5 *	53.96	08	.00	.02			
Fem. chair	.10	-5.93	.57	.00	.01			
Female mass	−.18** *	93.35	1.19	.00	.02			
Interactions								
Fem. × Family	09	-239.62**	-2.11	.00	04			
Fem. × Fem. sponsor	.22*	-144.13	34	.00	02			
Fem. × Fem. chair	07	-14.13	-1.70	01	02			
Fem. × Fem. mass	.21*	-200.18*	-2,48	.00	.00			
Constant	.86****	-269.90*	-2.92	.07***	02			
R ²	.21	.18	.16	.20	.18			

^{*}Percent time elapsed measures the percentage of the hearing that has elapsed when the speaker first talks. Smaller percentages indicate early engagement

competitive, linguistic strategies (Coates 1988), female committee members may be more at ease or more inclined to speak when the chair of the committee is also female. However, other research indicates that even when a token male (or female) is present, the group dynamics change toward a less supportive interaction style (Smith-Lovin and Brody 1989). Therefore, the presence of a female chair may not be

in committee discussion.

bWords spoken and turns taken are discrepancy scores computed as expected frequency minus actual frequency. Negative numbers reflect less activity, positive numbers reflect more activity than average. Words spoken measures the time a speaker holds the floor, turns taken, how often a speaker initiates

and engages in discussion.

Conterruptions made and interruptions received are calculated by summing together the number of all the types of interruptions a speaker made (or received)

divided by the total number of turns in the hearing. See Figure 1 for a diagram and description of the coding scheme.

^aFemale mass represents the proportion of the committee hearing (committee members, sponsor, and chair) that is female. The variable was coded female tokenism = 1 (less than 30% female), female critical mass = 2 (30%-50% female), female majority = 3 (over 50% female).

 $p \le .15$. $p \le .10$. $p \le .05$. $p \le .05$.

enough to bring out female voices. Yet the more democratic leadership style of women—as opposed to men's more autocratic style—may provide the opportunity for more women to participate (see Eagly and Johnson 1990). But being democratic implies an inclusive style that should facilitate all voices, not just women's voices. Thus, men's more aggressive verbal behavior may be even more pronounced under a female chair. Based on these conflicting propositions, female committee members may participate more or less than men when the chair is female.

It may not be enough that the chair or the sponsor is female if the rest of the committee is comprised of mostly males. In order for women to be heard, they may need to comprise some minimal percentage in a committee hearing (Kanter 1977). Conversely, the increased presence of women may result in more verbal activity by men reacting to women's intrusion in the legislative setting (Yoder 1991). The 12 hearings were divided into three groups based on the percentage of women legislators at the hearing (including all the committee members, the sponsor, and the chair). Three bills comprise the female tokenism category, in which less than 30% of the legislators were women. Five bills are in the female critical mass, where 30% to 50% of the committee is comprised of women. Finally, four bills fall into the category of female majority, where, by definition, over 50% are women. If Kanter's proposition holds, female committee members will become more verbally active when more women are present. If Yoder is correct, female committee members will be less active due to men's stepped-up participation to counter the increased proportions of women at the hearing.

Results of Additional Considerations. As multivariate model 2 in Table 3 shows, after controlling for situational factors related to the hearing, the same pattern of male domination found in multivariate model 1 remains. Regardless of whether the chair or the sponsor is a man or a woman or the hearing is on a family issue, female committee members engage later, speak significantly fewer words, take significantly fewer turns, and make and receive fewer interruptions than their male counterparts. However, committee members speak significantly earlier as the proportion of women on the committee increases.

Model 3, which expands on model 2 by adding interaction terms of female committee members by each of the hearing variables, clarifies the differential effect of female presence and positional roles on male and female committee members' participation in the hearing. It is the men, rather than the women, who become significantly more vocal when women comprise greater proportions of the committee, supporting Yoder's "intrusiveness" effect. Similarly, male committee members engaged earlier—and female committee members, later—when the sponsor of the bill was a woman. An examination of the transcript reveals that male committee members asked questions of female sponsors immediately after their introduction and more male committee members en-

gaged early in questioning witnesses that testified for a female-sponsored bill, regardless of the proportion of women at the hearing. Both behaviors tended not to be present when a man sponsored a bill, suggesting that females (but not males) in positions of importance have their ideas scrutinized by rank-andfile men.

While the male committee members engage significantly earlier, they are not speaking or taking more turns under the different situational characteristics of the hearing. The same cannot be said for female committee members. Women not only engage later when there are more women at the hearing, but they also speak fewer words. And women, contrary to the expectation, speak significantly less than men at hearings addressing a family issue, even after controlling for interest in the issue area (which remains significant).

More generally, each of these models, while unable to explain a large proportion of the variance (model 3 explains between 16% and 21%), provide compelling evidence that sex is a powerful explanatory variable. Even when a particular variable is not statistically significant, the direction of the relationship is consistent with female committee members participating less under a variety of controls and interactions, while legislators with a personal or professional interest in the issue participate more.

Witnesses

An examination of witnesses' speaking behavior will help flesh out more of the committee hearing dynamics.

Factors Influencing Witness Speaking Behavior. As noted, witnesses have many constraints on their speaking behavior—most notably that they must wait for the chair to call them forward to speak. Therefore, gender differences in speaking behavior among witnesses may not reflect gender differences between the witnesses but rather differential treatment by the chair (who determines the order of testimony¹⁰ and has the power to limit testimony) and the committee members (who may ask questions of the witnesses). While partisan affiliation is not applicable (at least not determinable), age is unknown, and terms in the legislature cannot be used to measure expertise and political savvy of a witness, other possible factors may be important. Among the most important is the formal affiliation of the witness. Is the witness a lobbyist, a state bureaucrat, an "expert" in a particular field, or a citizen? To most legislators, lobbyists and bureaucrats are likely to be well known, respected, and mostly seen as reliable sources of information (Straayer 1990). However, the familiarity between these witnesses and the legislators may also bring out entrenched ideological differences that cause positive and negative reactions among the legislators. Individual lobbyists and bureaucrats should receive mixed opportunities to speak at the hearing, depending upon the composition of the

committee and the issue at hand. Experts may not be personally known by most of the legislators at the hearing, but by virtue of their professional title and experience (e.g., a doctor who specializes in high-risk obstetrics at a hearing on premature infants), experts (as a group) should be given the most opportunity to testify and answer questions. Citizens have no institutional or professional affiliation. Representing only themselves, they are likely to relay a personal story regarding aspects of the bill. Based on research findings that representatives rely on more professionalized sources of information to make policy and voting decisions, unaffiliated witnesses should be given the least opportunity to engage in the committee hearing (see Jewell 1982; Mooney 1991). There are other important characteristics of the witnesses that may cause legislators to treat them differentially. In Colorado, the sponsor of the bill orchestrates the witness testimony, especially in the first committee hearing. Speakers who come to testify at the request of a legislator should receive more opportunity to speak than witnesses who come forward on their own initiative. Relatedly, witnesses who testify in favor of the bill may be treated differently than those who are against it.

Male and female witnesses with different affiliations and roles may not be equally situated with regard to how committee members and chairs respond to them. For example, male experts may be seen as more credible than female experts. Interaction terms will disentangle these potential gendered effects. Finally, the same four hearing variables used to distinguish differences among committee members' verbal behavior are used in multivariate model 2 to test whether witnesses are treated differently under a variety of hearing circumstances related to gender. One additional hearing variable—the number of witnesses at the hearing—is included because the chair knows at the beginning of the hearing how many people have signed up to testify. Given a variety of time constraints the chair works within, the actual number of witnesses signed up for a bill may produce different speaking patterns imposed by the chair. Specifically, the more witnesses signed up, the less time each witness will be allowed to speak and the fewer questions the chair will allow committee members to ask.

Results of Witness Speaking Behavior. As Table 4 shows, the bivariate regression model that tests only sex is not significant for four of the five dependent measures. The chair does not have men and women witnesses come forward to testify earlier or later in the hearing based on their sex. While women witnesses speak less than the expected number of words and men speak more, these differences are not significant, indicting that male and female witnesses are given equal amounts of "floor time." There are no gender differences in the number of turns taken suggesting that committee members (and the chair) are as likely to ask follow-up questions from witnesses of either sex. Interruption patterns do vary by the witnesses' sex. Like their committee member counterparts, female witnesses are significantly less

likely to use interruptions to gain a speaking turn than are male witnesses. However, there are no sex differences with regard to witnesses' receiving interruptions, indicating that men and women witnesses are equally likely to be interrupted by the legislators.

Multivariate model 1, based on six witnesses' characteristics, produces several interesting patterns but sex is not significant for the same four dependent measures as found in the bivariate model. Witnesses who come forward on their own, rather than through the sponsor, speak significantly later in the hearing but speak as much—and take as many turns as—the sponsor's witnesses. As hypothesized, "experts" were given the most speaking time, as measured in words spoken and turns taken. Female witnesses continued to make significantly fewer interruptions than male witnesses after controlling for their institutional affiliation, sponsor affiliation, and position on the bill. Interestingly, witnesses who represented themselves made significantly more interruptions, indicating, perhaps, their lack of familiarity with the chair's moderator role.

Multivariate model 2, based on both interaction terms for the witnesses' characteristics and committee hearing characteristics, significantly improves the explained variance for the dependent measures. In hearings on family issues, witnesses spoke significantly earlier and made more interruptions. When the sponsor was a female, witnesses began speaking later in the hearing. This occurred for two reasons: female sponsors tended to give longer introductions to their bills (averaging 1,149 words, as compared to the 806 words for males) and, as noted previously, female sponsors received more questions from male chairs and male committee members during and after their introduction, rather than moving directly to the first witness. The latter interpretation is also borne out in the model where witnesses began speaking earlier in hearings chaired by a female (a woman chair tended to move directly to witness testimony) and were engaged later when there were more female legislators present. We saw earlier that male committee members became more verbally aggressive when more women were present and when the sponsor was a woman. A more extensive interaction model (not shown)11 found that under a female chair or when the hearing was on a family bill, it was the male witnesses who were making the interruptions, suggesting that increased verbal aggressiveness when women are in power and when women's traditional interests are being discussed is not limited to gendered dynamics among the legislators. Although the model measures witnesses' speaking behavior, these results tell us more about the interaction between witnesses and legislators and how male legislators treat female sponsors at the beginning of the hearing.

The interaction terms in multivariate model 2 produced some interesting gendered patterns that were absent in the main effects model. Overall, the direction of the relationships is consistent with the gendered pattern found among the committee members. Specifically, female witnesses who opposed a bill had

TABLE 4 Bivariate and Multivariate Analyses of Speaking Behavior among Witnesses at Hearing (N = 69)

	UNSTANDARDIZED REGRESSION COEFFICIENTS					
MODELS AND INDEPENDENT VARIABLES	% TIME ELAPSED ^a	WORDS SPOKEN ^b	TURNS TAKEN ^b	INTERRUPTS MADE (%)°	INTERRUPTS RECEIVED (%)	
Bivariate model:						
Female	.04	-144.26	-1.06	02 **	01	
Constant	.47	64.81	.46	.04***	.04***	
R^2	.01	.01	.01	.04	.01	
Multivariate model 1						
Speaker						
Female	.04	-113.74	84	03***	01	
Witness affiliation ^d						
Expert	08	305.13**	2.36**	.03	01	
Bureaucrat	03	163.86	1.68	.01	.01	
Individual	01	-223.15	-1.53 ·	.04**	.00	
Witness role						
Self-init.	.17***	-147.80	60	.01	.02	
Against	09	31.10	1.09	.01	.02	
Constant	.46	38.79	15	.03***	.03***	
R ²	.10	.11	.11	.11	.09	
Multivariate model 2						
Family	21***	195.01	.30	.04**	.02	
Fem. sponsor	.34***	-262.27	16	02	.02	
Fem. chair	37****	270.10	.76	.06**	03	
Female mass ^e	.14***	9.96	.36	.00	.03**	
No. of witnesses	09****	55.62	.13	.01	.00	
Speaker	.00	55.52	.,,	,		
Female	05	227.00	1.28	01	.00	
Witness affiliation	.00	2200	1,25		100	
Expert	14	263.21	2.56	.04**	01	
Bureaucrat	02	367.55	3.79***	.03	.04**	
Citizen	01	150.58	.37	.03	.01	
Witness role	.01	100.00	.07	.00	.01	
Self-init.	.27***	-213.63	-1.59	03	.00	
Against	13	353.40	3.71**	.03	.07***	
Interactions	.10	000.40	0.7 1	.00	.07	
Witness affiliation						
Fem. (witness) × expert	.18	263.21	-1.10	05	02	
Fem. (witness) ×	.10	200.21	1.10	.00	.02	
bureaucrat	.09	-434.20	5.31**	06**	08***	
Fem. (witness) × citizen	.09 .02	-434.20 -756.39*	-5.03	03	01	
Witness role	.02	130.38	-5.03	03	01	
Fem. (witness) × self-init.	31**	439.38	6.67**	.13****	.13****	
	31** .63****	-1260.58***	-8.81***	.13**** 14***	13****	
Fem. (witness) × against	.80****					
Constant R ²		-533.65	-2.9 8	05	.07	
Π	.37	.23	.24	.37	.34	

[&]quot;Percent time elapsed measures the percent of the hearing that has elapsed when the speaker first talks. Smaller percentages indicate early engagement in

significantly less opportunity, as measured by all the dependent variables, to participate in the hearing than male witnesses who opposed a bill. And female citizens spoke less than male citizens, who were asked more questions by committee members as evidenced in the greater number of turns male wit-

committee discussion.

bWords spoken and turns taken are discrepancy scores computed as expected frequency minus actual frequency. Negative numbers reflect less activity—positive numbers, more activity—than average. Words spoken measures the time a speaker holds the floor, turns taken, how often a speaker imitates and engages in discussion.

[&]quot;Interrupts made and interrupts received are calculated by summing together the number of all the types of interruptions a speaker made (or received) divided by the total number of turns in the hearing. See Figure 1 for a diagram and description of the coding scheme. Omitted category is lobbyist.

^{*}Female mass represents the proportion of the committee hearing (committee members, sponsor, and chair) that is female. The variable was coded female tokenism = 1 (less than 30% female), female critical mass = 2 (30%-50% female), female majority = 3 (over 50% female).

^{*} $p \le .15$.
** $p \le .10$.
*** $p \le .05$.
**** $p \le .01$.

nesses took. While we might expect that citizens and witnesses opposed to bills would receive fewer opportunities to participate in the hearing, this is actually only the case for those who are women. Among the less "politically connected" witnesses, there appears to be a significant gender credibility gap.

However, there is a surprise, too. Female witnesses who testified on their own accord (rather than being asked by the sponsor) spoke earlier in the hearing than their male counterparts. These women also took more turns, made more interruptions, and received more interruptions, but they did not speak significantly more words than the men. The verbal patterns indicate that these women were persistent in trying to take the floor away from other speakers but were unable to hold it long enough to actually say more than the men.

Among the more politicized witnesses, male bureaucrats were asked more questions (they took more turns) but also received more interruptions. Female bureaucrats took significantly fewer turns and made and received fewer interruptions, indicating that they were asked far fewer follow-up questions by the legislators after their initial testimony.

Sex differences among the witnesses—as well as other sex differences related to the sponsor, chair, and percentage of female committee members—are present. However, these statistical measures miss other very distinctive treatments that male chairs used when addressing or acknowledging female witnesses. A woman who testified was usually addressed by her first name by male chairs, whether she was an unknown expert, citizen, familiar lobbyist, or bureaucrat; but a witness who was a man received a title in front of his name, both at the time of introduction and at the conclusion of his remarks. Female chairs used titles with both men and women who were unknown to them and reserved first names for witnesses of both sexes with whom they were familiar. 12 The most egregious example of this sexist treatment occurred in a hearing on a health issue, in which several doctors testified. Although the woman witness clearly stated her title and name as "Dr. Elisa Jones," 13 the male chair addressed her repeatedly as "Elisa" and finally thanked "Mrs. Jones" for her testimony. Needless to say, perhaps, none of the male doctors were referred to as anything but "Dr. ___.'

This all-too-familiar unconscious downgrading of women's authority and position in society, while not affecting the participation rate of female "expert" witnesses, may be providing another view of the sexist culture in committee hearings that permeates the participation rates for some female witnesses and most female committee members. Women experts may be given equal opportunity to speak, but one has to wonder if women's expert testimony is seen as equally credible with men's.

Chairs

In the first analysis of positional roles, we saw that men and women chairing hearings had different

TABLE 5

Type of Chair Turn Taken by Sex

1		•		
TYPE OF	MEAN	PERCENT		
TURN	MALE	FEMALE	T	P
Procedural	78.6	94.4	-3.01	.009
Clarification	4.7	2.2	1.15	.269
Question/opinion	16.7	3.4	3.32	.007
Total %	100.0	100.0		

Note: The percentages shown are the mean percentages for the chairs of each gender, based on 8 male chairs (who collectively took 391 turns) and 9 female chairs (who collectively took 338 turns).

speaking behaviors, with female chairs participating less-and interrupting less-than male chairs. In the analysis of witnesses and committee members' speaking behavior, we saw that under a male chair, the chair and his male colleagues on the committee questioned female sponsors early in the hearing. A slightly different approach to examining chair behavior can illuminate what these differences mean to committee members and witnesses. Since chairs play a central role in directing committee discussion, the type of turns taken by chairs were coded into one of three categories: Parliamentary turns were procedural turns, as when the chair calls a witness or recognizes a committee member who wanted to speak; clarifying turns request an affirmation from a previous speaker about the content or meaning of a statement; question/ opinion turns interject a new idea into the discussion. 14 Beyond taking the floor away from speakers, male chairs also influenced and controlled committee hearing discussions through engaging in substantive comments more than female chairs did. Table 5 examines the sex differences by type of turn.

Men and women chairs did not differ in their use of clarification questions. However, women were more likely to act as facilitators of the hearing, as evidenced through their almost exclusive use of procedural turns. And while men, too, used mostly procedural turns, in one out of six turns (on average), men interjected personal opinions or guided the committee members and witnesses to a topic of their interest. Men used their position of power to control hearings in ways that we commonly associate with the notion of positional power and leadership (see Jones 1989). Indeed, the earlier gendered patterns among committee members may be the interaction of women chairs acknowledging all voices (to be more inclusive) in conjunction with male committee members' more assertive verbal behavior (as evidenced in their interruption and speaking patterns) when there is a woman sponsor, a feminized hearing, or a family issue being considered. In other words, while the conversational terms of the game had been changed through different chair styles, the more dominant verbal behavior of men as committee members became accentuated in a hearing where the chair used a more democratic, less autocratic style (Eagly and Johnson 1990).

DISCUSSION

Although it is commonly believed that women's increased presence in state legislatures will lead to their greater power and influence in policymaking, this assertion has never been tested through observation. As an initial step toward empirically documenting these changes in women's political access to power, I examined their speaking behavior in legislative committee hearings. The findings reported here suggest that women legislators, despite their numerical and positional gains, may be seriously disadvantaged in committee hearings and unable to participate equally in legislative committee hearings. These findings are not actually surprising given our culture and the social construction of male power. Perhaps most disturbing are the results that substantiate Yoder's (1991) thesis of intrusiveness. Contrary to some scholarly expectations and legislators' selfreported behavior (Thomas and Welch 1991), the more women on a committee, the more silenced women became. It may be that women are particularly active in behind-the-scenes negotiations, but informal participation that alters a bill has to surface as formal participation in the form of amendments in committee or on the floor (Hall 1987).

Indeed, in the present research, there was a significant positive relationship between speaking and offering nontechnical amendments (t = $\bar{2}$.66, p = .02), with men dominating both. Participation and influence appear to be strongly related, but it may be premature to conclude that women's impact in policymaking is being undermined thoroughly. For example, in the current data set, the bills sponsored by men or women were equally likely to pass out of the first committee hearing (female $n = \hat{4}$, male n = 4) and become law (female n = 2, male n = 3). Even among all 360 bills introduced in the 1989 session of the Colorado House there were no statistically significant sex differences in traditional legislative outcome measures such as the final passage of the bill (female 52%, male 55%). However, traditional outcome measures such as passage rates are problematic, because they only capture final points in a complex policymaking process whose participants are neither politically, institutionally, nor socially equal. In attempting to assess women's impact (as chairs, sponsors, or committee members), such acontextual nonsubstantive measures can be seriously misleading. For example, other more sensitive process measures found that women-sponsored bills were significantly more likely to be assigned to more than one House committee (39%, as compared to 27%), were subjected to longer discussions in first committee hearings (averaging 67 minutes, as compared to 54 minutes), and received twice as much hostile witness testimony (12%, as opposed to 6%) (Kathlene, Clarke, and Fox 1991). Thus despite the appearance of equal treatment via the measure of equal success, the more contextual measures suggest that women were even more successful than men since women's bills had a more difficult passage through the legislature. Similarly, the current research demonstrating highly gendered conversational dynamics defies the sex balance in passage rates, indicating that women are able to get their legislation through the process even in the face of men's more vocal participation in committees.

How do we understand women's apparent passage-rate success in the context of such a process imbalance? Without a discourse analysis of the committee discussions, we can only speculate. But some of the gendered patterns may be tapping into differing legislative styles, including men and women's attitudinal approaches toward policy problems and solutions and leadership styles (Eagly and Johnson 1990; Kathlene 1991). However, it would be a mistake to interpret the social and institutional gendered patterns as merely reflections of individual gender differences with no cumulative effect on power and influence. Each gendered layer interacts and reinforces other layers in ways that can enhance existing power (Ragins and Sundstrom 1989), which may indeed thwart women's impact on policymaking. For if legislative bodies rely on committees to inform and guide policy decisions and these same committees are actually the locus of biased information (Jones, Baumgartner, and Talbert 1993), then when men control the policy debate early in the process their gender power will be enhanced within an institutional process that is already structured to "hear" and promote a myopic view of an issue. This has significance for all public policy but I would argue that it is particularly disconcerting for policy issues in which women have a special stake (e.g., women's rights, women's health concerns, and domestic violence). If male chairs have a propensity to interrupt speakers and choose the points to be discussed, women's issues will not receive a fair or (probably) informed hearing. If rank-and-file men are propelled to "take up" and "take away" the floor from rankand-file women when a female sponsors a bill or the issue heavily affects women, then women's imprint on the policy process will be undermined. The alternative-looking to women chairs as potential power brokers of women's interests—assumes either that their leadership style is similar to men's or that their "different" leadership style will have the same effect as men's on controlling debate in hearings. But there may be little reason, theoretically, to believe that a more democratic or inclusive style that women tend to use will produce the same control over the agenda as the more commonly used autocratic style of men.

Ultimately, to understand gender power differentials, we need to examine how the content of a bill changes as it passes through the process and look closely at who is influencing the changes. Is male verbal aggression directed at women-sponsored bills or women's issues resulting in a transformation of their bills that is disproportionately influenced by male voices? Or do women as chairs or committee members have ways (other than verbal participation in committees) to imprint their concerns on legisla-

tion as it moves through the process? For example, whether women's ideas raised in committee hearings are reflected in amendments sponsored by men become an important indirect measure to consider in future analyses.

Examining the speaking behaviors of legislators during committee hearings provides us with yet another view of how powerful gender is in our culture. Bringing more women into politics will not translate directly into a proportionate amount of female power and influence. In fact, there may be a powerful backlash present when women exceed a certain critical mass in a highly masculinized institution such as legislative politics (Yoder 1991). Yet it is clear that the presence of more women legislators will lead to more women sponsoring bills and chairing committees. When women chaired committees the dynamics of the hearing changed for witnesses and female sponsors in complex ways, some of which may provide for more democratic policymaking, particularly if the negative effects of increased verbal aggressiveness by men can be countered. This examination of speaking behavior only begins to tap into gender differences that may have profound substantive implications for legislative decision making.

APPENDIX

The Pearson correlations for the dependent variables in each analysis are presented in Table A-1.

Notes

An early version of this research was originally presented at the annual meeting of American Political Science Association, 1992, Chicago. The expanded model was originally presented at the annual meeting of the Western Political Science Association, 1993, Pasadena. The research was funded, in part, by the Center for the American Woman and Politics, a unit of the Eagleton Institute of Politics at Rutgers University, under a grant from the Revson Foundation, and the Purdue Research Foundation. I would like to thank Joseph Smith and Silvo Lenart for their discussions on measurement issues and Georgia Duerst-Lahti for her feedback on the normative implications.

1. Research on women in management supports these findings in the legislative setting (Rosener 1990).

2. The Colorado House is bound by its own chamber rules, which set procedures for amending bills in committee and on the floor and set speaking time and turn limits during the third reading on the floor (NCSL 1992).

3. Of the 164 House bills that did not become law, 52.4% died in first committee, 17.6% died in second committee (usually this was House Appropriations), 10.3% died on the House floor, 17.6% died in the Senate, and 2.0% were vetoed

by the governor.

- 4. The larger sample of 68 bills were selected from all 360 bills introduced in the 1989 session of the Colorado State House based on a number of criteria: (1) sex of the sponsor, (2) sponsor's terms in the legislature, (3) issue area of the bill, (4) state fiscal impact, and (5) public visibility. The sample of 68 bills was representative of the 360 bills introduced in the 1989 session in terms of these criteria and legislative outcome factors, including House committee hearing outcomes, amendment action, House or Senate floor action, and governor action (see Kathlene 1991).
- 5. This time elapsed to speaking does not include committee members who were silent during the entire hearing. The measures of words spoken and turns taken captures silent members, where silent speakers' words and turns are coded as 0.
 - 6. I found that sponsor amendments were likely to result

SPEAKERS	% TIME ELAPSED	WORDS SPOKEN	TURNS TAKEN	INTERRUPTS MADE (%)
All speakers ^a				
Words spoken	42	-	_	****
Turns taken	40	.37	_	and the same of th
Interruptions made (%)	43	.49	.75	
Interruptions received (%)	31	.29	.61	.42
Committee members only ^b				
Words spoken	36		_	Monorma
Turn taken	45	.82		vancours.
Interruptions made (%)	11	.21	.42	*****
Interruptions received (%)	37	.65	.59	17
Witnesses only ^c				
Words spoken	40		_	A
Turns taken	24	.83		
Interruptions made (%)	21	.31	.39	
Interruptions received (%)	03	.28	.48	.34

from one of three conditions: (1) the sponsor did not have all the relevant detailed information before the legislative deadline for introducing bills, (2) the amount of the fiscal note had not been determined by the legislative staff prior to bill introduction (typical for the majority of bills), and (3) negotiations with interested groups had already occurred between the time the bill was introduced and its first committee

7. A high number of turns taken could also reflect interruptions, in which case turns taken would not be a good measure of active speaking involvement. Overall, however, interruptions accounted for only an average of 11.2% of the speaker turnovers. However, this low interruption rate may vary by groups, so that each analysis also included a test of differences in mean words spoken per turn by dividing the number of each speaker's words spoken by the number of full turns the speaker took. A full turn was a substantive utterance and did not include turns restricted to only social introductions or parliamentary procedures. An interrupted turn was likely to be substantive as speakers tended to interrupt during more heated debates of the issue. In each subanalysis reported here, there were no significant differences found in the measure of words spoken per turn. Therefore, I believe that the number of turns taken does accurately measure the extent of speaking engagement.

8. All the committee member regression analyses were run twice—first using freshman status as the tenure variable and then using terms in the legislature (1-4) as the tenure variable. The analyses did not differ based on the measure of tenure

used.

9. The variable capturing the feminization of the committee hearing was coded in two different ways: as the three-interval categories described in the text and as a continuous percentage measure. Both variables performed similarly in all the analyses.

10. It was common for the chair to invite the sponsor to express a preference for the order of the witnesses. Sometimes the sponsor was interested in ordering all of the witnesses, but usually the sponsor only had a preference for who spoke first. Therefore, any gender differences found in the amount of hearing time elapsed before speaking could be an indicator of gendered *sponsor* treatment of the witnesses as much as, if not more than, differential treatment by the chair.

11. A full interaction regression model was also run but the sex/hearing interaction terms did not significantly improve the explained variance. The only difference between the main-effects hearing characteristics and the interaction model was the emergence of male interruption patterns that accounted for the increased interruptions made under a female

chair and at a family hearing.

12. It is not hard to distinguish between familiar and unfamiliar witnesses. When a chair knows the witness, he or she usually make a remark indicating their familiarity, such as "Nice to see you again" or "It's been a long time since you've come to this committee." A witness who is unknown elicits slower and more careful (if not mistaken) enunciation of their name, along with the chair offering precise directions to the witness: "State your name and whom you represent, for the tape."

13. Dr. Elisa Jones is a fictitious name.

14. The categories of the type of turns resembles Hall's (1987) coding of chair's turns as administrative vs. substantive, although I further breakdown substantive remarks based on my personal observations, in committee hearings, that chairs become involved in the issues of the hearing in two distinct ways.

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DOMESTIC POLITICAL AUDIENCES AND THE ESCALATION OF INTERNATIONAL DISPUTES

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International crises are modeled as a political "war of attrition" in which state leaders choose at each moment whether to attack, back down, or escalate. A leader who backs down suffers audience costs that increase as the public confrontation proceeds. Equilibrium analysis shows how audience costs enable leaders to learn an adversary's true preferences concerning settlement versus war and thus whether and when attack is rational. The model also generates strong comparative statics results, mainly on the question of which side is most likely to back down. Publicly observable measures of relative military capabilities and relative interests prove to have no direct effect once a crisis begins. Instead, relative audience costs matter: the side with a stronger domestic audience (e.g., a democracy) is always less likely to back down than the side less able to generate audience costs (a nondemocracy). More broadly, the analysis suggests that democracies should be able to signal their intentions to other states more credibly and clearly than authoritarian states can, perhaps ameliorating the security dilemma between democratic states.

n international crisis occurs when one state resists a threat or demand made by another, with both taking actions that suggest that the dispute might be decided militarily. Crises are frequently characterized as "wars of nerves." Measures such as troop deployments and public threats make crises public events in which domestic audiences observe and assess the performance of the leadership. For reasons linked to this public aspect of crises, state leaders often worry about the danger that they or their adversary might become locked into their position and so be unable to back down, make concessions, or otherwise avoid armed conflict.

In this article I model an international crisis as a political "war of attrition." The formalization is motivated by an empirical claim, namely that crises are public events carried out in front of domestic political audiences and that this fact is crucial to understanding why they occur and how they unfold. I characterize crises as political attrition contests with two defining features. First, at each moment a state can choose to attack, back down, or escalate the crisis further. Second, if a state backs down, its leaders suffer audience costs that increase as the crisis escalates. These costs arise from the action of domestic audiences concerned with whether the leadership is successful or unsuccessful at foreign policy (Fearon 1990, 1992; Martin 1993).

The formalization has three major benefits. First, it helps answer an important question about the origins of war that is missed in the informal literature and begged by existing formal models of crisis bargaining. Briefly, if fighting entails any cost or risk, then rational leaders would not choose war until they had concluded that attack was justified by a sufficiently low chance of an acceptable diplomatic settlement. Thus another way to ask the question "Why do wars occur?" is to ask what leads states to abandon the hope of a cheaper, nonmilitary resolution. A theoret-

ical answer requires us to explain how a state with rational leaders would *learn*. During a crisis, how do leaders come to revise their beliefs about an opponent so that attack is preferred to holding out for concessions? I shall argue that neither the informal nor the formal literature on international conflict supplies satisfactory answers.

The answer suggested here is that audience costs are an important factor enabling states to learn about an opponent's willingness to use force in a dispute. At a price, audience costs make escalation in a crisis an informative although noisy signal of a state's true intentions. They do so in part by creating the possibility that leaders on one or both sides will become locked into their position and so will be unable to back down due to unfavorable domestic political consequences. I find that in the model, a crisis always has a unique horizon—a level of escalation after which neither side will back down because both are certainly locked in, making war inevitable. Before the horizon is reached, the fear of facing an opponent who may become committed to war puts pressure on states to settle. The model thus captures a common informal story about international crises—that their danger and tension arise from the risk of positions hardening to the point that both sides prefer a fight to any negotiated settlement.

The second major benefit of the formal analysis is a set of comparative statics results that provide insights into the dynamics of international disputes. The strongest and most striking of these bear on the question of which state is more likely to concede in a confrontation. I find that regardless of the initial conditions, the state more sensitive to audience costs is always less likely to back down in disputes that become public contests. The intuition is that the greater the domestic cost for escalating and then backing down, the more informative is the signal of escalation and the less escalation is required to con-

vey intentions. A stronger domestic audience thus allows a state to signal its true preferences concerning negotiated versus military settlements more credibly and more clearly.

This result and the audience cost mechanism underlying it suggest hypotheses about how state structure might influence crisis bargaining. For example, if actions such as mobilizing troops create larger audience costs for democratic than for authoritarian leaders, then democratic states should be less inclined to bluff or to try "limited probes" in foreign policy—to make military threats and then back off if resistance is met. More broadly, stronger domestic audiences may make democracies better able to signal intentions and credibly to commit to courses of action in foreign policy than nondemocracies, features than might help ameliorate "the security dilemma" (Herz 1950; Jervis 1978) between democratic states.

The comparative statics results also speak to the question of how relative military capabilities and relative interests influence the outcomes of international disputes. Conventional wisdom suggests that the state with inferior military capabilities, or with fewer "intrinsic interests" at stake, is more likely to back down (e.g., George and Smoke 1974, 556-61; Jervis 1971; Snyder and Diesing 1977, 189-95). Surprisingly, in the model, neither the balance of forces nor the balance of interests has any direct effect on the probability that one side rather than the other will back down once both sides have escalated. The reason is that in choosing initially whether to threaten or to resist a threat, rational leaders will take into account observable indices of relative power and interest in a way that tends to neutralize their impact if a crisis ensues. For example, a militarily weak state will choose to resist the demands of a stronger one only if it happens to be quite resolved on the issues in dispute and so is relatively willing to escalate despite its military inferiority. The argument implies that observable aspects of capabilities and interests should strongly influence who gets what in international politics but that their impact should be seen more in uncontested positions and faits accomplis than in crises. Which side backs down in a crisis should be determined by relative audience costs and by unobservable, privately known elements of states' capabilities and resolve.

The third major benefit of the analysis is slightly more technical. The model clarifies how international crises differ structurally from the classical war of attrition studied by economists and theoretical biologists (Maynard Smith 1982, chap. 3; Tirole 1989, chap. 8). In the classical case, two firms (or animals) compete for control of a market (or territory) that is not large enough to support both at a profit. The competition lasts until one or both players "quit." International crises are analyzed here as a war of attrition that differs from the classical model in two important respects. First, in crises state leaders possess an additional option beyond continuing the contest or quitting—they can always choose to attack. Second, whereas in the classical war of attrition both sides pay

costs for continuing the contest, in international crises it is empirically more plausible to assume that only the side that backs down suffers audience costs. The existence of a military "outside option" along with audience costs proves to have major consequences for strategic behavior. Together they create the possibility of "lock in" and thus give crises a horizon. More technically, whereas the classical war of attrition has an infinity of (asymmetric) equilibria involving delay, the game studied here has a unique equilibrium distribution on outcomes up to the horizon.

First, I briefly review the relevant formal literature and also elaborate the theoretical puzzle: Given incentives to misrepresent, how can states involved in a dispute rationally reach the conclusion that the opponent would prefer war to backing down? I then informally discuss possible answers, arguing for the centrality of domestic audience costs, model an international crisis as a political attrition contest to examine the logic of equilibrium behavior, and, finally, draw some general conclusions.

THE THEORETICAL PUZZLE

The costs and risks of war supply states with strong incentives to locate nonmilitary settlements that both sides would prefer to a fight. Most often, it seems, their efforts are successful: very few international disagreements become wars. This may seem unsurprising at first glance. One might expect that given the incentives to avoid war, state leaders who disagree on some issue could simply tell each other what they would be willing to accept rather than fight, and then choose a mutually acceptable bargain. The problem, however, is that states can also have strong incentives to misrepresent their willingness to fight in order to gain a better deal. Given these incentives, quiet diplomatic exchanges may be rendered uninformative about a state's preferences. For example, in the Cuban Missile Crisis Kennedy did not ask Khrushchev what he would do if the United States were to impose a blockade or to attack the missile sites in Cuba: answers would have been almost worthless as indicators, due to Khrushchev's incentives to misrepresent (and Kennedy may also have had an incentive not to tip his hand) (cf. Wagner 1989, 197).

States in a dispute thus face a dilemma. They have strong incentives to learn whether there are agreements both would prefer to the use of force, but their incentives to misrepresent mean that normal forms of diplomatic communication may be worthless. I argue that international crises are a response to this dilemma. States resort to the risky and provocative actions that characterize crises (i.e., mobilization and deployment of troops and public warnings or threats about the use of force) because less-public diplomacy may not allow them credibly to reveal their own preferences concerning international interests or to learn those of other states.

To support this claim it must be shown how such actions can credibly reveal that a state would prefer

using force to making concessions. In particular, how is it that actions like mobilization and public warnings allow learning? If states can have incentives to misrepresent their willingness to use force, why should such actions be taken as credible indicators?

For the most part, the informal literature on international conflict and the causes of war takes it as unproblematic that actions such as mobilization "demonstrate resolve." The literature has focused instead on how psychological biases may impair a leader's ability to interpret crisis signals (e.g., Lebow 1981; Snyder and Diesing 1977, chap. 4; Jervis, Lebow, and Stein 1985). The prior question of how a rationally led state would learn in a crisis, given incentives to misrepresent, has not been answered in a theoretically thorough or satisfactory way.

Consider the inference problem faced by a state whose adversary in a dispute has just mobilized troops. If rational, the state's leaders should increase their belief that the adversary will fight only if a high-resolve adversary is more likely to mobilize than an adversary that in fact prefers backing down to war. Thus, if mobilization is to convey information and allow learning, it must carry with it some cost or disincentive that affects low-resolve more than highresolve states. In Spence's (1973) terms, mobilization (or any other move in a crisis) must be a costly signal if it is to warrant revising beliefs. Costless signals, which often include private diplomatic communication and sometimes more public measures, will be so much "cheap talk," since a state with low resolve may have no disincentive to sending them.

To explain how states learn in a crisis, we need to know what makes escalation or delay costly for a low-resolve state that in fact prefers making concessions to military conflict. It is tempting to answer "the risk of war", but this would beg the question since we are trying to establish how this risk arises in the first place. I shall argue that the role of domestic political audiences has typically been crucial for generating the costs that enable states to learn. First, however, I briefly review how the published formal literature on crisis bargaining has addressed the issue.

A number of studies have developed models in which states rationally update their beliefs about an adversary's resolve in the course of a crisis (Bueno de Mesquita and Lalman 1992; Fearon 1990, 1992; Kilgour 1991; Morrow 1989; Nalebuff 1986; Powell 1990; Wagner 1991). Though this is not always apparent, the mechanism that enables learning in each case is costly signaling.

While updating of beliefs occurs in these models, they actually do not address the question of how and why states might rationally come to conclude that fighting was preferable to holding out for concessions. The reason is that almost all of the models have finite horizons: the modeler exogenously determines that one of the states in the game will have a final choice between backing down or fighting. In effect, one player will ultimately have no choice but to "take it or leave it," and this restriction creates a cost for escalation. In actual crises, by contrast, whenever a

state has the option of attacking it also has the option of delaying or doing nothing. If there are horizons in actual crises they arise *endogenously*, as a consequence of the fact that for some reason waiting ultimately becomes an undesirable choice. Models that exogenously fix a horizon cannot explain why a state would choose to use force (and thus why wars occur) because they cannot explain what makes force preferable to holding out for concessions by the other side.

There are two partial exceptions to this argument. Nalebuff's (1986) and Powell's (1990) models of nuclear brinkmanship have something like an infinite horizon: they allow states to escalate in a crisis indefinitely, until one side backs down or nuclear war occurs. However, in these models states never *choose* to attack. Instead, war can occur only as a result of an accident beyond either side's control. Thus these formalizations cannot and were never intended to explain why states would *consciously* choose to abandon peace for war.

Historically, war has virtually always followed from the deliberate choices of state leaders, if not always as the result they originally intended (Blainey 1973; Howard 1983). Since this pattern seems likely to continue even in the nuclear age, it makes sense to ask how states could reach the conclusion that attack was worth choosing.

AUDIENCE COSTS, STATE STRUCTURE, AND LEARNING IN INTERNATIONAL DISPUTES

I shall consider several types of costs that could serve to make the actions that states take in crises informative about their actual willingness to fight. I argue that while there are several plausible candidates that may play a role in specific cases, audience costs are probably most important and characteristic of crisis bargaining. I shall then discuss variations in audience costs across regime types, suggesting that they may be most significant in states where foreign policy is conducted by an agent on behalf of a principal, as in democracies.

Signaling Costs in Crises

Two sorts of costs that leaders face for backing down in a crisis should be distinguished. First, there is the domestic and international price for conceding the issues at stake, which is the same regardless of when concessions are made or after how much escalation. Second, there are whatever additional costs are generated in the course of the crisis itself. By the costly signaling argument, only such added costs can convey new information about a state's resolve. To ask what enables learning in a crisis—and thus why some states ultimately choose to attack—is to ask what makes escalating and then backing down worse for a leader than simply conceding at the outset.

There are a number of possible mechanisms,

grouped here into three categories: physical costs, costs linked to the risk of accidental or preemptive war, and international and domestic audience costs.

The first class includes the financial and organizational costs of mobilizing and deploying troops and also simple impatience ("time preferences") on the part of state leaders. The economic burden of mobilization is sometimes significant enough to convey information about resolve. The fact that Israel's economy cannot bear full mobilization for very long may make Israeli mobilization unusually informative (Shimshoni 1988, 110). One could also argue that the enormous costs of Desert Shield, given well-known U.S. budget constraints, helped make the deployment a (partially) credible indicator of Bush's preferences (Fearon 1992, 153-54). But since the early nineteenth century, the financial costs of mobilization rarely seem the principal concern of leaders in a crisis, particularly in comparison to how their performance looks to domestic and foreign audiences. In few cases do financial costs seem to be what makes crises into political "wars of nerves."3

For similar reasons, pure time preferences appear less significant a signaling mechanism in crises than in buyer-seller bargaining and other economic examples.4 Under time preferences, delay in a crisis would be a costly signal because a leader with a high value for settlement versus war is relatively more impatient to enjoy whatever benefits a resolution would allow. If state leaders are sometimes impatient for a deal, it seems more often due to domestic political pressures (e.g., American elections or Gorbachev or Yeltsin's need for cash) than to a pure preference by the leader for "territory today rather than next month."

The second class of signaling costs concerns risks of war that are generated in some direct way by crisis escalation. Schelling's famous notion of the "threat that leaves something to chance" falls into this category (1960, chap. 8). Schelling suggested that in nuclear crises, at least, escalation or delay might create a risk of war resulting from something other than the deliberate choices of state leaders (e.g., a mechanical mishap or an unauthorized launch). If such risks exist, then escalation in a crisis will be a costly signal of resolve, since the risks are less worth running for a state with low interest in the issues at stake.

In the Cuban Missile Crisis, American decision makers did indeed worry about the risk of war stemming from a mechanical or a command-andcontrol accident (e.g., Blight and Welch 1990, 109, 311). But even in this most intense of all nuclear crises (a "best case" for the threat-that-leaves-somethingto-chance argument), the key decision makers were much more concerned about risks of war connected to what the other side would *choose* to do. While the risk of accidental war may contribute to crisis learning, it rarely, if ever, seems to provide the main cost of escalating a dispute.6

First-strike advantages, or incentives for preemptive war, provide a more appealing explanation in this class. If escalating a crisis entails a risk the other side will conclude that concessions are unlikely

enough to justify seizing a first-strike advantage, then escalation might be a costly signal of resolve. By running a real risk of preemption, delay in a crisis might credibly reveal a high willingness to fight rather than concede.

This mechanism appears to have figured in some historical cases. For example, part of what made the Russian mobilization in 1914 an informative signal of Russia's willingness to fight was that it was undertaken in the knowledge that it would increase Germany's incentive to choose preemptive war (Fearon 1992, chap. 5).7 In theory, however, first-strike advantages could also have the opposite effect. A state might conclude that since the adversary has not so far availed itself of a first strike, it must be more willing to back down than initially anticipated. Further, major concerns about loss of first-strike advantage do not seem common in case-evidence on international crises, and even when such concerns are present, as in 1914, they often compete with worries about the

political disadvantages of going first.8

While each of the preceding mechanisms may well foster crisis learning in some cases, I would argue that none fits with our intuitive sense of what it is that makes international crises into political wars of nerves. The reason is that none of these mechanisms recognizes the public aspect of crises, the fact that they are carried out in front of political audiences evaluating the skill and performance of the leadership. In prototypical cases (e.g., the standoff leading to the 1991 Gulf War, the Cuban Missile Crisis, and July 1914), a leader who chooses to back down is (or would be) perceived as having suffered a greater "diplomatic humiliation" the more he had escalated the crisis. Conversely, our intuition is that the more a crisis escalates, the greater the perception of diplomatic triumph for a leader who "stands firm" until the other side backs down.

Political audiences need not and do not always have this pattern of perceptions and reactions: they are social conventions that are at times resolved differently. For example, leaders of small states may be rewarded for escalating crises with big states and then backing down, where they would be castigated for simply backing down. Standing up to a "bully" may be praised even if one ultimately retreats. Nonetheless, at least since the eighteenth century leaders and publics have typically understood threats and troop deployments to "engage the national honor," thus exposing leaders to risk of criticism or loss of authority if they are judged to have performed poorly by the relevant audiences. Two illustrations follow, both taken from eighteenth-century diplomacy. While a wealth of similar examples are available from the nineteenth and twentieth centuries, these earlier cases suggest that political audiences have mattered in international confrontations for a long time.

The Seven Years War (1757-64) between France and Britain was preceded by several years of "crisis" diplomacy—threats, warnings, and troop mobilizations and deployments (Higonnet 1968; Smoke 1977, chap. 8). In response to French demands on the Ohio

River Valley, the Duke of Newcastle chose in late 1755 to send two regiments to America to impress the French with British resolve. The decision distressed several of Newcastle's colleagues and ambassadors, who seem to have felt that the action engaged the honor of the king and so committed the cabinet to a warlike course, perhaps unnecessarily. One wrote, "It requires great dexterity to conduct [these diplomatic and military moves] in such a manner to maintain the honor of King and Nation" (Higonnet 1968, 79). In a later interview with Rouillé, the French minister of foreign affairs, the British ambassador reported that the minister "complain'd very much of the licentiousness of our Publick papers in exaggerating things beyond measure which only served to irritate and stir up animosity amongst the lower sort of People in both Nations without a just cause" (p. 80). This complaint suggests that even in nondemocratic, eighteenth-century France, a minister could be concerned with what I have called domestic audience costs: it seems that British pamphlets could have the effect of increasing Rouille's costs for acceding to British demands (as they increased Newcastle's domestic costs for ceding French demands).

About 35 years later, Britain and Spain nearly went to war over an obscure incident involving alleged Spanish insults to British seamen who had landed on Vancouver Island, along with competing claims on the territory (Manning 1904). Once again, both states resorted to troop mobilizations, forceful diplomatic notes, and public threats. There is strong evidence that these moves created significant domestic audience costs for Prime Minister William Pitt: "With an election imminent, the Opposition was ready to make the most of any of the Government's mistakes in negotiating" (Norris 1955, 572). Pitt's vote for navy credits in Parliament and his government's publication of an account of the Vancouver incident led opposition politicians almost to clamor for appropriate satisfaction of British honor and right. Pitt would have faced serious domestic political costs for backing down, much larger than if he had chosen initially to pursue a less public and aggressive line in the dispute. 10

The notion that troop movements and public demands or threats "engage the national honor"—thus creating audience costs that leaders would pay if they backed down-continues strongly through the nineteenth and twentieth centuries. Such costs can be classified according to whether the audience that imposes them is domestic or international. Relevant domestic audiences have included kings, rival ministers, opposition politicians, Senate committees, politburos, and, since the mid-nineteenth-century, mass publics informed by mass media in many cases. Relevant international audiences include a state's opponent in the crisis and other states not directly involved, such as allies. Here the costs of escalating and then backing down would be felt indirectly through injury to the state's reputation for threatening the use of force only when serious.

Leaders engaged in disputes appear to worry about

both international and domestic audiences. Domestic audience costs may be primary, however. Backing down after making a show of force is often most immediately costly for a leader because it gives domestic political opponents an opportunity to deplore the international loss of credibility, face, or honor. 11 Because governments are far more likely to be deposed or to lose authority due to internal political developments than due to foreign conquest and because opposition groups frequently condition their activities on the international successes and failures of the leaders in power, domestic audiences may provide the strongest incentives for leaders to guard their states' "international" reputations. Audience costs thus figure in a domestic system of incentives that encourages leaders to have a realist concern with their state's "honor" and reputation before international audiences.

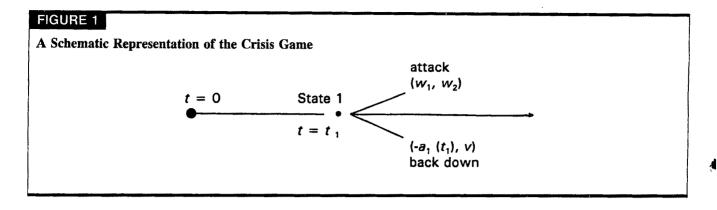
Agency Relations and Audience Costs

As noted, audience costs have a strongly conventional aspect; how they are felt and implemented depends on shared perceptions and expectations in a society. Nevertheless, the historical norm seems to have domestic political audiences punishing a leader who concedes after having deployed troops more than one who concedes outright. Why this norm? My theoretical results will suggest a possible explanation, which I anticipate here in order to develop some broader points about how audience costs vary across types of states.

Equilibrium analysis of the crisis model reveals that a state's ex ante expected payoff in a dispute is *increasing* in the degree to which escalatory moves create audience costs for the state's leadership. The reason, in brief, is that greater audience costs improve a state's ability to commit and to signal resolve.

Thus both democratic and nondemocratic leaders should have an incentive to represent that they will pay added domestic political costs for "engaging the national honor" and subsequently backing down. The extent to which such representations are believable, however, depends on the nature of the domestic political institutions that the leadership faces. In democracies, foreign policy is made by an agent on behalf of principals (voters) who have the power to sanction the agent electorally or through the workings of public opinion. By contrast, in authoritarian states the principals often conduct foreign policy themselves. The result here suggests that in the former case, if the principal could design a "wage contract" for the foreign policy agent, the principal would want to commit to punishing the agent for escalating a crisis and then backing down. On the other hand, principals who conduct foreign policy themselves may not be able credibly to commit to self-imposed punishment (such as leaving power) for backing down in a crisis.12

Examples of the apparent effect and import of forceful public speeches by democratic and nondemocratic leaders suggest that this argument is at least



plausible. Repeatedly, leaders in democratic countries have been able credibly to jeopardize their electoral future by making strong public statements during international confrontations. A few prominent examples are Lord Salisbury's speeches during the Fashoda crisis of 1898, Lloyd George's Mansion House speech during the Agadir crisis of 1911, Kennedy's televised speech announcing the presence of Soviet missiles in Cuba, and Bush's many declarations on Kuwait in 1990 (including the "this will not stand" remark). 13 By contrast, even more forceful public bluster by authoritarian leaders (e.g., Hitler, Khrushchev, Mao, and Saddam Hussein) appears to create fewer credible audience costs, and to have correspondingly lower value as signals of intent. For example, it was quite difficult for Western observers to know what to conclude about Saddam Hussein's willingness to fight from his many strong public refusals to pull out of Kuwait in the Fall of 1990. One reason, I would argue, is that it was difficult to know what, if any, added domestic political costs such a tyrant would suffer for making concessions at the last minute.

This is not to suggest that authoritarian states are completely unable to generate audience costs in international confrontations or that democracies can invariably do so. On the one hand, nondemocracies may evolve institutional arrangements in foreign policy that give domestic audiences an ability to sanction decision makers. For example, the politburo after Stalin could sanction the paramount Soviet leader, and eighteenth- and nineteenth-century monarchs could replace unsuccessful ministers. Moreover, since the price of losing power is often greater for a dictator than for an elected leader, a weak or unstable authoritarian regime might be able to create significant expected audience costs in a crisis. On the other hand, in democracies the existence of multiple politically relevant audiences may make it difficult for foreign leaders to gauge the costs created by public statements or actions, particularly before elections. The idea that democratic leaders on average have an easier time generating audience costs is advanced here as a plausible working hypothesis that has interesting theoretical and empirical implications.

INTERNATIONAL CRISES AS POLITICAL ATTRITION CONTESTS

I shall describe a model of a crisis as a political attrition contest, informally discuss equilibrium behavior when the states know each other's values for conflict, and then develop the main results on equilibrium with uncertainty about resolve.

Two states—1 and 2—are in dispute over a prize worth v > 0. The crisis occurs in continuous time, starting at t = 0. For every finite $t \ge 0$ before the crisis ends, each state can choose to attack, quit, or escalate. The crisis ends when one or both states attack or quit. "Escalate" can be thought of either as simply waiting or as taking actions such as mobilizing or preparing troops.

Payoffs are given as follows. If either state attacks before the other quits, both receive their expected utilities for military conflict, w_1 and w_2 . These are the states' values for war, incorporating military expectations, values for objects in dispute, and costs for fighting. They can be thought of as levels of "resolve," since the higher w_i , the higher the risk of war state i is willing to run in hope of attaining the prize. Throughout, I shall suppose that neither w_1 nor w_2 is greater than 0.14

If state i quits the crisis before the other has quit or attacked, then its opponent j receives the prize while i suffers audience costs equal to $a_i(t)$, a continuous and strictly increasing function of the amount of escalation with $a_i(0) = 0$. I will often consider the linear case $a_i(t) = a_i t$, where $a_i > 0$ is a parameter indicating how rapidly escalation creates audience costs for state i. Figure 1 schematically depicts the structure of the contest, with payoffs indicated in the case that state 1 quits or attacks first at time t_1 . ¹⁵

Call this game, with common knowledge of all parameters, G and the subgame beginning at t, G(t). A pure strategy in G is a rule s_i specifying a finite time $t \ge t'$ to quit or attack in every G(t'), for all $t' \ge 0$. I shall write $\{t$, attack $\}$ for the subgame strategy "escalate up to t, then attack" and $\{t$, quit $\}$ for "escalate up to t, then quit."

Equilibrium under Complete Information about Resolve

If the states knew each other's levels of resolve, they could in principle look ahead and anticipate what would happen if they were to escalate and create a crisis. For example, in the linear case where $a_i(t) = a_i t$, they would see that ultimately the audience costs for backing down would be so large that neither would quit: doing so would be strictly worse than attacking. In other words, both states would eventually become "locked in."

However, they would also notice that one side would become locked in *before* the other (excepting certain symmetric cases). Suppose, for example, that audience costs increase linearly and at the same rate on both sides, and that state 1's value for war is higher than state 2's. Then if both sides escalated the crisis, state 1 would reach the point where its leaders preferred conflict to backing down sooner than state 2 would. At this point, state 1 has in effect committed itself not to back down, while state 2 still prefers making concessions to a fight. Thus a rational state 2 would have to back down at this point. Anticipating this at the outset, state 2 would quit immediately rather than pay the larger audience costs that would go with publicly observable escalation. ¹⁶

Thus with complete information about resolve, no public crisis occurs. Instead, if audience costs increase at the same rate on both sides, the state with the lower value for conflict immediately cedes the prize rather than incur costs above those associated with the loss on the issue. If audience costs increase at different rates, then the side with weaker audience effects may be forced to concede even if it has a higher value for war (since it may require more escalation to commit itself to fight).

This equilibrium logic mirrors a logic found in many analytic and diplomatic historical discussions of international disputes. It is often argued that in crises that do not become wars, states look ahead and the side expecting to do worse in military conflict then backs down. But the standard argument does not work by itself: audience costs are required. If it were known that the state with the higher value for war nonetheless preferred making some concessions to a fight, why should the state with the lower value for war necessarily back down? If both prefer concessions to a fight, how can either make a credible threat to go to war and why should this be related to their values for military conflict? Increasing audience costs supply an answer. The state with the higher value for war may be able, in a public crisis, to reach more quickly the point where it prefers conflict to paying the audience costs of backing down.

Incomplete Information about Resolve

With complete information, no public war of nerves occurs because the ultimate outcome can be seen in advance. In a rationalist framework, international crises occur precisely because state leaders *cannot*

anticipate the outcome, owing to the fact that adversaries have private information about their willingness to fight over foreign policy interests and the incentive to misrepresent it. I now consider equilibrium in the model in which the states have private information about their willingness to go to war.

Three main theoretical results are developed. Proposition 1 establishes that in any equilibrium in which a crisis may occur, the crisis has a unique *horizon*—an amount of escalation after which neither state will back down and war is certain. Proposition 2 characterizes the set of equilibria in which a crisis may occur. Proposition 3 asserts that in this set, the probability distribution on outcomes is unique up to the horizon time. Throughout, the equilibrium concept is a modification of perfect Bayesian equilibrium (Fudenberg and Tirole 1991) for an infinite game, with an additional restriction ruling out strongly optimistic off-equilibrium-path inferences. Details on the solution concept, along with all proofs, are given in the Appendix.¹⁷

Preliminaries. Formally, suppose that each state knows its own level of resolve w_i but knows only the distribution of its adversary's resolve w_j . For i=1,2, let w_i be distributed on the interval $W_i \equiv [\underline{w}_i,0]$, $\underline{w}_i < 0$, according to a cumulative distribution function F_i that has continuous and strictly positive density f_i . If refer to the crisis game with this information structure and all other elements assumed to be common knowledge as Γ.

In informal terms, f_1 and f_2 represent the states' precrisis beliefs about each other's value for war on the issue in dispute. For example, the more weight f_1 puts on values of w_1 that are close to 0 (as opposed to very negative), the greater is state 2's initial belief that 1 has a relatively high willingness to fight rather than make concessions.

Crises in the Model have a Unique Horizon. This proposition is developed by way of two lemmas, which also help to make clear the logic of strategic choice in the model. I begin with a definition. A crisis has a horizon if there is a level of escalation such that neither state is expected to quit after this point is reached. Formally, let $Q_i(t)$ be the probability that state i quits on or before t in some equilibrium. Then $t_h > 0$ is a horizon for Γ if in this equilibrium t_h is the minimum t such that neither $Q_1(t)$ nor $Q_2(t)$ increase for $t > t_h$. If a horizon exists, war has become inevitable by the time it is reached.

Lemma 1 establishes that in any equilibrium in which a crisis may occur, there must exist a horizon. Horizons are thus shown to arise *endogenously*, as a consequence of the equilibrium choices by the states involved. The intuition for this result is straightforward in the case of linearly increasing audience costs: eventually the price of backing down will be so great that even the least-resolved type of state would prefer to attack rather than quit. But the result holds even if "maximum escalation" (arbitrarily large t) will not

create large-enough audience costs to commit every type of state to war.

LEMMA 1. In any equilibrium of Γ in which both states choose to escalate with positive probability, there must exist a finite horizon $t_h < \infty$.

Lemma 2 characterizes the behavior of states that choose a strategy that could lead to war. The first part establishes that in equilibrium no state will choose to attack *before* a horizon time. When there is no advantage to striking first, a state unwilling to make concessions will want to delay attack as long as there is any chance that the other side will back down, thus avoiding the risk of an unnecessary military conflict while maximizing the chance of a "foreign policy triumph."

The second part of the lemma shows that a state will choose the strategy of escalating to the horizon and then attacking if and only if its privately known level of resolve, w_i , is sufficiently large. Thus crises in the model separate states according to their unobservable willingness to fight over the issues: a highly resolved state credibly reveals its motivation by choosing an unyielding crisis-bargaining strategy. This gives it a greater chance of prevailing if the crisis ends peacefully but at the (unavoidable) price of a greater risk of war (cf. Banks 1990). It follows that for outside observers, crisis outcomes must be unpredictable to a significant degree. Comparative resolve strongly influences the outcome, but the fact that resolve is privately known (and unobservable) gives rise to public crises in the first place.

LEMMA 2. In any equilibrium of Γ with t_h as the horizon and in which escalation may occur, (1) if state i chooses $\{t, attack\}$, it must be the case that $t \ge t_h$; and (2) state i will choose $\{t, attack\}$ where $t \ge t_h$ if $w_i > -a_i(t_h)$ and only if $w_i \ge -a_i(t_h)$ (for i = 1, 2).

Lemma 2 implies that the ex ante (precrisis) probability that state i will choose a strategy involving attack is $1 - F_i(-a_i(t_h))$, the prior probability, that $w_i \ge$ $-a_i(t_h)$. Thus we can write state i's ex ante expected utility for escalating up to a horizon time t_h and then backing down as $u_i(t_h) = F_j(-a_j(t_h))v^n + (1 - F_j(-a_j(t_h)))(-a_i(t_h))$. The function $u_i(\cdot)$ proves to play an important role in defining equilibrium strategies and establishing uniqueness. It is easily shown that $u_i(t)$ is continuous and strictly decreasing and that if audience costs increase "enough" with t there is a unique level of escalation $t_i^* > 0$ such that $u_i(t_i^*) = 0$. Loosely, if the opponent j can generate sufficient audience costs by escalating a crisis, there will be a unique level of escalation such that state *i* would be indifferent between backing down at t = 0 and at t_i^* were this the horizon. I assume in what follows that the states are able to use escalation to generate audience costs at least this large.20

Proposition 1, which follows from lemmas 1 and 2 and from the observations about $u_i(\cdot)$, establishes that if a horizon exists it is unique and is defined as $t^* = \min\{t_1^*, t_2^*\}$. To give a bit of intuition, if a crisis were expected to have a horizon longer than t^* , low-

resolve states would prefer to quit immediately rather than "bluff" up to t^* , so t^* could not be the true horizon. On the other hand, if the crisis were expected to have a shorter horizon than t^* , then at least one side would have incentives to bluff by escalation that would make equilibrium unsustainable at t^* : the signal sent by escalating would not be informative enough.

PROPOSITION 1. Let t_i^* be the unique solution to $u_i(t) = 0$ for i = 1, 2 and let $t^* = min\{t_1^*, t_2^*\}$. For any equilibrium of Γ in which escalation occurs with positive probability, the horizon must be t^* .

Equilibrium Strategies and Beliefs. Proposition 2 details the incomplete-information crisis game's equilibria that involve escalation. It indicates that there is a family of substantively identical equilibria: all have t^* as the horizon and have essentially identical behavior in the crisis up to t^* . After t^* , the states may choose any time to attack; the payoff structure leaves this open, not incorporating any incentives for either military delay or an immediate strike. (For ease of exposition, I give normal form strategies that can satisfy the perfection requirements detailed in the Appendix.)

PROPOSITION 2. Take the labels 1 and 2 such that $t^* = t_2^* \le t_1^*$. Let $k_1 \equiv u_1(t^*) \ge 0$. The following describes equilibrium strategies for state i = 1, 2 as a function of type, w_i :

For $w_i \ge -a_i(t^*)$, state i plays $\{t, attack\}$ with any $t > t^*$

For $w_i < -a_i(t^*)$, state i plays $\{t, quit\}$, where t is chosen according to any pure strategies that yield the cumulative distributions

$$\mathfrak{D}_1(t) = \frac{1}{F_1(-a_1(t^*))} \frac{a_2(t)}{v + a_2(t)}$$

for state 1, and

$$\mathfrak{Q}_2(t) = \frac{1}{F_2(-a_2(t^*))} \frac{k_1 + a_1(t)}{v + a_1(t)}$$

for state 2, both on the interval [0, t*].

The states' beliefs in equilibrium are given as follows.

For $t \le t^*$, state i believes that the probability $j \ne i$ will not back down (i.e., $Pr(w_i \ge -a_i(t^*)|t)$) is

$$\frac{v+a_i(t)}{v+a_i(t^*)}.$$

For $t > t^*$, state i's beliefs follow by Bayes' Rule in accord with the opponent's strategy for attacking. For any $t > t^*$ off the equilibrium path, let i believe that $w_i > -a_i(t^*)$ and is distributed according to F_i , truncated at $-a_i(t^*)$.

PROPOSITION 3. In any equilibrium of Γ in which escalation may occur, the equilibrium distribution on outcomes before the horizon time t^* implied by proposition 2 is unique.

An Informal Description of Equilibrium Behavior. Equilibrium behavior in the incomplete information game has the essential features of a war of nerves. At the outset, one side is expected to prefer to make concessions quietly, without a public contest. This state concedes with some probability (k_1/v) at t=0. If it does not make concessions, then its adversary immediately raises its estimate of the state's willingness to fight, and the war of nerves begins. Neither side knows whether or exactly when the other might be locked in by increasing audience costs, but beliefs that the other prefers war to making concessions steadily increase as audience costs accumulate. The reason is that states with low resolve are increasingly likely to have backed down, the more the crisis escalates. Ultimately, in crises that reach the horizon, the only sorts of states remaining have relatively high values for war on the issue. At this point, both sides prefer conflict to backing down, and both know this: attack thus becomes a rational choice.

At a price, then, audience costs enable the states to learn about each other's true willingness to fight over the interests involved in the dispute.21 The price is paid in two ways. First, a state may escalate or delay for a time and then quit when its adversary matches it. Though the state is still unsure if the adversary really would be willing to fight rather than make concessions, its belief that this is possible has increased and it finds it worthwhile to cut its losses. Second, two states may escalate up to the horizon and then fight, even though one or both would have preferred making immediate concessions rather than this outcome. The dilemma created by private information and incentives to misrepresent is that neither can reliably learn that the other would be willing to go this far without taking actions that have the effect of committing both sides to a military settlement.

One further feature of equilibrium in the model deserves comment before I turn to more specific comparative statics results. The more a crisis escalates, the less likely is either side to back down (regardless of precrisis beliefs). In technical terms, the hazard rate is decreasing: the probability that one's opponent will quit after (say) five escalatory moves is less than the probability that the state will quit after four moves. Thus, as escalation proceeds, states in the model gradually become more pessimistic about the likelihood that the adversary will concede after the next round, and outside observers become increasingly concerned that war may be "inevitable."

AUDIENCE COSTS, CAPABILITIES, AND INTERESTS IN INTERNATIONAL DISPUTES

Comparative statics analysis of the equilibrium yields theoretical insights into how three variables affect state behavior and crisis outcomes. I consider in turn the impact of audience costs, relative military capabilities, and relative interests. For expositional convenience, I discuss the case of linearly increasing audience costs, $a_i(t) = a_i t$.

Audience Costs

A striking feature of the equilibrium behavior just described is that the state less able to generate audience costs (lower a_i) is always more likely to back down in disputes that become public contests. This holds regardless of the value of the prize to either side and regardless of the states' initial beliefs about the other's resolve. Thus if actions such as mobilization generate greater audience costs for democratic than for nondemocratic leaders, we should find the democracies backing down significantly less often in crises with authoritarian states. 22

By itself, intuition can justify the opposite prediction quite easily. One might think that the side less sensitive to its domestic audience would fear escalation less. Knowing this and fearing large costs of retreat, the side with a stronger domestic audience might then be more inclined to back down. But this argument misses the signaling value of escalation for a state with a powerful domestic audience. While such a state may be reluctant to escalate a dispute into a public confrontation, if it does choose to do so this is a relatively informative and credible signal of willingness to fight over the issue. That is, the greater the costs created by escalation for a leader, the more likely the leader is to be willing to go to war conditional on having escalated a dispute. Conversely, escalation by a state that will suffer little domestically for backing down says less about the state's actual willingness to fight.23

This dynamic has several further implications. First, the signaling and commitment value of a stronger domestic audience helps a state on average, by making potential opponents more likely to shy away from contests and more likely to back down once in them. In the model, a state's ex ante expected payoff increases with its audience-cost rate a_i . This result provides a rationale for why, ex ante, both democratic and authoritarian leaders would want to be able to generate significant audience costs in international contests.

Second, if democratic leaders tend to face more powerful domestic audiences, they will be significantly more reluctant than authoritarians to initiate "limited probes" in foreign policy. Showing this formally requires that we add structure to the model analyzed here, which does not represent an initial choice of one state to challenge or threaten the other. When such an option is added—say, state 1 chooses whether to accept the status quo or to challenge state 2—it is easily shown that the less sensitive state 1 is to audience costs, the greater the equilibrium probability that the state will try a limited probe. ²⁴

Third, when large audience costs are generated by escalation, fewer escalatory steps are needed credibly to communicate one's preferences. (Formally, the expected level of escalation decreases with a_1 and a_2 .) Thus crises between democracies should see signifi-

cantly fewer escalatory steps than crises between authoritarian states—an empirically supported prediction (Russett 1993, 21).

Finally, the equilibrium results bear on the question of how regime type influences the risk that a crisis will escalate to war. When two states in the model have the same audience cost rates $a = a_1 = a_2$, the risk of war conditional on a crisis occurring proves to be *independent* of *a*, other things being equal (cf. Nalebuff 1986).²⁵ As audience-cost rates diverge, the high-audience-cost state becomes more likely to escalate, while the lower-audience-cost state becomes more likely to back down. The net effect on the risk of war may be positive or negative, although it is positive for a broad range of plausible parameter values. For example, whenever the distribution of w_1 is uniform, the risk of war (given a crisis) strictly increases as a_1 increases above a_2 . The model thus suggests a theoretical mechanism that could conceivably help explain the observation that crises between democracies and nondemocracies are more warprone than are crises between democracies (Chan 1984; Russett 1993). In the model, democratic leaders have a structural incentive to pursue more escalatory, committing strategies when they face authoritarians than when they face fellow democrats, and this can generate a greater overall chance of war.

Relative Capabilities and Interests

Two of the most common informal claims about state behavior in international crises are that (1) the militarily weaker state is more likely to back down and (2) the side with fewer "intrinsic interests" at stake is more likely to back down. These arguments are problematic. If relative capabilities or interests can be assessed by leaders prior to a crisis and if they also determine the outcome, then we should not observe crises between rational opponents: if rational, the weaker or observably less interested state should simply concede the issues without offering public, costly resistance. Crises would occur only when the disadvantaged side irrationally forgets its inferiority before challenging or choosing to resist a challenge (Fearon 1992, chap. 2).

A second striking result from the equilibrium analysis is that observable measures of the balance of capabilities and balance of interests should be unrelated to the relative likelihood that one state or the other backs down in crises where both sides choose to escalate.

In formal terms, observable capabilities and interests influence the distribution of the states' values for going to war and thus the states' initial beliefs about each other's willingness to fight (f_1 and f_2). For example, the more the balance of military power favors state 1, the more state 1—and the less state 2—is initially expected to be willing to use force. Regarding interests, the more the issues in dispute are initially thought to be important for, say, state 1, the more state 1 is initially expected to be willing to fight rather than back down.

In equilibrium, the initial distributions of the states' values for war have a direct influence on the probability that one state or the other will concede without creating a crisis. In accord with intuition, the weaker state 2 is militarily or the less its perceived stake, the more likely it is to cede the prize without offering visible resistance.²⁷ However, if it *does* choose to escalate, then the odds that state 2 rather than state 1 will back down in the ensuing contest,

$$\frac{a_1v + a_1a_2t^*}{a_2v + a_1a_2t^*},$$

are not directly influenced by relative capabilities or interests. For example, when the states have the same audience cost rates ($a_1 = a_2$), they are equally likely to back down in a crisis and equally likely to go to war, regardless of ex ante indices of relative power or interests.

Less formally, the result suggests that rational states will "select themselves" into crises on the basis of observable measures of relative capabilities and interests and will do so in a way that neutralizes any subsequent impact of these measures. Possessing military strength or a manifestly strong foreign policy interest does deter challenges, in the model. But if a challenge occurs nonetheless, the challenger has signaled that it is more strongly resolved than initially expected and so is no more or less likely to back down for the fact that it is militarily weaker or was initially thought less interested.

CONCLUSION

International crises are a response to a dilemma posed by two facts about international politics: (1) state leaders have private information about their willingness to use force rather than compromise, and (2) they can have incentives to misrepresent this information in order to gain a better deal. In consequence, quiet diplomatic exchanges may be insufficient to allow states to learn what concessions an adversary would in truth be willing to make. I have argued that states resolve this dilemma by "going public"-by taking actions such as troop mobilizations and public threats that focus the attention of relevant political audiences and create costs that leaders would suffer if they backed down. Though there are exceptions, the historical norm seems to have domestic audiences punishing or criticizing leaders more for escalating a confrontation and then backing down than for choosing not to escalate at all.

A game-theoretic analysis showed that such audience costs allow states to learn about each other's willingness to fight in a crisis, despite incentives to misrepresent. When escalation creates audience costs for both sides, states revise upward their prior beliefs that the other is willing to use force as the crisis proceeds. If escalation reaches a certain level (the "horizon"), both states prefer fighting to backing

down, and both know this. At this point, attack becomes a rational choice.²⁸

Equilibrium analysis yielded several novel propositions about how audience costs, relative capabilities, and relative interests influence the outcomes of international confrontations. Some broader implications follow.

A substantial literature in international relations argues that international anarchy, combined with states' uncertainty about each other's motivations, is a powerful cause of international conflict (Glaser 1992; Herz 1950; Jervis 1978; Waltz 1959, 1979). Unsure of each other's intentions, states arm and take actions that may make others less secure, leading them to respond in kind. States' inability to commit themselves to nonaggressive policies under anarchy may exacerbate, or even make possible, such "security dilemmas."

The results of my analysis suggest that domestic political structure may powerfully influence a state's ability to signal its intentions and to make credible commitments regarding foreign policy. If democratic leaders can more credibly jeopardize their tenure before domestic audiences than authoritarian leaders, they will be favored in this regard. For example, in the model examined here, high-audience-cost states require less military escalation in disputes to signal their preferences, and are better able to commit themselves to a course of action in a dispute.

This observation provides a theoretical rationale that might help explain why the quality of international relations between democracies seems to differ from that between other sorts of states. If democracies are better able to communicate their intentions and to make international commitments, then the security dilemma may be somewhat moderated between them. For example, the leaders of a democratic state that is growing in power may be better able to commit themselves not to exploit military advantages that they will have in future, so reducing other states' incentives for preventive attack. 30 Likewise, alliance relations between democracies may be less subject to distrust and suspicion if leaders would pay a domestic cost for reneging on the terms of the alliance, so "violating the national honor" in the eyes of domestic critics.31

One tradition within realism argues that democratic leaders are at a disadvantage in the game of realpolitik: domestic constraints reduce their freedom to maneuver and so may prevent them from playing the game as hard or as subtly as it may require (e.g., Morgenthau 1956, 512-26). However, as Schelling (1960) observes, in bargaining a player can benefit from having fewer options and less room to maneuver. I have shown how the presence of a politically significant domestic audience can improve a democratic leader's ability to commit to a course of action and to signal privately known preferences and intentions in a clear, credible fashion. These are advantages that could help in the game of realpolitik and might also make democracies better able to cope with the security dilemma.

APPENDIX

A formal statement of the solution concept used for the incomplete information game follows. Because Γ has a continuum of information sets, standard definitions of perfect Bayesian equilibrium (Fudenberg and Tirole 1991) and sequential equilibrium (Kreps and Wilson 1982) do not apply. I propose an adaptation of perfect Bayesian equilibrium, adding a refinement criterion that rules out some optimistic interpretations of out-of-equilibrium play. To avoid measure-theoretic complications, attention is restricted to pure strategy equilibria. Throughout, i=1,2 and $j\neq i$.

I begin by defining a Bayesian Nash equilibrium for the normal form version of Γ . Here, a pure strategy for state i is a map s_i : $W_i \to \mathbb{R}^+ \times \{\text{quit, attack}\}$, where \mathbb{R}^+ is the set of nonnegative reals. Using F_i , every s_i induces a unique pair of cumulative distributions $Q_i(t)$ and $A_i(t)$, which are the probabilities that state i quits or attacks by t if i follows s_i . By the properties of cumulative distribution functions, $Q_i(t)$ and $A_i(t)$ are increasing, right-continuous, and have well-defined left-hand limits for all t (Billingsley 1986, 189). Let

$$Q_i^-(t) \equiv \lim_{s \to t^-} Q_i(s).$$

"State w_i 's" expected payoff for $\{t, \text{ quit}\}$, given s_j , is then

$$U_i^{q}(t, w_i) \equiv Q_j^{-}(t)v + (Q_j(t) - Q_j^{-}(t))((v - a_i(t))/2)$$
$$+ A_j(t)w_i + (1 - Q_j(t) - A_j(t))(-a_i(t))$$

or, if $Q_i(t)$ is nonatomic at t,

$$U_i^q(t, w_i) = Q_i(t)v + A_i(t)w_i$$

$$+ (1 - Q_i(t) - A_i(t))(-a_i(t)).$$

Similarly, type w_i 's expected payoff for $\{t, \text{ attack}\}$ given s_i is

$$U_i^a(t, w_i) = Q_j^-(t)v + A_j(t)w_i + (1 - Q_j^-(t) - A_j(t))w_i$$

= $Q_i^-(t)v + (1 - Q_i^-(t))w_i$.

DEFINITION. $\{t', quit\}$ $(\{t', attack\})$ is a best reply for type w_i given s_i if

 $t' \in argmax_t U_i^q(t, w_i) \text{ and } U_i^q(t', w_i) \geq max_t U_i^a(t, w_i)$

 $(t' \in argmax_t U_i^a(t, w_i) \text{ and } U_i^a(t', w_i) \geq max_t U_i^q(t, w_i)).$

DEFINITION. (s_1, s_2) is a Bayesian Nash equilibrium for the normal form version of Γ if (1) $\{F_i, s_i\} \Rightarrow \{Q_i(t), A_i(t)\}$, and (2) under s_i , every type w_i chooses a best reply, given s_i .

Just as the normal form version of the complete information game G has multiple Nash equilibria, so are there multiple Bayesian Nash equilibria for Γ . However, many of these require states to choose strategies that do not seem optimal or sensible in the dynamic (extensive form) setting. These are ruled out by the "perfection" requirements I shall give.

In the extensive form, a complete pure strategy in Γ is a map s_i : $\mathbb{R}^+ \times W_i \to \mathbb{R}^+ \times \{\text{quit}, \text{ attack}\}$, with the restriction that if $s_i(t', w_i) = \{t, \text{quit}\}$ or $\{t, \text{ attack}\}$, then $t' \leq t$. For all $t' \geq 0$, define the "continuation game" $\Gamma(t')$ as follows: (1) payoffs are as in Γ , except beginning at t'; and (2) "initial beliefs" are given by a cumulative distribution function $F_i(\cdot; t')$ on W_i . A strategy s_i implies a strategy for state i in every continuation game $\Gamma(t')$; call this $s_i|t'$. Further, using $F_i(\cdot; t')$, $s_i|t'$ induces a pair of unique "conditional" cumulative probability distributions $Q_i(t|t')$ and $A_i(t|t')$, analogous to $Q_i(t)$ and $A_i(t)$ already defined. From these, expected payoff functions for $\Gamma(t')$, $U_i^q(t|t', w_i)$ and $U_i^a(t|t', w_i)$ follow as before.

We can now define a weak extensive form solution concept requiring that (1) s_1 and s_2 induce Bayesian Nash equilibria in every continuation game $\Gamma(t)$, and (2) beliefs $F_i(\cdot;t)$ are formed whenever possible using Bayes' Rule and s_i , while $F_i(\cdot;t)$ can be anything when Bayes' Rule does not apply.

DEFINITION. $\{(s_1, s_2), F_1(\cdot; \cdot), F_2(\cdot; \cdot)\}\$ is a perfect Bayesian equilibrium for Γ if

- (A) (s_1, s_2) induces a Bayesian Nash equilibrium in Γ and for all $t \geq 0$, $(s_1|t, s_2|t)$ induces a Bayesian Nash equilibrium in $\Gamma(t)$, using $F_1(\cdot; t)$ and $F_2(\cdot; t)$; and
- (B) for all t such that t is reached with positive probability under s_i (i.e., $Q_i(t) + A_i(t) < 1$), $F_i(\cdot; t)$ is $F_i(\cdot)$ updated using Bayes' Rule and s_i .

This solution concept is weak in the sense that it imposes no restrictions on how states would interpret completely unexpected behavior by the adversary. For instance, if state 1 escalates unexpectedly at time t, the concept allows state 2 to conclude that state 1 is without doubt the *least resolved* type \underline{w}_1 . Further, it would allow state 2 to maintain this belief even as state 1 continued to escalate. Seemingly implausible "optimistic beliefs" of this sort can be used to support a continuum of perfect Bayesian equilibria in Γ for most initial parameter values (with t^* as the maximum possible horizon). The following criterion rules out such optimistic off-equilibrium-path inferences and so refines the set of equilibria. ³² It is stronger than is needed for the proofs that follow, but it has the advantage of a very simple definition:

(C) For all
$$t > 0$$
 such that $Q_i(t) + A_i(t) = 1$, $F_i(-a_i(t); t) = 0$.

This says that if state i escalates beyond t when it was expected to have quit or attacked prior to time t, then state j believes that i's value for war w_i is at least as great as i's value for backing down at time t. In the text and in what follows, I refer to a pair of strategies (s_1, s_2) and a system of updated beliefs $F_i(\cdot; t)$ that satisfy A, B, and C as an equilibrium of Γ . I now proceed to proofs of lemmas and propositions in the text, starting with several observations (proofs for observations 2 and 4 are straightforward and are omitted).

Observation 1. Suppose that in an equilibrium of Γ , $Q_i(t)$ is atomic at t'. Then $\{t'$, quit $\}$ and $\{t'$, attack $\}$ are

never best replies for state i for any w_i and are chosen with zero probability in equilibrium.

Proof. Suppose to the contrary that in some equilibrium type w_i chooses $\{t', \text{ quit}\}$ where $Q_j(t') > Q_j^-(t')$. State w_i then receives an ex ante expected payoff of

$$Q_j^-(t')v + (Q_j(t') - Q_j^-(t'))((v - a_i(t'))/2) + A_j(t')w_i + (1 - Q_i(t') - A_i(t'))(-a_i(t')).$$

By right continuity of $Q_j(t)$ and $A_j(t)$, the deviation $\{t' + \varepsilon, \text{ quit}\}, \varepsilon > 0$, yields an expected payoff arbitrarily close to

$$Q_i(t')v + A_i(t')w_i + (1 - Q_i(t') - A_i(t'))(-a_i(t'))$$

as ε approaches 0, which is strictly greater than the payoff for $\{t', \text{ quit}\}$. Thus $\{t', \text{ quit}\}$ cannot be a best reply for any type w_i . An identical argument applies for $\{t', \text{ attack}\}$. Q.E.D.

Observation 1 implies that if in some equilibrium t_h is the horizon, it cannot be that both states choose to quit with positive probability at t_h . Further, we can now write state w_i 's equilibrium ex ante expected payoff for $\{t, \text{ quit}\}$ as

$$U_i^q(t, w_i) = Q_i(t)v + A_i(t)w_i + (1 - Q_i(t) - A_i(t))(-a_i(t))$$

and state w_i 's equilibrium ex ante expected payoff for $\{t, \text{ attack}\}\$ as $U_i^a(t, w_i) = Q_i(t)v + (1 - Q_i(t))w_i$.

Observation 2. $U_i^a(t, w_i)$ increases with $Q_i(t)$ for all w_i . Thus in any equilibrium of Γ no type of state i will choose $\{t, attack\}$ (and $A_i(t) = 0$) whenever there exists a t' > t such that $Q_i(t') > Q_i(t)$.

Observation 3. Suppose $t_h>0$ is a horizon in some equilibrium of Γ in which escalation may occur. Then for all $\varepsilon>0$ state i quits with positive probability in the interval $[t_h-\varepsilon,t_h)$ for i=1,2.

Proof. If t_h is a horizon, then by definition at least one state (say, i) must quit with positive probability in the interval $[t_h - \varepsilon, t_h]$ for all $\varepsilon > 0$. I first show that this implies that the same must hold for j. If the contrary is true, then in some equilibrium, there must exist a $t' < t_h$ such that for all $t \ge t'$, $Q_j(t) = Q_j(t')$. By observation 2, $A_j(t) = 0$ for $t < t_h$, so $U_i^q(t, \cdot) = Q_j(t)v + (1 - Q_j(t))(-a_i(t))$ for $t < t_h$. $U_i^q(t, \cdot)$ is strictly decreasing in t whenever $Q_j(t)$ is constant and less than 1, so if the contrary is true and $Q_i(t') < 1$, then no type of i would be willing to choose {t, quit} for any t > t', contradicting the hypothesis that t_h is the horizon. If $Q_i(t' < t_h) = 1$, then $\{t < t', \text{ quit}\}$ is not a best reply for any w_i , implying that $Q_i(t') = 0$. It follows that t' must equal 0— $\{t, \text{ quit}\}$ with $0 < t \le t'$ never being a best reply for any w_i —so escalation does not occur with positive probability, contradicting the hypothesis. Thus both states must quit with positive probability in the interval $[t_h - \varepsilon, t_h]$ for all

By observation 1, there can be no equilibrium in which both states choose $\{t_h$, quit} with positive

probability. Thus in any equilibrium with t_h as the horizon, at least one state (say, i) quits with positive probability in the interval $[t_h - \varepsilon, t_h)$ for all $\varepsilon > 0$. If observation 3 is false, then it must be possible to have an equilibrium in which $Q_i(t)$ is atomic at t_h but j does not quit with positive probability in an interval $[t_h - \delta, t_h)$ for small-enough $\delta > 0$. But then $Q_j(t)$ will be constant and less than 1 for $t \in [t_h - \delta, t_h)$, so by the same logic as in the last paragraph, i will not be willing to choose $\{t, \text{ quit}\}$ with $t \in [t_h - \delta, t_h)$, contradicting the hypothesis.

OBSERVATION 4. Suppose that t_h is the horizon in some equilibrium of Γ . By observations 2 and 3, for j=1,2, $A_j(t)=0$ for $t< t_h$. Thus $\{t>t_h$, attack} yields an ex ante expected payoff of $U_i^a(t,w_i)=Q_j(t_h)v+(1-Q_j(t_h)w_i)$, while $\{t< t_h, quit\}$ yields $U_i^q(t,\cdot)=Q_j(t)v+(1-Q_j(t))(-a_i(t))$. Since $U_i^q(t,\cdot)$ is independent of w_i , for all t such that $\{t, quit\}$ is a best reply for state w_i , $U_i^q(t,\cdot)$ must equal a constant (call it k_i).

Proof of Lemma 1. Suppose to the contrary that there exists an equilibrium of Γ in which escalation may occur and in which $Q_i(t)$ is strictly increasing for all t for some state i. By observation 2, $A_j(t) = 0$ for all $t \ge 0$ (since for all $t \ge 0$ there exists a t' > t such that $Q_i(t') > Q_i(t)$). And, by observation 4, t's equilibrium expected payoff for $\{t, \text{ quit}\}$ is

$$U_i^q(t,\cdot) = Q_i(t)v + (1 - Q_i(t))(-a_i(t)) = k_i$$

implying that

$$Q_j(t) = \frac{k_i + a_i(t)}{v + a_i(t)} (*).$$

However, because $A_i(t) = 0$ for all t, it must be that

$$\lim_{t\to\infty}Q_j(t)=1.$$

From (*), this will be possible only if

$$\lim_{t\to\infty}a_i(t)=\infty$$

or if $k_i = v$, both of which generate contradictions. If

$$\lim_{t\to\infty}a_i(t)=\infty,$$

then no type of i will be willing to choose $\{t, \text{ quit}\}$ for arbitrarily large t, if $\Gamma(t)$ actually occurred. If $k_i = v$, then $Q_j(0) = 1$, implying that j does not escalate with positive probability. Q.E.D

Proof of Lemma 2. Part 1 follows immediately from observations 2 and 3. As for the second part, fix an equilibrium in which escalation may occur and there exists a horizon t_h . Let T_i be the set of times such that for all $t \in T_i$, $Q_i(t)$ is either atomic or strictly increasing. Observations 3 and 4 imply that \tilde{t} 's ex ante expected payoff for $\{t \in T_i$, quit $\}$ is

$$U_i^q(t, \cdot) = Q_j(t)v + (1 - Q_j(t))(-a_i(t))$$

= $Q_j^-(t_h)v + (1 - Q_j^-(t_h))(-a_i(t_h)) = k_i$.

Also from observation 4, type w_i 's ex ante expected payoff for $\{t > t_h$, attack $\}$ is

$$U_i^a(t, w_i) = Q_i(t_h)v + (1 - Q_i(t_h))w_i$$

which is at least as great as $U_i^a(t_h, w_i)$.

There are now two cases to consider. First, if $Q_i^-(t_h)$ = $Q_i(t_h)$, then i's ex ante expected payoff for $\{t < t_h\}$ quit, $t \in T_i$, is $Q_i(t_h)v + (1 - Q_i(t_h))(-a_i(t_h))$, which implies that i does better to choose $\{t \ge t_h$, attack if $w_i > -a_i(t_h)$ and only if $w_i \ge -a_i(t_h)$. Second, if $Q_i^-(t_h)$ $< Q_i(t_h)$, then there exists a $\hat{w}_i < -a_i(t_h)$ such that type \hat{w}_i is indifferent (ex ante) between $\{t > t_h$, attack $\}$ and $\{t < t_h, \text{ quit}\}\$ and thus a measurable set of types $W_i' \equiv$ $(\hat{w}_i, -a_i(\hat{t}_h))$ that strictly prefer $\{t > t_h, \text{ attack}\}$ to $\{t < t_h, t_h\}$ quit}. But this is impossible. The action $\{t > t_h$, attack} yields $Q_j(t_h)v + (1 - Q_j(t_h))w_i$, while $\{t_h + \varepsilon, \text{ quit}\}$ yields $Q_j(t_h)v + A_j(t_h + \varepsilon)w_i + (1 - Q_j(t_h) - A_j(t_h + \varepsilon))(-a_i(t_h + \varepsilon))$. If $A_j(t_h) \neq 1 - Q_j(t_h)$, then for small enough $\varepsilon > 0$, the quit strategy does strictly better for all $w_i \in W_i$. If $A_i(t_h) = 1 - Q_i(t_h)$, then all $t > t_h$ are off the equilibrium path. Condition C implies that for t > t_h , $F_i(-a_i(t); t) = 0$, so $Q_i(t|t_h) = 0$ in all $\Gamma(t)$ for $t > t_h$. But if j will not quit after t_h , then $\{t > t_h$, attack\} cannot be a best reply in the continuation games $\Gamma(t > t_h)$ for types in W_i . Thus $Q_j^-(t_h) < Q_j(t_h)$ is impossible in any equilibrium with $t_h > 0$ as the horizon, and the first case must hold.

Proof of Proposition 1. From lemma 2, it follows that in any equilibrium with horizon $t_h > 0$, the ex ante probability that state j chooses $\{t \ge t_h$, attack $\}$ is $1 - F_j(-a_j(t_h))$. Thus, using observation 3, $U_i^g(t_h - \varepsilon, \cdot)$ can be made arbitrarily close to $F_j(-a_j(t_h))v + (1 - F_j(-a_j(t_h))(-a_i(t_h))$, which, by consequence, must equal k_i .

Choose labels such that $t_2^* \le t_1^*$. I show first that t_h cannot be strictly greater than t_2^* in any equilibrium. If it were, then state 2's payoff for $\{t_h - \varepsilon, \text{quit}\}$ would be $k_2 = F_1(-a_1(t_h))v + (1 - F_1(-a_1(t_h)))(-a_2(t_h)) < 0$, which is impossible. Because $A_1(0) = 0$, state 2 can assure itself at least 0 by the strategy $\{0, \text{quit}\}$, and so state 2 would not be willing to choose $\{t, \text{quit}\}$ for any t > 0, contradicting observation 3.

Nor can t_h be strictly less than t_2^* . If it were, then both states must expect an equilibrium payoff $k_i = F_j(-a_j(t_h))v + (1 - F_j(-a_j(t_h)))(-a_i(t_h)) > 0$ for $\{t < t_h, \text{quit}\}$. Since for $t \in T_i$, $t < t_h$, $k_i = Q_j(t)v + (1 - Q_j(t))(-a_i(t))$, $k_i > 0$ implies that for both states there must exist a $t_i' \geq 0$ such that $Q_j(t)$ is atomic at t_j' and such that $Q_j(t) = 0$ for all $t < t_j'$. If this were not the case, then for one state, $Q_j(t)$ would require types of j to play $\{t$, quit} when this yielded a payoff of 0 or less, which could not be a best reply for any type. Moreover, it must be the case that $t_1' = t_2'$; if not, then for state i with $t_i' < t_j'$, $\{t_i'$, quit} yields a payoff less than or equal to zero. But this contradicts observation 1, since in no equilibrium can both states quit with positive probability at the same time. Thus t_h must equal t_2^* in any equilibrium of Γ and thus, $t^* > 0$ is unique.

Proof of Proposition 2 (Sketch). That the proposed strategies form a Bayesian Nash equilibrium in Γ follows immediately from lemma 2 and the fact that $\mathfrak{D}_j(t)$ is chosen so that all types $w_i < -a_i(t^*)$ are indifferent among $\{t, \text{ quit}\}$ for all $t < t^*$ ($k_1 = u_1(t^*)$ and $k_2 = 0$). For the continuation games $\Gamma(t')$, $t' \geq 0$, Bayes' Rule implies that if $\{F_i, s_i\} \Rightarrow \{Q_i(t), A_i(t)\}$, then

$$Q_{i}(t|t') = \frac{Q_{i}(t) - Q_{i}(t')}{1 - Q_{i}(t') - A_{i}(t')}$$

and

$$A_{i}(t|t') = \frac{A_{i}(t) - A_{i}(t')}{1 - Q_{i}(t') - A_{i}(t')},$$

for t > t' and t' such that $Q_i(t') + A_i(t') < 1$. Notice that when they are defined, $Q_i(t|t')$ and $A_i(t|t')$ are linear transformations of $Q_i(t)$ and $A_i(t)$, respectively. This fact can be used to show that if $\{t, \text{quit}\}$ is a best reply for type w_j given s_i , then it remains so in all continuation games $\Gamma(t' \leq t)$, provided that $Q_i(t') + A_i(t') < 1$ under s_i (and likewise for $\{t', \text{attack}\}$). Thus the strategies given for types given in the proposition form Bayesian Nash equilibria in every continuation game $\Gamma(t')$ up to the earliest time t'' such that for one of the two states, $Q_i(t'') + A_i(t'') = 1$ (if t'' exists). It is straightforward to show that beliefs off the equilibrium path (t > t''), if t'' exists) accord with condition C.

Proof of Proposition 3 (Sketch). By proposition 1, the horizon must be t^* in any equilibrium in which escalation may occur. The only question, then, is whether there are equilibrium distributions on outcomes up to t^* that differ from $\mathfrak{D}_1(t)$ and $\mathfrak{D}_2(t)$. Arguments similar to those for observations 1 and 3 establish that any equilibrium quit distributions must be nonatomic and strictly increasing on $[0, t^*)$. But $\mathfrak{D}_1(t)$ and $\mathfrak{D}_2(t)$ are the only nonatomic strictly increasing distributions that make types $w_i < -a_i(t^*)$ willing to quit at any $t \in [0, t^*)$ and also support t^* as the horizon.

Notes

Presented at the annual meetings of the American Political Science Association, Washington, 1993. Thanks to Atsushi Ishida, Andy Kydd, Robert Powell, Jim Morrow, Matthew Rabin, and Barry Weingast for comments.

Rabin, and Barry Weingast for comments.

1. Studying the "diplomacy of insults," Barry O'Neill (n.d.) independently developed an attrition model of international contests that focuses on this same second feature.

2. For the original discussion of costly signaling in economics, see Spence 1973. On cheap talk (which may be informative in some contexts), see Farrell 1988; Crawford and Sobel 1982; Rabin 1990. The crisis signals discussed herein are atypical in that they create costs that are paid only if the signaler takes a certain future action ("backing down") rather than regardless of what the signaler does in the future (as in Spence's classical case). One implication is that these signals can have a commitment (or "bridge-burning") effect. For a discussion of costly signaling in crises, see Fearon 1992, chaps. 3 and 4, and for the seminal treatment of signaling in international relations, see Jervis 1970.

3. For example, the financial costs of sustained mobilization do not appear as a significant factor in the case studies found in Betts 1987; George and Smoke 1974; Lebow 1981; or Snyder and Diesing 1977.

4. Time preferences or opportunity costs are the main rationale given for the costs of delay in models of buyer-seller bargaining. For an overview of these models, see Fudenberg and Tirole 1991, chap. 10. On time preferences in crisis bargaining, see also Morrow 1989, 949.

5. Schelling's own account views the threat that leaves something to chance primarily as a tactical move available to both sides, rather than as a mechanism for revealing private information about resolve (see Fearon 1992, chap. 3; Maxwell 1968).

6. On the importance of distinguishing between "loss of control" in a crisis due to pure accident and "loss of control" due to unanticipated but deliberate decisions, see Powell 1985

7. Trachtenberg (1991) shows convincingly that the German need to mobilize and attack before Russian mobilization was far advanced was known in both Berlin and St. Petersburg.

8. See the citations in n. 3. I consider how first-strike advantages affect crisis bargaining and escalation in work-in-progress

9. A number of examples from Balkan conflicts are discussed in Fearon 1992, 184–85.

10. According to Norris, "Pitt was conscious that he must negotiate an agreement acceptable to the new parliament when it met in the autumn, or face political annihilation" (1955, 574). The terms that would have been acceptable to the parliament were (intentionally) made harsher by Pitt's public escalation of the crisis. Spain ultimately backed down and Pitt was much praised for his diplomatic triumph.

11. Significant American examples include the heat Acheson took for "losing China" and Johnson's and Nixon's fears about domestic criticism for sending the wrong signal to the communists over Vietnam) (see, e.g., Gelb and Betts 1979, 220–26.

12. Alternatively, domestic audiences may draw harsh inferences about a leader's competence if the leader backs down in a crisis after escalating. If they do, then this would also create an audience cost that would be felt more strongly in democratic states. On the use of incentive schemes to improve an agent's bargaining power, see Katz 1991.

13. For discussion and citations, see Fearon 1992, chap. 3. 14. Nothing important changes if w_1 and w_2 are allowed to

14. Nothing important changes if w_1 and w_2 are allowed to be greater than zero but less than the value of the prize. Also, there is no loss of generality in setting both sides' value for the prize equal to v.

15. Payoffs have been defined except for simultaneous quits or attacks, which do not play much of a role in the sequel. If one state chooses to attack at t and the other chooses to quit or attack at the same time, they receive (w_1, w_2) . If both quit at time t, state i receives $(v - a_i(t))/2$.

The assumption that the winner gets v independent of the amount of escalation makes the analysis more tractable without discarding a key feature of crises that distinguishes them from the classical war of attrition, namely, that only one side pays the costs of escalation if a player quits. The assumption that w_i does not depend on t is also made for tractability; it would be both interesting and desirable to relax it. Discounting is omitted for simplicity and so that I can focus on the independent impact of audience costs.

16. For all values of w_1 and w_2 , there is one other outcome obtainable in a subgame perfect equilibrium, this one involving the play of weakly dominated strategies: both sides choose $\{0, \text{ attack}\}$. If each expects the other to attack immediately, neither has an incentive to deviate. This equilibrium disappears in an alternating-move version of the game. If the two states would be locked in at the same time, then there are three equilibrium outcomes (beyond the weakly dominated one): either one of the two states quits immediately or both play a mixed strategy up to the lock in time.

17. I have omitted proofs of the comparative statics results. These are available on request, along with less-compressed versions of the proofs given here.

- 18. I assume also that f_1 and f_2 are independent, which is most naturally interpreted to mean that uncertainty is about the opponent's cost-benefit ratio for war rather than about military capability. For a discussion of this issue, see Fearon
- 19. This interpretation of $u_i(t_h)$ as i's expected utility for $\{t_h, t_h\}$ quit} is valid only if i neither quits nor attacks with positive probability at t_h , but the proofs do not depend on the interpretation.

20. If they cannot (e.g., if $a_1(t) = a_2(t) = 0$ for all t), then there may not exist an equilibrium in which learning occurs.

21. In the linear case, as the audience-cost rates a_1 and a_2 approach zero, the horizon time t* approaches infinity, meaning that an arbitrarily large amount of delay or escalation is required to credibly signal willingness to fight.

22. In formal terms, the probability that state 1 will back down prior to the horizon time is $a_2t^*/(v + a_2t^*)$. The probability that state 2 will do the same, conditional on the crisis occurring (i.e., lasting longer than t = 0), is $a_1 t^* / (v + a_1 t^*)$. Thus if $a_1 > a_2$, state 2 is more likely to back down than state 1, and vice versa. This result holds for any precrisis beliefs, f_1 and f_2 .

23. The probability that state i will fight conditional on a crisis occurring is $v/(v + a_i t^*)$, $j \neq i$, and t^* proves to rise as a_i

falls, implying the result in the text.

24. In the Cold War period the Soviet Union appeared generally more willing than the United States to threaten the use of military force and then back off or moderate on meeting resistance. This, at any rate, is a reading consistent with standard interpretations of the set of major Cold War crises (e.g., Betts 1987; George and Smoke 1974; and Snyder and Diesing 1977). Certainly the United States has used force on many occasions in Latin America and elsewhere, but military probes to gauge other parties' willingness to resist appear uncommon. Maoz and Russett (1992, 253) report that 62% of 271 post-1945 crises between democracies and "autocracies" were initiated by the autocracy—a number that would extremely unlikely to occur if democracies were just as likely to try military probes.

25. Relevant other things will not be equal if democratic leaders tend to have higher (audience) costs for fighting wars than do nondemocratic regimes. Indeed, the same argument that suggests that democratic leaders will suffer more politically for backing down after escalating a crisis suggests that they will be more sensitive to potential war costs. In the model, crises are less likely to escalate to war the greater are the states' costs for fighting (or, equivalently, the lower v).

26. When the distribution of w_1 is logistic up to $w_1 = 0$, the probability of war given a crisis (Pr(war|crisis)) increases as a_1 increases above a_2 whenever the median value of w_1 is sufficiently low. For example, let $F_1(z) = (1 + \exp(-z - m))^{-1}$ for z < 0, and $F_1(0) = 1$. If v = 1, then the result holds whenever m > 1, which means that the typical state 1 is not willing to run 50% risk of war for the prize. Ultimately, for highly asymmetric situations (very large a_1 , very small a_2), Pr(war|crisis) begins to decrease with a_1 in the logistic case. For instance, if v = 1 and m = 5, Pr(war|crisis) is .045 when a_1 = a_2 , and reaches a maximum at .11 when a_1 is about 50 times greater than a_2 .

27. The probability that state 2 backs down at t = 0 is k_1/v . A shift in the balance of power (understood as the probability that 1 would win a war) shifts f_1 to the right and f_2 to the left; this has the consequence of increasing k_1 . An increase in the intensity of state 1's interests at stake shifts f_1 to the right (without affecting f_2), which also has the consequence of

increasing k_1 .

28. An important limitation of the attrition model of crises (a limitation common to most other models of "crisis bargaining") is that it gives states only two ways to resolve a dispute peacefully: one side or the other must "back down." While some evidence suggests that many crises in fact have this aspect (Snyder and Diesing 1977, 248), we would like to know why. A natural next step is to consider models with continuous-offer bargaining (e.g. Fearon 1993; Powell 1993).

29. There is in fact a large set of different arguments

lumped under the "security dilemma" heading, but this criticism cannot be pursued here. For a critique of standard 'security dilemma" reasoning, see Kydd 1993.

30. Schweller (1992) provides some evidence suggesting that democracies neither engage in nor are the targets of preventive war. Fearon (1993) shows that between rationally led states, preventive war arises from the rising power's inability to commit not to exploit the future bargaining advantage it will have.

31. A similar argument about alliances is developed by Gaubatz (1992), who presents evidence indicating that alliances between democracies last longer than alliances involv-

ing nondemocracies. See also Fearon 1992, 355

32. The refinement is in the spirit of Cho and Kreps (1987) D1 criterion. Even without the refinement, comparative statics results are only marginally weakened for the set of perfect Baysian equilibria of Γ.

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PEACE, WAR, AND THE BALANCE OF POWER

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Levery possible relation between the distribution of power and the likelihood of war has been defended somewhere in the literature on international politics: war is least likely if power is distributed equally, war is least likely if power is distributed unequally, and the distribution of power has no effect on the likelihood of war. I try to settle this dispute by examining the effect of expectations about the outcome of war on the choice between war and negotiation. I argue that each of these mutually contradictory propositions can be derived from some plausible set of premises and thus that which one is correct depends on which set of premises best describes a situation. The most important factors affecting the relation between the distribution of power and the likelihood of war are (1) whether the terms of a compromise agreement that might be accepted in lieu of war affect the distribution of power between the antagonists and therefore the probability that the agreement will be enforced and (2) how many states' interests will be affected by the outcome.

The change in the distribution of power caused by the collapse of the Soviet Union has led to renewed interest in the relation between the global distribution of power and the likelihood of war. More recently, there has been controversy about whether increasing the military strength of the Bosnian forces in the Yugoslavian civil war would make a settlement of that conflict more likely. Unfortunately, the literature on international politics is not much help in thinking about questions such as these, since every possible relation between the distribution of power and the likelihood of war has been defended somewhere in it: war is least likely if power is distributed equally (Claude 1962, 56-66); war is least likely if power is distributed unequally (Blainey 1988, 108-24; Organski 1958, 292-93); and the distribution of power has no effect on the likelihood of war (Wittman 1979, 749-51). Moreover, the empirical literature on this subject is equally divided.1

Because the arguments that have been offered in support of these contradictory propositions have often been incomplete and intuitive, it seems plausible that the cause of this disagreement has been sloppy reasoning and that the right answer can be found by being more careful about what conclusions follow from what premises. One might therefore be tempted to believe that if one of these propositions can be derived from a formal model it must be true and the others false. I will argue that such an inference would be wrong. I will show how each of these contradictory propositions can be derived from some plausible set of premises and therefore argue that the answer to the question depends on which premises best describe a situation.

Any formal model supporting one or another of these contradictory propositions would be complex. I cannot here fully develop several formal models to illustrate the implications of various alternative sets of assumptions and then compare them. I shall therefore merely sketch several lines of argument that

could provide the basis for the construction of formal models, referring the reader to examples in the literature whenever possible. Formal theorists will find this insufficiently rigorous. My excuse is that in this case one must choose between generality and rigor, and some rigorous models in the literature have made unwarranted claims to generality.

First, I shall summarize the reasons that scholars have given in support of each of the various possible relations between the distribution of power and the likelihood of war. I will argue that the sources of their disagreement lie in the degree to which they treat attackers and defenders symmetrically, the assumptions they make about the relation between the balance of forces and the expectations of decision makers, and the assumptions they make about the alternative to war. While they differ in these respects, however, all writers on this subject tend to assume that any conclusions that one might draw about the effects of the distribution of power in a world of two states also hold true in a world of more than two states. Subsequently, I shall consider what justification there is for any of the assumptions these writers have made. I will begin by examining the relation between fighting and negotiating in a world of two states and then examine the effect of adding more states.

The main conclusion is that the most important factor affecting the relation between the distribution of power and the likelihood of war is whether the terms of a compromise agreement that might be accepted in lieu of war affect the relative power of the antagonists and therefore the probability that the agreement will be enforced. If they do, then a second important factor is the number of states whose interests will be affected by the outcome. Thus the answer to the question of what relation one should expect between the distribution of power and the likelihood of war is, "It depends." The point of the analysis is to try to make clear what it depends on.

THE SOURCES OF DISAGREEMENT

The idea that war is least likely if power is distributed equally seems so obvious to many people that it is rare that anyone feels the need to justify it. However, it is clearly based in part on two assumptions: (1) the less likely a state is to be successful, the less likely it is to start a war; and (2) the smaller a state's military forces are relative to its adversary's, the less likely it is to be successful. If one state is known to have no incentive to use force even if it expected to be successful, these assumptions imply that the likelihood of war could be minimized by making that state as powerful as possible relative to its potential adversaries. But since there is no reliable way of knowing which states, if any, will never be tempted to use force, most writers assume that all states are equally likely to contemplate the use of force if the prospects of success are favorable. Many people infer from these assumptions that the probability that either of two potential adversaries will start a war must be least when their forces are equal (see, e.g., Claude 1962, 56; Mearsheimer 1991, 154).²

However, if we assume (as is commonly done) that there are only two possible outcomes of war (i.e., one state wins or the other does), then this conclusion does not follow from these premises. Since the distribution of power between two potential combatants affects the probability of success for both and the probabilities of success must sum to one, decreasing one state's probability of success must increase the other's by the same amount. Thus it is not clear why any decrease in the first state's incentive to use force should not increase the other's by the same amount, leaving the probability of war unchanged. If so, the distribution of power would be unrelated to the probability of war.

The simplest (but not the only possible) answer to this objection is that there are three possible outcomes of war, not just two: one or the other side wins, or there is a stalemate. The probability that the war will end in stalemate is greatest when the forces of the two sides are equal. In that case the sum of the probabilities that each state will win is least when their forces are equal. Note, however, that while the probability that either state will use force may, as a result, be least in this situation, both states may still have an incentive to use force that is uncomfortably large.

This reasoning assumes that wars are caused by the states that initiate them, so that one has only to examine the incentives of aggressors to estimate the probability of war. One justification for the claim that inequality of power minimizes the probability of war is based on the fact that it takes at least two states to make a war and assumes that the reasoning just applied to attackers also applies to defenders. If states that are unlikely to be successful are unlikely to use force, then weak defenders are least likely to resist an attack. And if defenders do not resist the demands of aggressors, then wars will not occur (Organski 1958, 292–93). Thus the greater the inequality of power, the

less the probability of war. This reasoning implies that if the likelihood of war is to be minimized, not every state can expect to be able to defend its interests.³

However, this argument only implies that given an attack, the probability that it will lead to war is less, the greater the superiority of the attacker. But the previous argument implies that the probability of an attack is least, the more nearly equal the two side's forces are. Thus the two together imply that the probability of war is greatest for distributions somewhere between equality and great inequality. To get the conclusion that the probability of war is minimized by inequality, one must show that there is some degree of inequality such that the probability that the victim of an attack will resist is smaller than the probability that a state will choose to attack when their forces are equal. But if defenders must choose between fighting against a superior force and capitulation, they may be willing to fight even if the chances of success are very small. Therefore, the degree of inequality that would be necessary to minimize the probability of war might have to be very great indeed.

However, because war is costly and its outcome uncertain, there ought to be some negotiated settlement that both sides would prefer to the costly lottery associated with war-in which case, the alternative to war will be not capitulation but a negotiated settlement. But then, since the value of any proposed agreement must be compared to the expected value of going to war, the minimum terms that each side will accept will be influenced by how optimistic it is about the outcome of war. Since the balance of power (i.e., the relative size of each side's military forces) influences the expected outcome of war, it influences the minimum terms that each will accept. But if a change in the balance of power makes one state more optimistic about the outcome of war and therefore leads it to increase its demands, it must make the other more pessimistic and therefore willing to accept a less favorable settlement. This is the basis for the claim that a change in the balance of power affects the terms of the settlement that states would accept as an alternative to war but not the probability that war will occur (Fearon 1992; Wittman 1979, 749-51).

But this reasoning assumes that decision makers' expectations about the outcome of war are consistent. If they are not and both sides are optimistic about the outcome, then there may be no negotiated settlement that they both prefer to war (Wittman 1979, 755–57). This provides another justification for the view that war is least likely when power is distributed unequally: Blainey (1988) claims that it is more likely that both antagonists will be optimistic when power is distributed equally than when it is distributed unequally and therefore that states are most likely to be able to agree on a negotiated settlement as an alternative to war when power is distributed unequally. According to Blainey, this explains why wars can be ended by negotiations even though negotiations failed to prevent them. 4 However, Blainey does not make clear why there should be a relation between the distribution of power and the consistency of two states' expectations concerning the outcome of

a possible war between them.

These conflicting lines of argument raise two fundamental questions: (1) Is a negotiated settlement always available as an alternative to war? and (2) If a negotiated settlement is available, how does the relative military power of the adversaries affect their choice between war and negotiation? I shall focus on the second question first. To answer it we must examine the relation between fighting and negotiating.

FIGHTING AND NEGOTIATING

Although war and negotiation are usually presented as alternatives to each other, I shall claim that war is best understood as part of a process of negotiation. Thus, while adversaries can certainly choose to negotiate rather than fight, if they fight it is because each sees fighting as a way to influence the outcome of negotiations. The place to begin, therefore, is not with an examination of war but with an examination of bargaining.

Bargaining

The essential feature of bargaining is that two or more actors will be better off if they coordinate their actions than if they do not but that they have conflicting preferences as to which actions are to be selected from some larger feasible set. The paradigmatic example is two people who can divide a sum of money between them if and only if they agree on how it is to be divided. If each of them is interested only in receiving the largest possible amount of money, then what each will be willing to accept will depend on his expectation of what the other is willing to accept. This is the sort of problem that game theory is supposed to help us analyze. It has proved, however, to be extraordinarily resistant to analysis. The purpose of most discussions of bargaining is to defend one or another solution to this problem. However, the question of what agreement rational bargainers will accept is less relevant to understanding why states prefer fighting to negotiating than the question of how long it will take them to accept it, a question that most bargaining theory has little to say about. Thus it may be more relevant to emphasize why what is commonly called "the bargaining problem" is such a hard problem.

Consider, then, two people in a room discussing what agreement they will sign, and suppose one of them demands 90% of the money. This may seem outrageous to the other, but if that is the only agreement the person making this demand is willing to accept, then the only rational choice is for the other to accept it, since 10% of the money is better than nothing. But this reasoning applies to either bargainer. Suppose, then, both demand 90% of the money. Then they will get nothing unless one or the other changes his demand. But which should it be?

Suppose after they have been deadlocked for a while one of them considers whether to give in. After all, deadlock could last forever, and it seems reasonable that both bargainers would prefer to accept something now to getting some larger sum later. But precisely because this is true of both, it is as good a reason for either to decide to wait for the other to give in as it is a reason to give in oneself. And this remains true no matter how long they have been deadlocked: if the other is about to give in, it is better to wait for his concession than to give in to him unnecessarily, no matter how much time has already passed.

Consider now the possibility of offering to accept a compromise instead. By conceding some of what the other is demanding, one reduces the potential gain to him of holding out for more: the difference between 90% and 50%, for example, is much less than the difference between 90% and 10%. Perhaps, then, a concession will lead to agreement. However, if the other expects the first concession to be followed quickly by another, then he may reason that while the potential reward to stubbornness has diminished, the probability of getting the reward has increased. Thus a concession may simply make the other more recalcitrant rather than less. But instead of offering further concessions, the bargainer who made the first one may decide that it had the wrong effect and offer no more.

The reason it is difficult to say what individuals should do in an unstructured face-to-face bargaining situation such as this is that every possible division of the money is supported not just by an equilibrium combination of strategies but by a sequentially rational equilibrium. There are two ways of circumventing this indeterminacy: one can extend the definition of rational behavior to include a variety of intuitions as to how a rational person should respond to it, or one can impose some further structure on the situation, (e.g., by requiring the bargainers to communicate offers to each other only in a certain order and at certain fixed intervals).5 However, any solution that is based on understanding the independent choices of the two bargainers must specify a bargaining strategy for both such that given the behavior specified by the strategy, at the time of the predicted agreement neither bargainer could expect to gain by waiting instead of accepting the prescribed agreement. But since the effect of waiting depends on how the other bargainer responds to it, it must be common knowledge that the prescribed solution is sequentially rational and therefore the value the bargainers place on agreements *now* relative to agreements *later* must also be common knowledge. Thus it is not surprising that two bargainers will often both be optimistic about the effect of holding out for better terms, and it seems entirely possible that even rational bargainers with a good understanding of the problem might take so long to reach an agreement that, had they anticipated the outcome, they both would have preferred to accept the other's offer at the outset.6

Bargaining and Fighting

Now suppose that one bargainer grabs the other's arm and twists it painfully behind him while demanding that he give in. Because this has the effect of increasing the cost of disagreement for the victim, standard solutions to the bargaining problem imply that this ought to worsen the terms of the bargain that he ultimately accepts. Note, however, that while this assault may mean that the victim is willing to surrender all the money to the other bargainer, it does not necessarily mean that. While the costs of disagreement are now much greater for the victim than for the attacker, the attacker presumably still (as before) prefers an agreement now to an agreement later and might therefore be willing to agree to less than everything in order to shorten the time to agreement. In other words, this assault may not deprive the victim of all bargaining power. This is because the one who is twisting the other's arm still requires the cooperation of his victim. If not (e.g., if he can simply take the other's money once he is no longer able to resist), then by disabling the other he can take everything.

Perhaps, however, the victim of this maneuver can evade the other's grip and retaliate. If he succeeds in twisting the arm of the first, then the bargaining advantage shifts to him. If not, then a struggle may ensue. At every moment during this struggle there are three possibilities: either bargainer may succeed in pinning the other down, or the struggle may continue. The struggle itself is unpleasant, however, and therefore either bargainer may decide he prefers to offer a compromise in hope that the other will agree and they can quit. Perhaps they might pause in their struggle to consider further offers and counteroffers, or perhaps they might continue to communicate while struggling. Either way, they must, as before, evaluate the terms of any possible offer relative to the possibility of doing better by holding out. However, the value of holding out has now become rather complex. It must include an evaluation of the unpleasantness associated with the struggle, an estimate of how long it might last, an estimate of the probability that either might ultimately emerge as victor, an estimate of the value of each of the possible agreements that might be accepted in the event that either won the struggle, along with the probability associated with each, and an estimate of the range of possible agreements that the other might accept later in the struggle along with the probability associated with each and how long it would take before the other was willing to accept them. Therefore there are now two reasons why bargaining may take so long as to be self-defeating: each can continue to be optimistic that the other will soon tire of the contest and offer a concession, and each can continue to be optimistic that he will soon defeat the other and gain an important bargaining advantage.

Moreover, the use of force adds a qualitatively different element to this situation. Suppose our two bargainers have struggled inconclusively for a while and that one offers a compromise based on his expectation of the consequences of continuing the struggle. If he releases his grip in response to the other's apparent agreement, this may allow the other bargainer to grab him in a hold he would be unable to break and compel him to accept a much worse agreement than the one he offered. Thus, because no one can compel a bargainer not to renege on an offer he has made, once the struggle has begun, neither bargainer may be willing to consider a compromise until its outcome has been decided.

This rather fanciful story about a wrestling match began with one bargainer trying to seize the other in a painful grip in order to force him to agree to a deal that was less favorable to him (and therefore more favorable to the attacker). Before attacking his fellow bargainer, however, he could merely threaten to do it, and the other could then threaten to retaliate in kind. They could then make demands and counterdemands of each other based on those threats. Since no one would have tackled anyone at that point, however, it is possible to doubt that anyone would or that if one did, the other would actually resist. Moreover, even if he did resist and were successful in avoiding being pinned down, he might quickly offer a compromise settlement. Thus each bargainer must choose between accepting some proposed agreement and using force to try to extract a better one. If they both decide to use force, this is not because they have decided not to bargain but because they believe fighting will improve their bargaining position.

Each bargainer's expectations of the consequences of rejecting any particular proposal can be divided into two categories: expectations about the outcome of bargaining in various possible circumstances (including the outright victory of one or the other bargainer) and expectations about the outcome of a fight. How many possible outcomes of a fight are there? One plausible answer is that there are three: one or the other loses, or there is a stalemate. Thus expectations should take the form of a probability distribution over these three possibilities. However, one could argue that no stalemate could last foreverthat one or the other would eventually tire and have to submit, though this might take a long time. In that case, one might want to say that there are only two possible outcomes: one wins, or the other does. But if they care about how long it takes for one to win (as surely they would), then we would have to say that there are infinitely many possible outcomes: a victory for one or the other at every possible time at which it might occur.

In thinking of their expectations concerning the outcome of fighting, one must distinguish between how uncertain they are and how consistent their expectations are. They are maximally uncertain if they believe all possible outcomes are equally likely. Their expectations are consistent, however, only if the probability they each assign to every outcome is the same. If they are both maximally uncertain, then their expectations must necessarily be consistent. If not and if each has private information relevant to

estimating the probability of the outcome, then their expectations may be inconsistent.

We began by assuming that these two people were bargaining over the division of a sum of money. Before force was introduced into the story, this money would have to be a potential addition to the wealth of each bargainer, so that the cost of failing to agree was simply that they would fail to gain anything. Once force is introduced, however, this is no longer true, since either might be willing to surrender money he already possessed to avoid further physical punishment, and the cost of failing to agree is continuation of this punishment (if bargaining takes place after fighting begins) or the possibility that one may be subjected to this punishment (if bargaining takes place before fighting begins). The distribution that exists prior to bargaining is one possible outcome of bargaining but not the only one, even if an agreement is reached without fighting. Thus whether these people can avoid fighting and whether they can both protect the assets they had at the outset are two different questions.

Suppose now that it is common knowledge that in the event force is used, one would quickly subdue the other (perhaps because he is a muscular athlete while the other is an uncoordinated wimp). Should they then be able to reach an agreement without the need to use force? If so, they must both prefer the expected value of bargaining prior to the use of force to the expected value of first fighting and then bargaining. If they have sufficiently conflicting expectations of the outcome of bargaining after fighting this may not be true, since by resisting when he has no chance of success one bargainer might be able to influence the other's expectation of how stubborn he would be once the struggle was over.7 Thus their ability to reach agreement without fighting depends on how consistent their expectations are of the outcome of

bargaining after the fighting is over.

Now suppose that they appear to be very evenly matched physically but that one has a black belt in karate. If this were commonly known, it would give that person a bargaining advantage. Precisely for this reason, however, if it is not commonly known then the other has good reason not to believe any claims to this effect. Thus, because the factors that are commonly known are evenly balanced, private information may lead to inconsistent estimates of the probability that each will win. But their ability to reach agreement without fighting depends on their having consistent expectations of the outcome of the bargaining that will take place once the fight is over, and this will be impossible if they have inconsistent expectations of the outcome of fighting. Thus, even if they have consistent expectations of the outcome of bargaining in the event that either won, inconsistent estimates of the probability that either will win may imply that there is no agreement prior to the use of force that they both prefer to fighting.

Even without introducing karate into the story, it is possible that because the two are evenly matched they expect that a struggle would be long and arduous, and therefore that each might believe that the other is likely to make a concession long before it is over. Thus, as long as fighting can be interspersed with negotiations, private information about each other's evaluation of the struggle itself can lead both of them to be optimistic about the outcome of fighting, even though it is common knowledge that a struggle fought to a decision would be long and costly for both sides. Therefore anyone who reasons that simply because they were evenly matched physically the prospect of a long and costly struggle would deter them from fighting must implicitly assume that once the fight began it could not be interrupted until one or the other had been defeated.

War and Negotiation

Since wars are fought by collectivities, a war's costs and benefits will fall differently on different individuals, who will therefore have different interests even if they have identical expectations—and they are very unlikely to have identical expectations. Thus wars give rise to intense internal conflicts whose pursuit can influence the course of the war. If we ignore this complication (as the literature on the balance of power does), then I shall claim there is a very close parallel between the story just told and interstate war.

Conventional interstate war is a contest in which each side tries to disarm the other in order to acquire the bargaining advantage to be derived from the unopposed use of force. In the limit, of course, force can be used to destroy one's adversary completely. Usually, however, the winner of a military contest requires the cooperation of its defeated enemy in order to accomplish its aims.8 Thus "victory" and "defeat" refer to the outcome of the clash between armed forces, not to the political objectives that motivated the conflict.9

Military contests may lead not to the defeat of one side or the other, however, but to stalemate. Fighting may last for a long time before it becomes common knowledge that a stalemate exists. But even if stalemate is commonly expected at the outset, the fact that fighting is costly means that it may be possible to derive a bargaining advantage from imposing those costs on the enemy, even though doing so requires subjecting oneself to costs as well. Thus one cannot assume (as many do) that a simple probability distribution over the two outcomes of victory and defeat is an adequate way of representing states' incentives to use force or that a belief that stalemate is the most likely outcome is sufficient to deter its use. 10

Wars, therefore, are contests in the use of force that are meant to induce one's opponent to agree to something. If the adversaries have inconsistent expectations of the outcome of bargaining after fighting, they are unlikely to be able to reach an agreement without fighting. And if they have inconsistent expectations of the outcome of fighting, they are unlikely to have consistent expectations of the outcome of the bargaining that will follow.

There are a variety of factors that are relevant to

estimating the outcome of fighting, including the relative sizes of the opposing forces, the nature of the weapons systems available, the ability of the antagonists to mobilize resources to replace those which are lost in combat, the skill of the opposing forces, the strategies to be employed, and so on. The state of some of these factors (e.g., the size of the opposing forces and the economic base that supports them) is public knowledge. However, information about other factors (e.g., the strategy to be employed) is not public, either because states try to keep them secret or because they have an incentive to mislead their adversaries when they publicize them. The greater the role of factors that decision makers have private information about, the greater the likelihood that their estimates of the outcome of war will be inconsistent. And the more nearly equal the publicly known factors are, the greater the role of factors about which the antagonists have private information. Hence it is most likely that both sides will feel optimistic when the balance of power is approximately equal and that their expectations will be consistent when their power is unequal. This reasoning therefore supports Blainey's claim that conventional war is least likely when power is distributed unequally between the potential combatants.¹¹

However, in comparing the choice between war and negotiation with a situation in which two individuals were struggling over the division of a sum of money, I implicitly assumed that agreements were enforceable (even if offers made while struggling were not). I also assumed that there was no relation between wealth and bargaining power. But agreements between states must be self-enforcing, and the correlation between wealth and power is an important feature of international conflict. This means not only that the wealthiest states are likely to be the strongest states but also that states can become stronger by defeating other states and acquiring their resources. Thus, the outcome of one contest can influence the expected outcome of subsequent contests, so that compromise agreements may not be enforceable.

To see why, suppose that two states are considering an agreement to redistribute resources as an alternative to fighting. The range of agreements they both would prefer to fighting depends on their expectations of the outcome of the fight. But if redistributing resources from one to the other worsens the expectations of the first and improves the expectations of the second, then after the redistribution the one who benefited from it may be able to enforce a demand for more. If so, the first agreement is not enforceable and therefore not really available. 12 In that case war can be avoided only if each side prefers the existing distribution to the present value of the distribution that it expects would be agreed to after one or the other was defeated or if one side is sufficiently pessimistic about the outcome of fighting that it prefers to accept defeat in advance. 13 This implies, as we have seen, that war is most likely if the balance is neither equal nor very unequal.

Moreover, states may prefer fighting to negotiating in these circumstances even if their expectations of the outcome of both fighting and negotiating are consistent. Blainey wrote: "When nations prepare to fight one another, they have contradictory expectations of the likely duration and outcome of the war. When those predictions, however, cease to be contradictory, the war is almost certain to end" (1988, 293-94). If compromise agreements are not enforceable, however, states may fight not because their expectations are inconsistent but because they are both sufficiently uncertain of the outcome of war to prefer it to defeat or the status quo. The war ends, therefore, not when the adversaries' expectations become consistent (since they may have been consistent at the outset) but when the ultimate outcome becomes nearly certain.

We have identified, then, two plausible lines of reasoning about the relation between the distribution of power and the likelihood of war. One leads to the conclusion that (other things being equal) there is an inverse linear relationship between inequality of power and the probability of war, since the more unequal the balance of forces, the more consistent adversaries' expectations of the outcome of a military contest will be and therefore (other things being equal) the more likely it is that they will be able to agree on the terms of a settlement as an alternative to war. The other leads to the conclusion that the relation between the distribution of power and the probability of war is nonlinear, since the probability of war is greatest for distributions somewhere between equality and great inequality; however, power has to be very unequal for the probability of war to be lower than it is when it is distributed equally. The crucial difference between these two arguments concerns the enforceability of agreements: the first assumes that all possible compromise agreements are enforceable, and the second assumes that compromise agreements are not enforceable and that therefore adversaries must choose between the status quo and negotiating after one side or the other has been defeated on the battlefield.

Note that both arguments are ceteris paribus arguments, and thus in practice any effect of a change in the distribution of power on the probability of war could be swamped by the influence of other factors. Note also that both assume (implausibly) that any two potential adversaries are equally likely to decide to use force to upset the status quo when faced with the same probability of military success. If instead one state prefers the status quo to any alternative regardless of the distribution of power, both lines of argument imply that the probability of war is least when that state's relative power is maximized.

Both these arguments also assume that only two states are in conflict with each other. However, wars are fought not just by individual states but by coalitions, so that the distribution of power between military antagonists depends not just on the power of individual states but also on which states decide to fight. We must therefore consider how this affects the

relation between the distribution of power and the likelihood of war.

ALLIANCES, THE DISTRIBUTION OF POWER, AND WAR

Let us continue to assume, following the previous discussion, that states have resources such as wealth, territory, population, and technology that are assets in fighting wars. Using their knowledge of the assets of potential adversaries, statesmen estimate the likelihood of the outcomes of wars between them. Because the outcomes of wars are also affected by factors that are not common knowledge, their probability estimates may not be consistent. However, given any two potential military antagonists, the more unequal they are with respect to their publicly known assets, the more consistent everyone's expectations will be. But now let us also assume that there are more than two states and that the assets of states can be combined, so that wars can be fought between coalitions as well as individual states.

We have seen that because assets such as wealth, territory, and population lead to military power, gains derived from either the actual or threatened use of force may be cumulative, so that any particular pair of adversaries must consider the effect of some proposed redistribution of these assets on the expected outcome of subsequent conflicts between them. When there are more than two states, this fact also implies that even allies must consider the effect of any redistribution on the expected outcome of subsequent conflicts in which they may be adversaries, and states that do not participate in a conflict must consider the effect of its outcome on some subsequent conflict they might have with one or another of the participants. Controversies about the implications of these facts are the central focus of the literature on the balance of power.

In examining their implications for the relation between the distribution of power and the probability of war, it will be convenient to assume that all the commonly known assets of a state that are relevant to predicting the military outcomes of wars it might engage in can be summarized in a single index represented by a real number, r (for "resources"). Using their knowledge of the distribution of these resources, together with any private information they may have, statesmen estimate the likelihood of the outcomes of wars fought between various possible combinations of states. (When states form alliances, the military resources available to the alliance are simply the sum of the resources of the member states.) Let us also assume that wars lead to one of three possible military outcomes: one or the other side's armed forces win, or there is a military stale-

Some recent game-theoretic models designed to investigate the stability of international systems have been based on such assumptions. However, these models have also made a very simple and unrealistic assumption about the relation between the distribution of power and the outcomes of wars: if $r_i > r_j$, then it is common knowledge that the probability that i will win a war with j is 1.0, and winning the war will give i control over all of j's resources; whereas if $r_i = r_j$, it is common knowledge that the probability of stalemate is 1.0, and a stalemate implies that no resources are redistributed (Niou and Ordeshook 1990; Niou, Ordeshook, and Rose 1989; Wagner 1986). With these assumptions, of course, states may or may not be eliminated (which is the question of system stability) but wars will occur only if negotiation is impossible, since there is neither uncertainty nor private information.

Nonetheless, these models are relevant to our problem, since one of the possible consequences of wars is the elimination of the defeated state or states and the absorption of their resources by the victors and since this fact will affect the incentive of states to participate in wars. Suppose, for example, that two states absorb all the resources of a third. This transfer will not be permanent if, as a result of it, one of the victors is then able to force its former ally to surrender its resources. Consider, then, a system of three states in which the resources are distributed 150, 100, 50. Then the assumptions just stated imply that if the largest state acquires any more resources it will be able subsequently to defeat both the other two. Thus neither of the other states will ally with it, and each will come to the aid of the other if it is attacked by it. Moreover, neither of the smaller states will attack the other, since doing so would allow the largest state to join the attack and gain resources. Therefore no transfers can take place in this situation, and the system is stable. 14

Suppose, on the other hand, that resources are distributed 100, 100, 100. Then a coalition of any two states can defeat the third, but will it absorb all its resources? Perhaps it will, if the resources of the defeated state are divided equally between them, leading to the distribution (150, 150). In that case, the system is not stable. However, such an agreement would not be enforceable, and nothing prevents either ally from trying to cheat and acquire more. If it does, then the other can only protect itself from this defection by switching to the side of the victim before it is eliminated. 15 And even if this fails to occur, the enemy is defeated, and the distribution (150, 150) is implemented, any exogenous change in either side's resource endowment (perhaps by economic growth) would leave the other unable to avoid defeat. Thus neither of the victorious states would want to eliminate the defeated state entirely. Either or both these possibilities would imply that the victim, while it might lose resources, would not be eliminated, which provides a justification for a "hidden hand" interpretation of the idea of the balance of power: in a system of more than two states, the balance of power will protect the independence of states even if all states seek to expand at the expense of others.¹⁶

With this conclusion in mind, consider again the

distribution (100, 100, 100). In this situation each state is exposed to the possibility that the other two will form a coalition and attack it. On the other hand, any two of them can secure themselves from attack by attacking the third and absorbing its resources until one of them has 150 units, which freezes the system and makes further transfers impossible. Thus there is a strong incentive for expansion even for states that have no interest in acquiring more resources.

This reasoning implies that three-member international systems are stable. But this conclusion implies in turn that five-member systems may not be, for if two states in a five-member system are strong enough to defeat any combination of other states, then they can eliminate two other actors and stabilize their winnings in a three-actor system. The traditional literature on the balance of power focuses on the need for states to ally against a potential hegemon, that is, a state that is on the verge of being able to defeat any combination of other states. In systems with more than three states, however, this reasoning implies that hegemonic coalitions are also a danger.

These conclusions rest on the assumption that the outcome of wars can be anticipated with certainty and that they are common knowledge. It we assume, more plausibly, that knowledge of the distribution of resources between potential adversaries leads to nonzero probability estimates for all the possible outcomes of war and that these probability estimates may not be consistent, then the incentive for coalitions to form in opposition to potentially hegemonic states or coalitions remains, but much else is altered.17 Uncertainty about outcomes implies that in every military conflict, each side faces some probability of defeat, though this probability can be diminished by increasing the resources on one's own side. In a world of two states, of course, each side can increase the quantity of resources available for military use by reallocating them from other uses; but a state's initial resource endowment (and its capacity for mobilization) implies that there is some upper limit to the quantity of resources it can devote to conflict. Alliances enable states to circumvent this limitation if other countries are willing to cooperate. Uncertainty about what other states will do, however, will lead to uncertainty about the strength of each side, and thus add to the uncertainty that already exists about the outcome of war.

Consider again the distribution (100, 100, 100), for example, and suppose the first two states combine to attack the third. With no uncertainty about the outcome of war, if their attack led to a distribution such as (150, 140, 10) then their gains would be secure, since the largest state could not defeat the other two combined and neither of the other two would allow the other to be attacked without coming to its assistance. With uncertainty, however, the second-biggest state may decide that such a distribution would lead to too great a risk of defeat by the most powerful state. Therefore the second state may want to leave the third stronger than it otherwise would, in order to reduce the threat that its own ally would subse-

quently pose. However, there is nothing to guarantee that the other states would agree either with its probability estimates or with its attitude toward risk. Suppose that war led to a distribution such as (140, 140, 20). Then, instead of being supported by the third state in an alliance against the first with a comfortable margin of superiority, the second state might find itself faced with a hostile alliance between the first and third states. In order to avoid this outcome, it might therefore agree to allow the first state to make greater gains at the expense of the enemy. This would increase the likelihood of postwar assistance from the third state in any military conflict with the former ally, but at the price of a smaller chance of winning in case such a conflict occurs.

Thus uncertainty implies that states face a dilemma in trying to secure the fruits of military victory. Maximizing security requires maximizing the power of whatever postwar coalition a state expects to belong to, as well as maximizing the incentives of the other members of this expected coalition to participate in it. But with uncertainty these objectives are inconsistent, since the incentive of states to participate in a coalition for the defense of the status quo is inversely related to its power relative to a potentially dominant state. One paradoxical implication of this fact is that while in a two-state world the fact that gains are cumulative implies that compromise agreements are not enforceable, in a world of three or more states the reverse is true: a state threatened by another state can increase the incentive of a third state to help defend it by conceding some of its resources to the threatening state and thereby weakening it-

What does all this tell us about the relation between the distribution of power and the likelihood of war? In a two-state world one could minimize the likelihood of war by maximizing the power of states that are satisfied with the status quo. However, it is not clear how to identify such states or to guarantee that they would always remain satisfied. Thus if compromise agreements are not enforceable, only equality of power will minimize the probability that either state will choose to use force against the other. In a world of three or more states, however, one must distinguish between the distribution of power among states and the distribution of power between some pair of coalitions that might fight each other. And if settlements are not enforceable, then even dissatisfied states must be concerned about how to protect what they already have. As a result, it is at least possible that threats to upset the status quo would always be met by an overwhelmingly powerful coalition, a possibility that would paradoxically require an equal distribution of power among states.

To see why, bear in mind that (other things being equal) the probability of one side's success in war varies directly with the ratio between its resources and those of its opponent and that the more unequal this ratio the less likely "other things" (i.e., assets that states have private information about) will matter. Thus states that wanted to deter an attack by all

possible antagonists would want to maximize the resources available to prevent every other state from upsetting the status quo. 19 Consider again, then, the distribution (100, 100, 100), and assume that no state wanted to diminish the power available to oppose some other state's expansion. Then, even if every state would prefer to have more resources, this distribution would have the same effect as the (150, 100, 50) distribution in the world without uncertainty earlier discussed; that is, no state would want to assist any other in expanding (since even if it expanded as well, the relative power of any coalition subsequently available to oppose the other expansionist state would be diminished), and any state that sought to expand on its own would be opposed by both the other two. Because distributions must be self-enforcing, in other words, even expansionist states would follow a collective security policy.

If this sort of symmetrical policy of deterrence is to succeed, however, any state contemplating expansion would have to be confident that all the resources that might potentially oppose it would in fact be used against it. Consider again the distribution (100, 100, 100) and suppose the first state believes that it has a strategy that will enable it quickly to defeat the second state at an acceptable cost (in spite of the equality of their military resources). If it is to be deterred from trying, it must believe that the third state will oppose it. But will it?²⁰ This depends not only on the third state's evaluation of the risks of not opposing it relative to the costs of doing so but also on what the third state believes about the second state's preferences; for it would be self-defeating for the third state to protect the second and then find itself facing a coalition between the second state and the first state—that is, in order for all states to follow a policy of collective security, it is necessary not only that they all prefer to follow a policy of maximizing deterrence but that it be common knowledge that they do. But precisely because military resources are distributed so equally, each state has reason to doubt that this is true of the others.²¹

Now that we have seen a connection between the idea of collective security and military deterrence, we can use the literature on deterrence theory to explore this problem further. The deterrence problem is to make one's threats credible when there is reason to doubt one's willingness to carry them out. This problem arises because the person or state making the threat has private information about his willingness to carry out the threat and has an incentive to misrepresent that information to the person or state being threatened. When modeled as a game with incomplete information, the standard conclusion is that to increase the credibility of its threat a state must take some action that it would be less likely to take if it were prepared to carry out its threat than if it were not (Wagner 1992). Doing this is much more difficult in the three-actor system I have described, however, than in the two-actor context in which the problem is normally discussed. For example, each state must try to show that it is prepared to join with another to defend the status quo but not to alter it by supporting another in an attack. It is not clear what actions would serve to discriminate between these two sets of preferences.²²

Moreover, if one state does attack another, this could be either because it does not share the preferences necessary to support a collective security policy or because it doubts that the others do. Should the others therefore seek to reestablish their credibility by jointly defending the status quo, or should they infer that the attacking state will not cooperate in a collective security system and therefore seek to make it less of a threat by reducing its power? And if they choose the latter alternative, how are they to protect themselves from each other after this is done?²³

Thus, while an equal distribution of power minimizes the role of private information in estimating the power of the community as a whole relative to the power of a single state that challenges the status quo, private information about the willingness of states to join in the defense of the status quo means that there may nonetheless be inconsistent expectations about whether a challenger would be successful. Consider, on the other hand, a distribution such as (140, 130, 30) and suppose that the two smaller states agree that any further increase in the power of the largest would be unacceptable and declare that they would cooperate to prevent it. If so, then the distribution itself would lend credibility to this claim, and this credibility could be reinforced by the formation of an alliance, the commitment of troops, and all the other actions that states customarily use to signal their intentions to each other. Thus this distribution lessens the role of private information in influencing states' expectations about who will defend whom against what. And the near equality of the two opposing coalitions reduces the incentive of either to use force against the other.

Thus, if wars affect the distribution of power, then (other things being equal) in a world of more than two states the distribution of power that minimizes the likelihood of war is one in which power is distributed sufficiently unequally among states that at least one state (or, if there are four or more states, a coalition of two states) is considered unacceptably close to dominance by the other states in the system, a distribution that will then lead to the formation of a stable opposing coalition which will nonetheless not be strong enough to have an incentive to use force to weaken the near-dominant state or states. The likelihood of war is minimized, in other words, when power is distributed sufficiently unequally among states that two stable but nearly equal opposing coalitions form.24

This conclusion can be used to illuminate post—World War II controversies among students of international relations about the relation between the distribution of power and the likelihood of war. Traditionally, when people have said that power was "balanced," they have meant that it was approximately equally distributed, so that any individual state that might contemplate expansion could be

opposed by another state of approximately equal power. In a world of more than two states, this also implied a multiplicity of possible coalitions of states hence the connection between "balanced power" and the shifting alliances of a "balance-of-power system." The conventional wisdom was that this distribution was the most stable one. This wisdom did not clearly distinguish, however, between stability in the sense of the preservation of the independence of the members of the system and stability in the sense of peace. This confusion was encouraged by the tendency to ignore the fact that while individual states could be opposed by other individual states with equal power, overwhelmingly powerful coalitions of states could form. This helped secure the stability of the system, since it meant that individual states had a long way to go before they were stronger than any possible countercoalition, and therefore hegemony could always be prevented. For precisely that reason, however, states could be uncertain about how far they might expand before they encountered effective opposition, and the fear that if they did not expand they might be defeated by superior coalitions gave them a strong incentive to do so. These facts help explain the two world wars of the twentieth century.25

After World War II, three competing ideas about how to secure the peace were influential in Great Britain and the United States: to restore the old system of "balanced power," to create a permanent alliance among the victors that would control the defeated countries, and to use international institutions to create a collective security system. There was instead a conflict among the members of the victorious coalition over how to secure their gains, which led to the Cold War, a development that can easily be understood in the context of the analysis offered. The leaders of the Soviet Union, fearing that their former allies would turn against them and join forces with the countries defeated in World War II, preferred a settlement that left Germany sufficiently weakerand the USSR sufficiently stronger—that their allies formed a counteralliance to deter the Soviets from any further expansion. ²⁶ The result was a distribution of power similar to the one described.

Because this conflict led to the division of Europe into East and West, it seemed to imply no possibility of a return to a system of shifting alliances among more or less equal states. For those who believed that such a system was the most stable one, this implied a pessimistic conclusion about the future. This interpretation was reinforced by the common view that it was such a polarization that had helped cause World War I. Instead of war, however, the polarization that accompanied the Cold War led to what John Gaddis has dubbed "the long peace" (Gaddis 1991). This fact supports an alternative view, based on the analysis offered here, that the polarization of the international system did not cause World War I but that its incomplete polarization helped make it possible and that it was the prolonged confrontation between East and West that helped keep the peace after World War II.²⁷

The distribution of power underlying this confron-

tation has mistakenly been identified with one in which the two largest countries can defend themselves without allies against any combination of adversaries (one of the common definitions of bipolarity).28 This has made it difficult to explain the fact that more than anything else, the Cold War between the United States and the Soviet Union has been a competition for allies" (Walt 1987, 3). However, this behavior can easily be understood in light of the argument presented here. It is precisely because it seemed so plausible that U.S. security did not depend on allies that so many resources were expended by the American government during the Cold War to demonstrate that it considered the defense of its allies to be a vital interest. This is consistent with the thesis that what is commonly known about the distribution of military capabilities is not enough to determine with certainty the vital interests of states, but a distribution of capabilities such as this one facilitates the exchange of private information about what those interests are. Thus the emphasis of the U.S. government on deterrence during the Cold War was not simply the result of the development of nuclear weapons—a fact that illustrates the close connection between my theme and deterrence.²⁹

Consider, on the other hand, the problem of deterring Iraq from its recent conquest of Kuwait. Even if it had been common knowledge that the U.S. government was committed to preventing any state in the Middle East from becoming unacceptably powerful, no one could have been certain that the United States would use force to liberate Kuwait if it were conquered by Iraq. And uncertainty about U.S. preferences gave the government of Saudi Arabia an incentive to avoid provoking the government of Iraq and thus to avoid stating clearly that Saudi Arabia would cooperate in opposing it. Moreover, because the United States had wanted the cooperation of Iraq in opposition to Iran, it had helped Iraq acquire sufficient military power that Saddam Hussein could believe that Saudi Arabia would not dare oppose it. But without the cooperation of Saudi Arabia, the liberation of Kuwait would have been impossible. Thus if, prior to the invasion of Kuwait, the United States had succeeded in convincing the government of Iraq that it had turned against it without convincing Saddam Hussein that the government of Saudi Arabia was prepared to oppose it as well, this would only have given Saddam Hussein an added incentive to absorb Kuwait. This, on a small scale, is an illustration of how shifting coalitions make deterrence more difficult.30

SUMMARY AND CONCLUSIONS

It is often said that war represents the failure of diplomacy. As Blainey pointed out, one could just as easily say that diplomacy represents the failure of war: the problem is to understand why states sometimes choose one and sometimes the other (1988, 292). And to understand the relation between the

distribution of power and the likelihood of war, we must understand how the distribution of power affects this choice. This is a much more complicated question than it is commonly made out to be, for two reasons. First, war and diplomacy are not just alternatives: the point of war is to influence the outcome of negotiations, so war can be a way of conducting diplomacy. Thus to understand the relation between war and diplomacy, one must understand the relation between the use of force and bargaining. Second, conclusions about the effect of the distribution of power on the probability of war that may seem plausible in a world of two states do not necessarily apply to historical international systems in which there have always been more than two states.

The primary function of force in bargaining is to improve one's bargaining position by increasing the costs of disagreement for one's adversary. Force can serve this function even if both sides can employ it against each other, as long as one expects to suffer proportionally less than the other (Pillar 1983, 144-95). However, force is much more effective if it can be used unilaterally, so that another function of force is to disarm one's enemy, thereby preventing him from using force against one while one uses force against him. All wars are to some extent contests in punishment, since all wars are to some degree costly and unpleasant. Interstate wars have primarily been contests in disarmament, however, in which each side seeks to acquire the ability to punish the other unilaterally by first destroying its military forces.

Counterforce contests, if fought to the finish, must end in the disarmament of one side or the other, or in stalemate. "Victory" and "defeat" in this context, then, refer to the outcome of a counterforce military contest and not to the achievement of the political objectives that may have motivated it, and "power" means whatever assets states have that affect the probability that they will win such a contest. The fruits of victory in this sense may be disappointing, and so power in this sense may be worth much or little. Wars, however, need not be fought to the finish, so that another possible outcome of war is that it may lead to a negotiated settlement before either side wins (or before it becomes certain who the eventual winner will be). Thus states may choose to fight even if the expected outcome of fighting is a military stalemate, if each is optimistic about the effect of a stalemate on the willingness of the other to compromise.

States must choose, then, between negotiating without fighting and fighting followed by negotiations. Any agreements that states reach must be self-enforcing, however, in the sense that implementing an agreement cannot be expected to enable one of the parties to overturn it and enforce a still more favorable agreement. Thus if the terms of an agreement affect the relative power of the parties to it (e.g., if what is in dispute is the distribution of territory and the military power of each is positively related to the amount of territory it controls), then compromise

agreements accepted in lieu of war may not be enforceable.

If agreements are enforceable, it is most likely that there will be an agreement that both antagonists prefer to war if their expectations concerning the likely outcome of war are consistent. This is the main reason for believing that the likelihood of war between two antagonists varies inversely with the degree of inequality in the distribution of power between them, because, for the reasons given, it seems plausible that inequality of power makes consistent expectations of the outcome of war more likely.³¹

However, much of the literature on international politics assumes not only that war is influenced by the distribution of power but also that war affects the distribution of power-and therefore that the outcome of one war will affect the expected outcome of subsequent wars. This implies that compromise agreements are not enforceable, so that if one state prefers the expected outcome of fighting to the status quo, war can be avoided only if the other prefers to surrender without fighting.³² Therefore in a twocountry world if both sides are equally likely to use force when faced with the same probability of winning, the likelihood of war is least if power is distributed equally (so that the likelihood that either will prefer war to the status quo is least) or if power is distributed very unequally (so that, for the weaker side, the probability of winning is outweighed by the expected costs of fighting).33

When there are more than two states, expectations of the outcome of wars depend on who will participate in them as well as on the distribution of power, and expectations about who will participate will be influenced by private information about how states define their interests. All states can be expected to have an incentive to defend their independence, however, so that the more a state's independence is threatened by another state or coalition of states, the more consistent should be everyone's estimates of the probability that it will join an opposing coalition. This leads to the conclusion that in a world of three or more states the likelihood of war is minimized when power is distributed sufficiently unequally that at least one state (or, if there are more than three states, a coalition of two states) is considered unacceptably close to dominance by the other states in the system, who therefore form a stable opposing coalition with nearly equal power.³⁴ This conclusion clarifies the relation between controversies about whether equal or unequal power distributions reduce the probability of war and controversies about whether bipolar or multipolar international systems are more stable.

The relation between the distribution of power and the likelihood of war depends, then, on whether the terms of possible compromise agreements that might be accepted in lieu of war influence the balance of power between the antagonists and therefore influence the probability that the agreement will be enforced—and if they do, it depends on how many states' interests will be affected by the outcome. Thus there can be no general answer to the question of

what relation one should expect between the distribution of power and the likelihood of war, and reasoning that may be appropriate for understanding conflicts among the great powers during certain historical periods may not be relevant to conflicts that have only local or regional significance or to other periods. Note that this conclusion would hold even if the only factor affecting the likelihood of war were the distribution of power, and power (i.e., all the publicly known attributes of states that affect the probability of their success in counterforce military contests) could be unambiguously measured.

In fact, of course, many other factors besides the distribution of power affect the likelihood of war, and their effect may be much greater. Moreover, the way in which decision makers translate the distribution of such commonly known assets of states as population and wealth into probability estimates of the outcomes of various possible wars may be difficult to discern and quite variable.³⁵ Thus it is not surprising that empirical tests of competing claims about the effect of the distribution of power on the probability of war have been inconclusive.

Finally, this discussion has two important implications for the use of formal models in investigating theoretical issues in the study of international politics: (1) the issues discussed here are more complicated than they are often made out to be and are therefore difficult to explore without the aid of formal models, but (2) in constructing such models one must be careful about the assumptions one makes. If the role of a formal model is to provide a counterexample to some general assertion in the literature, the realism of its assumptions is not very important.36 But if a model is to tell us something we did not already know about some historical situation, it is important that its conclusions not require that states in that situation either be able to do things they could not in fact do or unable to do things that they could in fact do. Many models assume that prior to war, any agreements states might make are enforceable but that once war begins no further negotiation is possible—and then proceed to analyze the implications of these propositions in a world of two states.37 As the current civil war in Yugoslavia graphically illustrates, these assumptions are not as innocuous as they appear.38 And even scholars who are likely to notice the significance of assuming that agreements between states are enforceable (e.g., those who identify themselves as "realists") tend to overlook the fact that the nonenforceability of agreements does not have the same significance in a world of more than two states that it has when there are only two.³⁹

Notes

An earlier version of this article was presented at the annual meeting of the American Political Science Association, 1992. I would like to thank James Fearon and Robert Powell for helpful conversations and correspondence on the subject.

- 1. For a recent discussion with citations to the literature, see Fearon 1992.
- It is sometimes suggested that this conclusion requires the assumption that all states are in fact interested in territorial expansion. However, this assumption is clearly stronger than necessary.
- 3. Both these arguments assume that it is impossible to decrease one state's likelihood of success in attacking without increasing the other's. Obviously this need not be true. In medieval times building a castle diminished the likelihood that one's enemies would be successful in attacking without increasing one's own chances of success in attacking one's enemies. Natural geographical barriers as well as great distances can have the same effect. This is one basis for the view that whether technology favors the defense over the offense is more important in influencing the likelihood of war than the balance of power. One cannot count on the appropriate technology, however, and given some prevailing technology, the relative size of military forces may still be important.
 - 4. A similar idea can be found in Timasheff 1965.
- 5. The latter method is the basis for the Rubinstein model of bargaining. For an accessible recent discussion of the relevant theoretical issues, see Kreps 1990. For an attempt to apply this model to the choice between fighting and bargaining, see Powell 1992.
- 6. One of the difficulties even with formal models of bargaining that assume incomplete information is that they predict that bargainers will reach agreement much sooner than they often do in fact (Powell 1992).
- 7. Consider Saddam Hussein's current reputation for compromise.
- 8. Because wars are contests between collectivities, there is a difference between postwar negotiations in which the defeated country retains a government and negotiates as a single political entity and a situation in which the victor occupies the defeated country and deals directly with the population. For discussions of bargaining between victor and vanquished, see Albert and Luck 1980; Keckemeti 1958; Sharp 1990. For a discussion of negotiated settlements of wars using cooperative bargaining theory, see Pillar 1983. For a discussion of negotiated settlements of civil wars, see Wagner 1993a.
- 9. One must distinguish between victory/defeat on one battlefield or in one theater and strategic victory/defeat. The Japanese defeated the U.S. forces at Pearl Harbor. Had the United States not retaliated and acquiesced in Japanese expansion in the Far East, this would have been because it chose not to, not because it was unable to, and in that case, it would have been considered a negotiated settlement between the United States and Japan influenced by a military engagement, not simply a military victory by Japan. One could say the same about a decision by the United States and England not to invade the European continent after the defeat of France by Hitler's Germany or a decision by the United States and its allies to acquiesce in the conquest of Kuwait by Iraq. This is relevant to understanding what Mearsheimer calls a "limited aims strategy" (1983, 23–66; see also Clausewitz 1976, 69, 90–99).
- 10. This is important to bear in mind in thinking about the impact of nuclear weapons on the likelihood of war. The combination of nuclear weapons technology and ballistic missiles changes the relation between the counterforce and punishment uses of organized military force and therefore the significance of the balance of power in the sense of that term employed here: because of ballistic missiles, it is difficult to disarm one's opponent completely, but adversaries are still able to do massive amounts of damage to each other (Schelling 1966, v-vii, 1-34). However, conventional wars have always been costly when neither side was able to disarm the other; and one cannot be certain that states with nuclear weapons will be dissuaded from engaging in the sort of contest in punishment that military stalemates have always made possible.
- Blainey actually seems to defend a stronger thesis—that inconsistent expectations of the outcome of war are a neces-

sary condition for fighting. Fearon (1992) analyzes a model illustrating the falseness of this claim, since even with consistent probability estimates of the outcome of war bargainers may not be able to reach an agreement they both prefer to fighting if their evaluations of the costs of fighting are not common knowledge. The same point is made more informally in Wagner 1993a. Because Fearon assumes consistent probability estimates, in his model the probability of war is completely unrelated of the balance of power.

Although Mearsheimer (1991) asserts that war is least likely when power is equal, his book, Conventional Deterrence, provides evidence that supports this version of the opposite view (idem 1983). In this book Mearsheimer presents several case studies supporting the thesis that war is most likely when decision makers believe they have a strategy that will enable them to win quickly without much cost—a blitzkrieg strategy (pp. 23–66). But he points out that "when a potential attacker enjoys an overwhelming force advantage, the importance of discovering and successfully implementing the optimum military strategy is not as great as when the forces are essentially balanced" (p. 65). This is consistent with the idea that when forces are equal, estimates of the likelihood of success are most likely to be influenced by factors whose status is private information.

12. For an example of this sort of problem, consider the worries of many Israelis about the effect of territorial compromises with the Palestinians on the ability of Israel to resist subsequent Palestinian demands. This may be what people have in mind when they say that because international conflict is a struggle for power, it is a zero-sum game. This is not a zero-sum game, however, since both adversaries would be better off if they could agree without fighting; the problem is that no agreement is an equilibrium.

13. For this to be true, any redistribution must reduce the beneficiary's probability of losing or its expected costs of war enough to compensate for the fact that the redistribution also reduces the beneficiary's net gains from winning. But it is unlikely to be common knowledge whether this is true or not, so that inconsistent beliefs about these effects may serve to inhibit compromise even if it is in principle possible. James Fearon has pointed out to me that if delays are expected between rounds of negotiation then with sufficient discounting of future benefits states might agree to settlements that led to piecemeal surrenders to territory. This provides a possible interpretation for what are called "salami tactics." I shall assume that states will not behave in this way.

14. One might object that this admittedly highly simplified depiction of the relation between the distribution of power before and after wars overlooks the fact that military resources are destroyed by war. However, as the examples of Japan and Germany illustrate, a war may destroy a country's current military capabilities without destroying its military potential—hence the significance of territorial expansion for the postwar balance of power.

15. Thus the victim has an incentive to make cheating easier by not resisting every member of the enemy coalition equally (Wagner 1986). For a historical example of this, see Kecskemeti's account of the debate among German leaders about what strategy to follow once they became certain of Germany's ultimate defeat in World War II (1958, 127–54). See also Koerner and Goemans 1990.

16. For a fuller discussion, see Wagner 1986. Niou and Ordeshook arrive at the same conclusion by assuming that the victim can protect itself from elimination by preemptively transferring enough resources to one of the members of the attacking coalition to give it 150 units and thus prevent any further transfers (Niou and Ordeshook 1990; Niou, Ordeshook, and Rose 1989). This is an extremely implausible assumption, however, not because voluntary transfers are impossible (they clearly are not) but because the assumption implicitly forbids any possible counteroffer to such a transfer. If counteroffers are possible, one must show why an ally could not prevent its partner from accepting such a transfer by threatening to ally with the victim instead. Clearly the victim would prefer such an offer to making the transfer, and the

ally who would be frozen out by such a transfer could be no worse off if he made it. Thus it appears that in any plausible noncooperative model of this situation in which counteroffers are not prohibited, all states desire more resources, and there is no uncertainty about the outcomes of wars, the preemptive transfers assumed by Niou and Ordeshook would not be equilibrium outcomes.

17. I shall continue to assume that military victory gives the winning side control over all the resources of the losing side and therefore that uncertainty only characterizes expectations of the outcome of combat. However, it is important to bear in mind that military victory only gives the victor the ability to use force unopposed and does not deprive the vanquished of all bargaining power. Thus victory does not necessarily mean that the winner can exploit all the resources of the loser or even that it will be strengthened by winning. Therefore there may be uncertainty about the effects of military victory as well as about whether to expect it. For a fuller and more rigorous analysis, see Wagner 1994.

18. Thus the unilateral transfers assumed by Niou and Ordeshook become more relevant when not all states desire to expand and there is uncertainty about the distribution of power.

19. Stephen Walt (1987) assumes that balance-of-power theory is inconsistent with the formation of defensive coalitions that are much more powerful than their adversaries and that such indexes as size, population, and gross national product are adequate measures of the power of states. Thus he concludes that the sizes of the alliances that formed against Germany and Japan and against the USSR are inconsistent with traditional balance-of-power theory. Neither assumption is warranted, however. See, e.g., the sections entitled "The Uncertainty of the Balance of Power" and "The Unreality of the Balance of Power" in Morgenthau 1967, 196–205.

20. Walt (1987) distinguishes between balancing and band-

20. Walt (1987) distinguishes between balancing and bandwagoning. If this distinction is as general as he claims, it ought to be applicable here, but in fact its implications are not clear. If the third state joins with the first, is that bandwagoning? We have seen that it is one way of trying to make itself more secure. Similarly, a concession by the second state to the first would increase the third state's incentive to defend it: Would it then be balancing or bandwagoning? These questions illustrate the fact that the distinction between balancing and bandwagoning is unclear unless one state is on the verge of dominance. This helps explain the motivation for Walt's ad hoc distinction between balancing against "power" and balancing against "threats."

21. Niou and Ordeshook (1990) discuss collective security

21. Niou and Ordeshook (1990) discuss collective security as a nonstationary equilibrium in a noncooperative model of balance-of-power system. However, their model assumes the possibility of preemptive unilateral resource transfers, no uncertainty about the outcomes of wars, perfect information (so that it is always known which state deviated from the equilibrium), and a preference for expansion on the part of all states. Without these assumptions it is doubtful that the punishment strategies necessary to sustain a nonstationary equilibrium of this sort could be supported.

equilibrium of this sort could be supported.

22. Consider, for example, how England might have shown, prior to World War I, that it would ally with Russia to prevent the expansion of Germany in case of a general war on the continent but would not support Russia if it tried to make gains at the expense of Germany and Austria.

gains at the expense of Germany and Austria.

23. Consider the question in the context of U.S.-Soviet cooperation in the war against Hitler's Germany and the U.S.-Soviet conflict over the future of Germany that followed.

24. For a fuller discussion, see Wagner 1994.

25. Kenneth Waltz was perhaps the first to point out this problem with international systems in which power was distributed more or less equally. However, he mistakenly identified it with the problem of securing contributions toward the supply of a collective good (Waltz 1979, 164–65). The real difficulty is instead the fact that until a state is close to dominance, opposing its expansion may not be a collective good. Therefore the problem diminishes not when multiactor international systems are replaced by two-actor systems

(which has never happened) but when states are powerful enough to attract organized opposition before they attempt to expand (Wagner 1993b; see also Koerner and Goemans 1990).

26. This interpretation of the Cold War implies that ideology was important not because it influenced the objectives of Soviet leaders but because it affected their expectations of the future behavior of their allies and because of the effect of the Stalinist system on the confidence of Soviet leaders in their

ability to control large amounts of territory.

27. With respect to World War I, compare the coalition that won the war with the alliances that preceded it. Moreover, while Britain's commitments (which reflected its interests) turned a continental war into a world war, uncertainty about what Britain would do helped make the war more likely (Levy 1990/91). Of course, during the Cold War not every state joined in opposition first to the Sino–Soviet alliance and then to the Soviet Union alone, and there was a great deal of debate about how large an opposing coalition was necessary. This is not surprising, given the uncertainty involved in estimating the distribution of power.

28. Many writers assume that since the United States and the USSR did not need allies there were only two "great powers", and therefore that the international system during the Cold War was equivalent to one with only two states. This has led to further confusion by mingling together controversy about the significance of the number of actors with controversy about the effect of the distribution of power among them. For a fuller discussion of the various meanings of the word bipolarity and their relation to the analysis offered here,

see Wagner 1993b.

29. In explaining the behavior of the American government during the Cold War, it is important to bear in mind that having to defend Europe from even very small military encroachments was considered almost as big a disaster as trying to defend it and failing. Thus the objective of policy was not simply to be able to defend one's interests with some acceptable probability of success, but to minimize the probability of having to do so. This was a much more ambitious objective than states had pursued prior to World War II.

30. I do not mean to suggest that the U.S. government made no mistakes in dealing with Iraq but only that criticisms of its behavior after the fact have understated the difficulty of the problem, in part because they assume that the United States alone, by acting differently, could have deterred the invasion of Kuwait. For a fuller discussion, see Stein 1992.

invasion of Kuwait. For a fuller discussion, see Stein 1992.

31. Bear in mind that "power" in this context means all the resources of states that influence the probability of their success in a counterforce military contest and that are matters of common knowledge.

32. See, however, the qualifications in n. 13.

33. However, since one side can become nearly certain of its eventual defeat before a war is fought to the finish, wars can still be ended by negotiation even though they could not be prevented by them. Moreover, when power is distributed equally, the role of private information in influencing expectations of the outcome of war is increased, so that if war occurs the two sides are likely in fact to have inconsistent expectations of its outcome. Blainey (1988) confused uncertainty with inconsistent expectations and thus falsely inferred that because wars can lead to negotiations and because prior to war both sides tend to be optimistic about its income, wars are always caused by inconsistent expectations and thus that inequality of power always reduces the likelihood of war.

34. In evaluating the significance of this conclusion, one must bear in mind the fact that it does not tell us how likely war is when its likelihood is minimized. Moreover, I have throughout assumed that the distribution of power could be changed only by wars and alliances. As a result first of the democratization and then of the industrialization of war, however, since the French Revolution the distribution of power has been subject to very large exogenous changes. When no state or pair of states is close to dominance, such changes magnify the difficulty states experience in developing consistent expectations of each other's behavior. When one or two states are close to dominance, however, an expectation of

an exogenous change that would strengthen them can provide a motivation for the other states to wage a preventive war, since even a 50% chance of defeating the near-dominant state or states before they become stronger may be preferable to a much smaller chance of defeating them later.

35. In particular, there is no reason to believe that if the power estimates of the Correlates of War Project are converted into percentages they can be used to represent the probability estimates of historical decision makers.

36. See, e.g., Fearon's (1992) use of a formal model to provide a counterexample to a claim in Blainey 1988.

37. See, e.g., the model analyzed in Bueno de Mesquita and Lalman 1992. There are other, stronger, assumptions also embedded in the Bueno de Mesquita and Lalman model, among which is the assumption that the state making a demand has a first-strike advantage if it chooses to fight but surrenders that advantage to the victim if it chooses to

negotiate.

38. The argument presented here is relevant to one of the issues in the current debate about how outsiders might help end the war in Yugoslavia-whether offering to enforce a settlement or increasing the power of the Bosnian forces would make a settlement more likely. Suppose outside intervention is expected to prevent the Serbs from defeating the Bosnians and therefore lead to a military stalemate. If both sides were confident that any settlement they reached would be enforced, then outside involvement leading to a stalemate might lengthen the war if each side were optimistic about the other's willingness to compromise on the terms of such a settlement in order to avoid continuation of the conflict. On the other hand, if (as seems more likely) neither side is confident of outside enforcement and any territorial settlement will influence the balance of power between the combatants in subsequent conflicts, then expectation of a stalemate might lead the Serbs to accept an armistice that preserves the existing distribution of territory. However, any subsequent change in the distribution of power between the two sides might lead to a reopening of the conflict. In the meantime, the war continues because the Serbs are too confident of winning and the Bosnians not sufficiently certain of losing to stop fighting. Thus, if the war cannot be ended by diminishing the expectations of the Serbs, it can only be ended by diminishing the expectations of the Bosnians.

39. On this point, see also Snidal 1991.

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STRATEGIC VOTING EQUILIBRIA UNDER THE SINGLE NONTRANSFERABLE VOTE

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Previous investigations of strategic voting equilibria in mass electorates have looked only at single-member districts. I shall investigate such equilibria in multimember districts operating under the single nontransferable vote system. What appear to be the most natural equilibria conform to the M+1 rule, according to which strategic voting in M-seat districts produces exactly M+1 vote-getting candidates in equilibrium, any others having their support totally undercut. This result provides the beginnings of a formal underpinning for Reed's recent extension of Duverger's Law to the Japanese case. The model also generates specific and empirically testable hypotheses concerning the exceptions to the M+1 rule that one ought to expect in equilibrium. I test these hypotheses with Japanese data. Finally, the model also reveals a type of strategic voting that is specific to multimember districts. I use Japanese data again to explore the empirical importance of this kind of strategic voting.

or as long as voting procedures have been used to decide important and controversial issues, there have been legislators and electors willing to vote strategically. Theoretical interest in strategic voting dates at least to Pliny the Younger (see Farquharson 1969) and probably earlier. I shall build on rather more recent and formal treatments of the strategy of voting: those framed in the decisiontheoretic and game-theoretic traditions. Most of this work has appeared in the last 25 years and focuses on the behavior of legislators (e.g., Austen-Smith 1987; Banks 1985; Farquharson 1969; McKelvey and Niemi 1978; Miller 1980; Ordeshook and Schwartz 1987; Shepsle and Weingast 1984). I shall focus on the other, less well trodden area of research into strategic voting, that dealing with the behavior of voters in mass elections.

I deal in particular with elections satisfying the following three criteria: (1) each voter casts a single vote (for a candidate, not a party), (2) there are $M \ge$ 1 seats to be filled in the district in which the election is held (the model focuses on one district at a time), and (3) plurality rule determines who wins the available seats (so the candidates with the M highest vote totals win election). Two electoral systems satisfy these criteria: the Anglo-American first-past-the-post system, in which M = 1, and the Japanese single nontransferable vote (SNTV) system, in which M > 1. The first of these systems is, of course, widely used; the second is currently employed at the national level only in Japan and Tajwan (and the Japanese are, as of this writing, in the midst of changing their electoral system). Although structurally very similar, the difference in district magnitudes between the two systems leads to substantial practical differences in systemic performance and party strategy. Nonetheless, the two systems are alike in their strategic voting equilibria, as will be shown.2

Although my primary interest here is in the theoretical nature of strategic voting under SNTV (I shall consider the Anglo-American system to be SNTV

with M = 1), some important and empirically testable implications of the theory arise along the way. These will be duly explored.

First, I shall review the previous formal literature on strategic voting (leaving the vast informal literature largely untouched). Then, I shall describe an M-seat K-candidate model of strategic voting under SNTV. Next, I shall show that in equilibrium, no votes are wasted at any margin, which entails both that identifiably weak candidates are deserted by their supporters (the feature upon which previous work has focused) and that identifiably strong candidates are deprived of their excess supporters. Finally, I shall explore the empirical usefulness of the model, using Japanese data as an illustration, and conclude.

PREVIOUS RESEARCH

The most widely recognized form of strategic voting occurs in single-vote plurality-rule elections held in single-member districts. As early as 1869, Henry Droop (an English advocate of proportional representation) recognized the basic logic: "As success depends upon obtaining a majority of the aggregate votes of all the electors, an election is usually reduced to a contest between the two most popular candidates. . . . Even if other candidates go to the poll, the electors usually find out that their votes will be thrown away, unless given in favour of one or the other parties between whom the election really lies" (quoted in Riker 1982, 756). Droop also saw an important systemic consequence of this district-bydistrict tendency toward strategic voting: the political nation was divided into two large catchall parties, rather than into many smaller and better-defined parties (see Riker 1982, 756-57). He thus anticipated what we now know as Duverger's Law-the proposition that electoral laws mandating the use of plurality rule in single-member districts tend to produce two-party systems (Duverger 1955; Riker 1982).

Formal mathematical study of strategic voting in the last 20 years has had two stages: an early decision-theoretic stage and a more recent game-theoretic stage. The decision-theoretic perspective on strategic voting (see McKelvey and Ordeshook 1972) is essentially the same as Droop's: some voter, whose favorite candidate has a poor chance of winning, notices a personal preference between the top two candidates; the voter then rationally decides to vote for the most preferred of these top two competitors, rather than for his or her overall favorite, because the latter vote has a much smaller chance of actually affecting the outcome than the former. The decision-theoretic approach adds to common sense not just greater precision about the assumptions implicit in Droop's reasoning (e.g., the probability not of victory but of ties and near ties matters directly) but also greater generality: the basic model has been extended to illuminate strategic behavior in multimember districts (Cox 1984), approval voting elections (Niemi 1984), and a variety of other electoral systems (Hoffman 1982)

Nonetheless, decision-theoretic analyses, both formal and informal, still deal essentially with a single voter in analytic isolation. The logical next step is to consider whether strategic voting by some voters makes such voting by others more or less likely. In particular, suppose a close third-place candidate in a single-member district begins to lose the support of his least committed followers (those who prefer him only slightly to one of the two frontrunners). This erosion of support will, if known (perhaps through polls), lead voters to reduce their estimates of the candidate's chances. But as the candidate's chances are seen to fall, some of his slightly more committed followers may abandon ship for one of the frontrunners. The process might, in theory, continue until the candidate was left with no support.

This line of thinking is game-theoretic. It essentially asks how much strategic voting there is in equilibrium. Should one expect that third-place candidates will always lose all of their support because of strategic decisions among their followers? Or are there general conditions under which this erosion of support is fairly limited or even negligible?

I addressed these questions earlier in the context of a model (similar to that in Ledyard 1984) in which three candidates compete for a single seat under the usual Anglo-American rules (Cox 1987). The key assumptions of the analysis were that all voters are instrumentally rational (i.e., they care whom they vote for only insofar as it affects the outcome of the election), that voters have incomplete information about each other's preferences over outcomes, and that all voters have "rational" expectations. I showed that in almost all equilibria some voters vote strategically and that the marginal impact of strategic voting was to decrease the effective number of parties (Laakso and Taagepera 1979).

Palfrey (1989), exploring essentially the same model, was able to characterize its equilibria in terms of candidate vote shares, showing that they fall into

two classes: Duvergerian equilibria (in which the level of strategic voting is such that the support of all but two of the candidates is undercut completely) and non-Duvergerian equilibria (in which two or more candidates are so nearly tied for second that the voters cannot decide which one to discount, leaving more than two significant candidates in the field). Palfrey believed that the non-Duvergerian equilibria were razor's-edge phenomena and hence that he had provided an internally consistent explanation of Duverger's Law in terms of strategic voting alone. Myerson and Weber however, have shown that this belief is incorrect (1993, 106). Although the non-Duvergerian equilibria do seem unusual because of their requirement that two or more candidates be virtually tied for second, they are not unusual in the mathematical sense of being nongeneric. The intuition behind them is roughly as follows. Suppose two leftist candidates (e.g., Charles Goodell and Richard Ottinger) and one rightest (e.g., James Buckley) are competing for a single post (one of the U.S. Senate seats for New York in 1970). The rightist is ahead, the two leftists trailing but close to one another. Under these conditions, leftist voters will have a hard time coordinating on one of the leftist candidates, and a non-Duvergerian result can (and did) ensue. This failure to coordinate, moreover, does not require nongeneric conditions on the distribution of voter types, such as that the two leftists have exactly the same number of voters ranking them first. If more voters rank leftist A first than rank leftist B first, for example, then the two can still end in a dead heat (or sufficiently close) if it is expected that B has a larger chance of being tied for first with the rightist than does A. Such beliefs appear unusual in that they require the "objectively weaker" candidate B to have a better chance of being tied for first than the "objectively stronger" candidate A, but they are not ruled out by the Bayesian equilibrium concept. At present, therefore, Duverger's Law cannot be derived exclusively from analyses of strategic voting equilibria.3

Myerson and Weber advance a model of voting equilibria applicable in a wide range of single-winner electoral systems—not just ordinary plurality rule but also approval voting, Borda's method of points, and many other systems as well. The main difference between their approach and Palfrey's is that they assume as an axiom something Palfrey derives endogenously, roughly, that candidates generally expected to place third or lower in the poll are much less likely to be tied for first than candidates generally expected to place first or second in the poll.

I shall try to extend the study of strategic voting to cover multimember districts. I adapt Palfrey's model to the multimember context, holding all other institutional features (one nontransferable vote per voter, seats allocated by plurality rule) constant. At the same time, some features of the Myerson and Weber (1993) and Hoffman (1982) approach are incorporated as well

A K-CANDIDATE M-SEAT MODEL

There are K candidates, indexed by $K = \{1, \ldots, K\}$, competing for M seats. The M candidates placing highest in the poll win the available seats. If two or more candidates are tied for Mth in the poll, then the tie is broken equiprobably. After any ties have been broken, the *outcome* of the election is a set of M elected candidates. The set of all possible outcomes, denoted Ω , accordingly has $\binom{K}{M}$ members. Candidates are nonplayers in the model; they take no actions and are simply "entities with fixed characteristics about which voters might care."

There are n voters, each possessing a (strict) preference ranking over the possible outcomes of the election. The analysis is simplified, without altering the basic point, if we assume that voters' preferences over outcomes are additively related to their preferences over candidates, that is, each voter *i* is able to assign Von Neumann-Morgenstern utilities to each candidate— u_{i1} , u_{i2} , . . . , u_{iK} —such that outcome $\alpha \in$ Ω is preferred to outcome $\beta \in \Omega$ if and only if $\Sigma_{i \in \alpha} u_{ij}$ $> \sum_{i \in B} u_{ii}$, with indifference obtaining if and only if the summed utilities are equal. Voter utilities can be rescaled in the standard fashion so that the most preferred candidate yields a utility of 1, the least preferred candidate, a utility of 0. After this rescaling, voter i's preferences (or voter i's type) can be described by the vector $u_i = (u_{i1}, \ldots, u_{iK})$, an element in the set $U = \{(u_1, \ldots, u_K): \max\{u_j\} = 1 \& \min\{u_j\} = 1 \}$ $0 \& u_i = u_k \text{ only if } j = k$.

The relative frequencies of the different possible types of voter preference are given by a cumulative distribution function F defined over U. I assume that all possible preferences are represented in the electorate, possibly with vanishingly small probability (formally, the support set of F is U). I also assume that the probability two randomly sampled voters have identical preferences is negligible (formally, F has no mass points).⁴

Each voter chooses a vote in order to maximize expected utility, something that depends not just on the voter's preferences over candidates but also on his or her *expectations* about how well each candidate is likely to do. These expectations are formalized here as a vector $\pi_i = (\pi_{i1}, \ldots, \pi_{iK})$, where π_{ij} equals i's (subjective) probability that a randomly selected voter will vote for candidate $j \in K$. Equivalently, π_{ij} is the expected proportion of the electorate who will vote for j, according to i. Given preferences (u_i) , expectations (π_i) , and knowledge of the number of voters (n), voter i faces a standard decision problem, whose details are given in Appendix A. The solution to i's problem (i.e., the set of votes that maximize expected utility, given u_i , π_i , and n) is denoted $V(u_i; \pi_i, n) \subseteq K$.

The model is completed with two further assumptions whose joint effect is to restrict the nature and consistency of voter expectations. First, I assume that the distribution of voter preferences, F, is common knowledge. Second, I assume that voters' expectations are publically generated (e.g., by polls and newspaper analysis of the candidates' chances), so

that diversity of expectation among the electorate is minimized. In the discussion that follows, I take this notion to the logical extreme and assume that every voter has the same expectations: $\pi_i = \pi$ for all i. This assumption can be replaced with a less stringent one, however, namely, that the expected *order* of finish of the candidates is the same for all voters, in the sense that they all agree on which candidates are *trailing* (expected vote shares that put them strictly below (M + 1)th place), which are *leading* (expected vote shares that put them strictly above Mth place), and which are *marginal* (neither trailing nor leading).

Given these two postulates, the maintained assumption of voter rationality implies a certain consistency between F and π in equilibrium, for, not all expectations π are "rational" in light of the voter's knowledge of the distribution F of voter preferences. Suppose (to take a four-candidate example) that some voter thought π equaled (.25, .25, .25), so that a randomly selected voter was equally likely to vote for any of the candidates. This expectation is clearly not consistent with a distribution of voter preferences in which the proportion of voters ranking candidate 1 last exceeds .75, since voting for 1 is a dominated strategy for such voters.

More generally, if π is a publically generated expectation common to all voters, and the number of voters is common knowledge, then each voter can simply compute the optimal votes $V(u; \pi, n)$ of all other voter types u. Then, knowing the distribution of voter types, the voter can calculate the proportion of the electorate who, given π and n, will vote for each candidate.

To see this, let $H_j(\pi, n) = \{u \in U: j \in V(u; \pi, n)\}$ be the set of all voter types who will vote for j, given π and n. Then the probability that a randomly sampled voter will vote for j is simply $\int_{H_j(\pi, n)} dF$. If $\int_{H_j(\pi, n)} dF$ is not equal to π_j , then the original expectations are not tenable: to continue to believe them entails believing that some other voters will not vote rationally.

This is the gist of the argument for imposing the following "rational expectations" condition on voter beliefs.

RATIONAL EXPECTATIONS CONDITION. The expectations π are rational with respect to the distribution F if, for all j, $\pi_j = \int_{H_i(\pi, n)} dF$.

The equilibrium conditions for the model are then two. First, every voter votes so as to maximize expected utility, given expectations π (and n); that is, voters of type u vote for a candidate in $V(u; \pi, n)$. Second, the expectations π satisfy the rational expectations condition. These equilibrium conditions are identical to those imposed in a symmetric Bayesian Nash equilibrium, the only differences being the interpretation of the model's elements.

VOTING EQUILIBRIA & WASTED VOTES

What are the equilibria of the model just specified? Letting $N(\pi) = |\{j \in K: \pi_j > 0\}|$ be the number of

candidates with positive expected vote shares, there are three main cases to consider: $N(\pi) < M$, $N(\pi) = M$, and $N(\pi) > M$.

Equilibria with $N(\pi) < M$. Equilibria with fewer votegetting candidates than there are seats to be had do not exist. Suppose, to the contrary, that $N(\pi) < M$ for some expectations π satisfying the rational expectations condition. In this case, perversely enough, no voter would actually vote for any candidate j such that $\pi_i > 0$ (i.e., $\pi_i > 0 \rightarrow j \in V(u; \pi, n)$ for no u). This follows because, given π , n, and the usual Nash assumption, each voter believes that all candidates j with $\pi_i > 0$ are certain to be elected. Thus there is no incentive to vote for them. On the other hand, voting for the most preferred of the candidates *j* with $\pi_i = 0$ is, under the same assumptions, certain to elect that candidate (by breaking a tie among $K-N(\pi)$ candidates, all with zero votes, in favor of the selected candidate).

Equilibria with $N(\pi) = M$. Such equilibria do not exist either. Consider a four-candidate, two-seat example to illustrate the point. If $\pi = (0, 0, .5, .5)$ is an equilibrium, then it must be that a voter preferring candidate 1 to 2, 2 to 3, and 3 to 4 would vote for 4 or for 3, given π . But voting for 4 is a dominated strategy for this voter. And given π , voting for 3 is not optimal, for it is possible that 3 will receive less than two votes. In these cases, a vote for 1 is superior to a vote for 3, since it either breaks a three-way tie for second in 1's favor (if 3 receives no votes) or puts 1 into a two-way tie with 3 for second (if 3 receives one vote), both desirable results. In all other cases (i.e., 3 has more than one vote) voting for 1 and voting for 3 yield identical outcomes. Thus, no voter of the specified type will vote for either 3 or 4, and given the assumption that there are some voters of each type, the assumption that π satisfies the rational expectations condition is contradicted.

Equilibria with $N(\pi) > M$. This is the most interesting case, and the one that takes the most work to nail down. Relabel the candidates, if necessary, so that $\pi_j \geq \pi_{j+1}$ for all $j \in K\{K\}$. Note that with this relabeling, the condition $N(\pi) > M$ implies that $\pi_{M+1} > 0$. Given a distribution F of voter types, I shall say that the expectations π are a limit of rational expectations if and only if, for every $\varepsilon > 0$, there exists an integer N and a sequence $\{\pi^n\}$ of rational expectations such that n > N implies $|\pi^n_i - \pi_i| \leq \varepsilon$; that is, π is a limit of rational expectations if and only if arbitrarily large electorates can have rational expectations that are arbitrarily close to π . The main result is presented in the following theorem and its corollary.

THEOREM 1. Suppose that either (1) $0 < \pi_j < \pi_{M+1}$ for some j > M+1 or (2) $\pi_M < \pi_1$. Then π is not a limit of rational expectations.

For the proof, see Appendix B. The basic logic of the proof is this: if $0 < \pi_j < \pi_{M+1}$, then candidate j is virtually sure to lose for sufficiently large n, and

voting for the most palatable of the candidates most likely to be tied for Mth yields a higher expected utility than voting for j; if $\pi_M < \pi_1$, then candidate 1 is almost certain to win for sufficiently large n, and, again, the instrumental voter's attention turns to those candidates with "asymptotically large" conditional tie-probabilities.

A direct corollary of Theorem 1 is the following.

COROLLARY 1. If π is a limit of rational expectations, then (1) $\pi_1 = \pi_2 = \ldots = \pi_M$ and (2) $\pi_j \in \{0, \pi_{M+1}\}$ for all j > M+1.

For M=1, this corollary simply extends Palfrey's earlier three-candidate result to cover an arbitrary number K of candidates. For M>1, the corollary carries two messages about wasted votes, rather than just one. Like Palfrey's previous work, it shows that strategic voting works against trailing candidates (those who fall "too far" behind in the polls). In addition, the corollary shows that strategic voting relieves leading candidates (those who are "too far" ahead in the polls) of their excess votes, reducing them to equality with all other likely winners. Thus votes are wasted neither on weak nor on strong candidates.

The nature of the two processes (winnowing out the weak, equalizing the strong) is slightly different. Voters who find themselves supporting a trailing candidate are more likely to desert that candidate as other voters desert him. There is thus a certain momentum to strategic voting as it affects weak candidates. Voters who find themselves supporting a leading candidate, in contrast, are less likely to abandon that candidate upon hearing of other defections. To illustrate this, consider a contest between three candidates (A, B, and C) in a two-seat district. Suppose A is ranked first by 40% of the electorate and is expected to get all of their votes, while B and C are each expected to get 30% of the vote. In this case, A voters who prefer C to B are tempted to switch their support to C, while A voters who prefer B to C are tempted to switch their support to B. If those who prefer C (B) succeed in bringing A and C (B) into a dead heat at 35% (by abandoning A), then similar action by those who prefer B (C) will be forestalled. If both sides (the A supporters who prefer B to C and those with opposite preferences) abandon A, then the worst outcome for both results. Thus, there is a Chicken aspect to the game: as soon as the existence of excess votes becomes common knowledge, both sides begin "driving" toward abandonment. The question is, Which side will swerve first? The present model, it should be noted, does not capture this Chicken aspect at all well formally, since the equilibrium concept simply assumes away all coordination difficulties. This may be one important reason that the model's predictions regarding the elimination of votes wasted on strong candidates fare so poorly in the empirical analysis.

As in the single-seat case, the corollary divides multiseat equilibria into two classes: (1) Duvergerian equilibria, with M + 1 vote-getting candidates, and

(2) non-Duvergerian equilibria, with more than M+1 vote-getting candidates. The Duvergerian equilibria entail a close M-way race for the M available seats, with a single runner-up, all other candidates being reduced to near-zero support. The non-Duvergerian equilibria look the same, except that there are two or more runners-up, whose nearly identical expected vote totals prevent any being winnowed out from the field of viable candidates.

STRATEGIC VOTING AS AN EXPLANATION OF REAL-WORLD DATA

I shall consider the empirical usefulness of the results just sketched. There is no question that instrumentally rational agents of the type stipulated, with rational expectations, will behave in a very precise fashion. But of course it is possible to doubt that real people are entirely instrumentally motivated or that they have rational expectations. And it is a matter of simple observation that the top M candidates in SNTV elections do not have virtually identical vote shares. Taken as is, then, the model's predictions meet with immediate empirical disconfirmation. A reasonable conclusion is that one or more of the model's assumptions are poorly approximated in the real world.

Overly precise predictions are typical of highly abstract models and a typical (often unstated) assumption of theoreticians is that the model's predictions could fairly easily be made more reasonable, without changing their qualitative nature, by adding a bit of "noise" or "friction" to the model. Since the empirical testing of rational choice models has been strongly criticized of late (Green and Shapiro 1993), I will explore what some of the noise to be added might be. Even if adding noise (e.g., noninstrumental voters or voters whose expectations are inconsistent) can in principle produce predictions not obviously false, there is still interest in two questions: (1) Do real-world data conform sufficiently closely to the model's predicted equilibria that one might believe that a model essentially similar to this one (just adding noise) might tally with real-world patterns? (2) Even if the real world conforms to sylized versions of the model's equilibria, are there other explanations that predict the same patterns? I shall examine each of these topics-noise, empirical patterns, and alternative explanations—in turn.

Noise

There are many frictions that would, if introduced into the model, soften its predictions. Here, I shall discuss only two: the introduction of noninstrumental and of "ignorant" voters. Introducing voters who are not purely instrumental but also derive utility from the particular action that they take (abstaining, voting for some candidate) can have various implica-

tions. If one assumes that voters bear some cost from voting but that conditional on voting, they do not care whom they vote for except insofar as it affects the outcome, then not much changes. One would have to assume that a fixed proportion of the electorate had nonpositive costs of voting (or something to this effect) in order to generate growing turnout, but after that, the model's results would go through unchanged.

On the other hand, if one assumes that voters care about whom they vote for as well as who wins, then much may change. For such mixed-motivation voters, action-contingent utilities would dwarf outcome-contingent utilities as the electorate grows because outcome-contingent utilities are discounted by the probability of a single vote affecting the outcome, whereas action-contingent utilities are not. Thus, a mixedmotivation voter who votes at all votes for the candidates that he or she likes to vote for, regardless of whether this is the candidate that "strategic voting" would dictate supporting. If the entire electorate had mixed motivations, then expectations about how well the candidates were doing would be of negligible importance to voter decisions, and any pattern of expectations could be consistent with voter rationality in equilibrium.

If only some voters have mixed motivations, then the result is more interesting. Let *Q* be the proportion of the electorate that is purely instrumental or outcome-oriented and let 1 - Q be the proportion with mixed motivations. In the limit, the latter group will be purely noninstrumental or action-oriented, as noted. Let F be the distribution of preferences in the instrumentally motivated electorate and let P_i be the proportion of the noninstrumentally motivated electorate that prefers voting for candidate j. Then the rational expectations condition becomes $\pi_j = Q \int_{H_j(\pi_j, n)} dF + (1 - Q)P_j$ for all j. If candidate j is trailing, then j loses all instrumental support but retains support among the mixed electorate. Thus, if Q is close to 1, the result does not change much: in equilibrium, strategic voting will reduce weak candidates-if not to zero support, then down to some fairly small minimum (i.e., $(1 - Q)P_i$) of noninstrumental support and will reduce strong candidates down to equality with all other likely winners.

Introducing "ignorant" voters relaxes a different premise of the model, namely, that the identity of trailing, marginal, and leading candidates is common knowledge. The publicness of this knowledge keeps all instrumental voters on the same page of the playbook: they *all* desert the (publicly identified) trailing and leading candidates in order to focus on the (publicly identified) marginal candidates.

One might argue for the reasonableness of the common-knowledge assumption as regards trailing candidates by noting the self-fulfilling character of voter expectations. If every voter believes that candidate j is out of the running, then j will in fact be out of the running. Moreover, if some voters, who previously intended to vote for j, come to believe that j is

behind, they will desert j, thereby making it more likely that j is behind.

The arguments just given do not really justify assuming that the identity of trailing candidates is common knowledge, however. They only justify a belief that in equilibrium, the identity of trailing candidates will probably be common knowledge. To simply assume the common-knowledge condition is similar to assuming that the players in a two-person Battle of the Sexes will coordinate on one of the two pure-strategy Nash equilibria. As regards leading candidates, voter expectations are not even self-fulfilling. If each voter believes that candidate *j* is leading (hence virtually sure to win), then each will abandon *j*, who will in fact finish out of the running.

If who trails and who leads is not common knowledge, then an extra degree of freedom is opened up in the model. In the extreme, the analyst can stipulate (possibly inconsistent) expectations for each voter. This degree of analytical latitude would be enough to make any pattern of aggregate vote returns consistent with some equilibrium of the model. On the other hand, placing limits on the extent to which voters' expectations differed would begin to restore some "bite" to the model's predictions.

These observations motivate asking how voters learn about the candidates' expected vote shares. In the real world, the forces generating common knowledge of candidate chances are polls, news analyses, candidate statements, and other bits of essentially free information. It has to be free information because rationally ignorant voters will not exert any effort to determine who is ahead for the same reason that they will not research candidate positions carefully (Downs 1957). Thus, the extent to which the real world approximates the model's strictures should depend on the availability and clarity of free information regarding the relative standing of the candidates. If voters are exposed to lots of free information (e.g., frequently published polls) that reveals some candidates to be clearly trailing the others and if this information seeps out to a large proportion of the instrumental electorate, then one expects that trailing candidates will be left with not much more than their noninstrumental support. If voters have no information regarding candidate chances (and diffuse priors), then sincere voting is consistent with expected utility maximization, and one does not expect objectively trailing candidates (those who have fewer voters ranking them first) to lose their instrumental support. If (to take a third example) voters have conflicting information regarding candidate chances, then strategic voting by some voters may "cancel out" strategic voting by others, leaving little or no observable impact on the aggregate distribution of votes.

From this perspective, the tendency of candidates trailing in multicandidate races to dispute the accuracy of the polls that show them trailing, to claim to have different results in proprietary polling, and to urge voters to ignore the polls is understandable. All these actions make good sense from the point of view

of preventing their last-place status from becoming common knowledge.8

Empirical Patterns

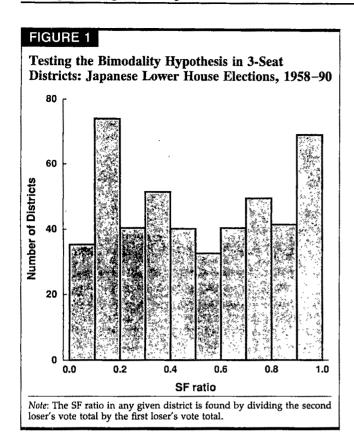
The model predicts that strategic voting will produce two main patterns: (1) trailing candidates will be deserted by all their supporters, and (2) leading candidates will be deserted by their excess supporters. In examining how these two predictions fare, I shall look at postwar Japanese electoral data from the period 1958–90. Evidence is fairly good that Japanese voters do strategically desert weak candidates. Steven Reed (1991), in an extensive examination of the postwar Japanese experience with SNTV, finds a clear and increasing tendency for there to be only M+1 viable candidates in each district. This pattern, of course, fits the Duvergerian equilibria identified by the model.

What of the non-Duvergerian equilibria? These equilibria all entail that the first and second losers receive nearly the same number of votes. Thus a theoretically interesting statistic is the *second*-to-*first* loser's vote total ratio (SF ratio). Under Duvergerian equilibria, the SF ratio will be near 0. Under non-Duvergerian equilibria, the SF ratio will be near 1. Thus, if one were to compute the ratio for a number of districts and plot the resulting distribution, one should find a spike at 0 and a spike at 1.

Allowing for some frictions in the model (e.g., some noninstrumental voters, some disagreement about which candidates are trailing or leading, and which are marginal), the prediction is softened. The SF ratio should either be close to 1 (when second losers are so close in the polls to first losers that they do not lose their support due to strategic voting) or close to 0 (when second losers are sufficiently far behind first losers that strategic voting kicks in and they are reduced to their noninstrumental support level, which I assume to be close to 0 for most candidates). The SF distribution, in other words, should be bimodal.

I have tested this bimodality hypothesis empirically in the case of Japan, using district-level electoral returns over the period 1958–90. The procedure, in the case of three-seat districts, was as follows. First, I computed the ratio of the vote total of the second loser (fifth-place candidate) to the vote total of the first loser (fourth-place candidate) for all districts with at least five candidates. Then, I produced a histogram to summarize the distribution of the resulting SF ratios (Figure 1). Results for four- and five-seat districts (the other frequently occurring types of district in Japan) are given in Figures 2 and 3.

As can be seen, the SF distribution is bimodal in each case. Values near .5 are rare, relative to those near 1 or 0; that is, it is much more common to have either a close or a distant second loser than an "in-between" second loser. Moreover, the closer the first loser to the last winner, the more likely a few more votes might change the outcome, the further



from .5 is the SF ratio (i.e., the stronger is the tendency for the ratio to be either near 1 or near 0).

Are the distributions displayed in Figures 1-3 significantly bimodal? One can reject the null hypothesis that the distribution is unimodal in the first two cases, using dip or depth tests (see Hartigan and Hartigan 1985). In the third case, the probability of observing the degree of bimodality visible in Figure 3 is a bit below .2, under the null hypothesis that the distribution is really unimodal. Thus there is some reason to doubt that the distribution is unimodal, but the evidence is not conclusive at conventional levels of statistical significance. Using a kernel densitybased test proposed by Silverman (1981), one can pit the null hypothesis of bimodality against the compound alternative of more than two modes. Doing so, one finds p-values of .22, .26, and .98 for the first, second, and third figures respectively. Thus one cannot reject the null of bimodality (in favor of multimodality) at conventional levels of significance. All told, the evidence is as it appears to be to the naked eye: the first two figures really are bimodal, and the third is harder to call but has some tendency toward bimodality.

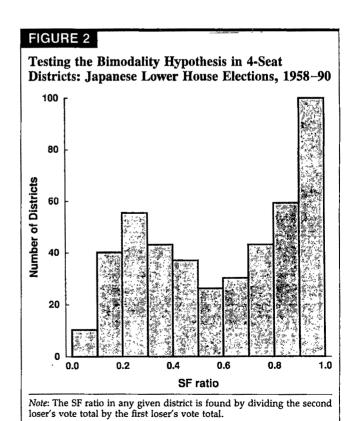
Substantively, does all this mean that one can reject the null hypothesis of sincere voting? It all depends on what one believes the distribution of voters' true preferences over candidates is. If one believes that second losers' true support tends to be either almost equal to, or much less than, first losers' true support, then the evidence presented here is consistent with sincere voting. I can see no reason for such an expectation regarding the distribution of preferences,

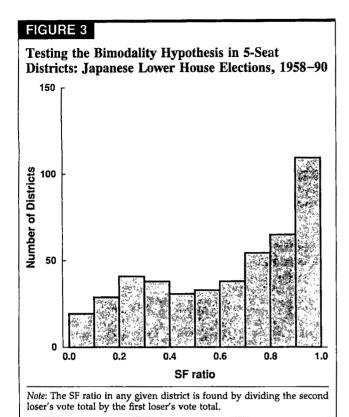
however, and accordingly take the statistical tendency toward bimodality to be evidence of strategic voting.

A second-order pattern that appears in the data is that the height of the mode near 0 declines with district magnitude. My interpretation of this is that the quality of voter information regarding candidate chances declines with district magnitude. In particular, it is harder to be sure who is trailing in a more crowded field in which very small vote percentages can win a seat. Whether this notion will hold up to scrutiny is a topic for further investigation.

Evidence that Japanese voters strategically desert leading candidates (those with more support than they need) is much less compelling than that they desert weak candidates. There is rarely anything like a dead heat among the top M candidates in a Japanese district, nor is there any movement toward such an ideal over time. Nonetheless, there is some evidence consistent with the model: (1) there is an over-time trend within the dominant Japanese party, the Liberal Democrats, toward fewer seats being lost on account of votes "wasted" on strong candidates (Cox and Niou 1994); (2) there is a statistically discernible tendency for fewer votes to be "wasted" on leading candidates when the margin of victory of the last winning candidate is narrower (indicating that votes switched from leading to marginal candidates are more likely to affect the outcome).

This last point is documented via an ordinary least squares regression, the results of which are reported in Table 1. The dependent variable, EXCESS, measures how far a particular district's race departs from the





theoretical prediction that the vote percentages of the top M candidates will be the same. It is computed by calculating, for each of the top M-1 finishers, the excess vote percentage they garner over that obtained by the Mth-place finisher and then adding the resulting M - 1 figures. For example, in a three-seat district in which the top three candidates get 30%, 25%, and 20%, excess = (30 - 20) + (25 - 30) = 15. The main independent variable in the analysis is MARGIN, equal to the vote percentage of the last winning candidate less that of the first losing candidate. The theoretical expectation is that smaller margins of victory will lead to fewer excess votes, since smaller margins indicate a better chance that votes currently wasted on leading candidates might affect the race between the marginal candidates, if transferred there. 10 Because five-seat districts have four candidates above the Mth place while four-seat districts have three and three-seat districts only two, one expects more excess votes in larger-magnitude districts. It thus makes sense to control for district magnitude, which is done via two dummy variables.

Examining the results diplayed in Table 1, one finds a positive and statistically significant effect: a 1% reduction in the margin of victory of the last winner produces, on average, nearly a .5% reduction in votes wasted on leading candidates. There is thus a mild tendency for closeness in the race for the last seat in a district to reduce the number of votes wasted on strong candidates, even if there are still quite a few such wasted votes cast.

Alternative Explanations

Although the model of strategic voting generates empirically testable predictions, some of which are new (in the sense that they have not been noticed in the previous literature), there are also some obvious alternative explanations that might explain the pattern of evidence just uncovered. The problem is that any class of agents who care about the outcome of the election (not just voters but also activists, contributors, and candidates) will tend to allocate whatever resources they control (labor, money, etc.) to marginal candidates, where they are more likely to affect the outcome, rather than to trailing or leading candidates, where they are less likely to affect the outcome. Moreover, allocation or reallocation of resources to marginal candidates should produce the clearest aggregate results (trailing candidates deprived of all instrumental support, leading candidates of excess instrumental support) when who is marginal and who is nonmarginal is widely agreed and the margin of victory is small. Thus, the empirical evidence adduced is far from proving that a significant proportion of the electorate is instrumentally motivated and in reasonable agreement on the relative standing of the candidates. It may be that contributors give only (or mostly) to marginal candidates, that trailing candidates try to sell their endorsement to marginals, or that leading candidates sell some of their excess support to marginals.

The elite-level hypotheses have some distinct advantages. First, it is more plausible that elite actors, having larger stakes in the outcome, will pay attention to how close the race is and respond by diverting resources to marginal candidates. Put negatively, it is unlikely that ordinary voters will pay any attention at all, since their single votes have an infinitesimal chance of affecting the outcome. If it is at all costly to

TABLE 1
Closeness Depresses Excess Votes: Japanese Lower House Elections, 1958–90

INDEPENDENT VARIABLE	DEPENDENT VARIABLE: EXCESS			
	COEFFICIENT ESTIMATE	STANDARD ERROR		
Constant Margin	7.27 .44	.35 .04		
Dummy for 4-seat districts Dummy for 5-seat	2.77	.42		
districts	5.41	.43		

Note: Adjusted $R^2 = .12$; N = 1,483. The variables are defined as follows. The unit of observation is an election in a particular district. Let V_j be the vote percentage garnered by the candidate placing jth in the poll in that district and let M be the district magnitude. Then (1) Excess = $\sum_{j=1}^{M-1} (V_j - V_{M})$, (2) Margin = $V_M - V_{M+1}$, (3) Dummy for 4-seat districts = 1 if M = 4 (0 otherwise), and (4) Dummy for 5-seat districts = 1 if M = 5 (0 otherwise). As an example of the computation of Excess, consider a three-seat district in which the top three candidates receive 30%, 25%, and 20% of the vote. In this district, Excess = (30 - 20) + (25 - 20) = 15. The equation is estimated by ordinary least squares.

find out who is marginal or to calculate expected utilities, rational voters should avoid these costs, since bearing them has virtually no impact on the outcome (Meehl 1977; Riker 1982).

Second, the elite models look as if they can better accommodate the rather substantial amount of votes wasted on leading candidates. Contributors who seek "access" or specific postelection favors will stick with leading but not with trailing candidates. Leading candidates have something to lose if they sell off "too much" excess support, while trailing candidates, for all intents and purposes, do not.

Nonetheless, despite the apparent advantages of elite-based models, it is not clear that one can reject the voter-based model. The informational and cognitive costs of strategic voting are modest and may be borne entirely as byproducts of everyday activities, such as reading the newspaper, watching television, or attending college courses in politics. Information on the relative standings of candidates is sometimes published in polls; it does not take a rocket scientist to understand traditional wasted-vote arguments, and these arguments are sometimes hard to avoid (being urged by concerned elites). All this suggests that voters in the real world may strategically desert weak candidates for essentially the reasons stylized in the model. It is true that the whole process is mediated by elites: they point out that the race is close and that votes on weak candidates are wasted. But the voters do the rest: they buy the argument and act accordingly. Empirically, I think that there is overwhelming evidence that voters have, in a variety of historical and electoral contexts, voted strategically in this sense (see Cox 1991). The question of the relative importance of strategic reallocation of votes in the mass electorate as opposed to strategic reallocation of other resources in the elite strata remains open, however. And the evidence for strategic desertion of leading candidates is another story entirely. Elites in Japan do not seem to point out that votes for a sure winner are wasted, and there is no direct evidence at present that voters act as if they knew.

CONCLUSION

I have investigated strategic voting equilibria in multimember districts operating under SNTV, building on the previous work of Cox (1987), Palfrey (1989), and Myerson and Weber (1993). "Strategic voting" in single-member districts refers to a voter deserting a more preferred candidate with a poor chance of winning for a less preferred candidate with a better chance at winning. In multimember districts, voters who care only about the outcome of the election will strategically desert both candidates who are "too weak" and candidates who are "too strong." Such outcome-oriented voters desert weak candidates in multimember districts for the same reason as in single-member districts. They desert strong candidates when those candidates have one of the M seats sewn up but there are other seats still up for grabs; for then the voter's vote has a much greater chance of affecting the outcome if cast for one of the "marginal" candidates—those on the edge between winning and losing. All told, instrumentally motivated voters under SNTV waste their votes neither on weak (submarginal) nor on strong (supermarginal) candidates.

ginal) nor on strong (supermarginal) candidates.

Equilibria of a "frictionless" model of strategic voting under SNTV—in which all voters are instrumentally motivated and all have rational expectations—are such that all wasted votes are wrung out of the system. As far as wasting votes on supermarginal candidates goes, this means that all winning candidates must be in a virtual dead heat in equilibrium, so that none will be perceived as having any "excess" votes that might profitably be transferred elsewhere. This prediction fares poorly in the Japanese data, although there is a tendency for fewer votes to be wasted on supermarginal candidates as the gap between last winner and first loser narrows.

Wringing out all votes wasted on submarginal candidates produces either Duvergerian outcomes (in which strategic voting erodes the support of all but one serious challenger, so that there are M+1 viable candidates all told) or non-Duvergerian outcomes (in which two or more serious challengers are so closely matched that none becomes the unique victim of strategic desertion and more than M + 1 viable candidates survive). Empirically, outcomes with M +1 viable candidates occur rather frequently. Thus, Duverger (1955) saw a tendency toward two-partyism in electoral systems employing single-member districts, while Reed (1991) saw a tendency for there to be four viable candidates in three-member districts, five viable candidates in four-member districts, and six viable candidates in five-member districts.

Both Duverger and Reed, of course, appealed to strategic voting (among other things) in explaining the observed tendency toward M + 1 viable candidacies (the M + 1 rule). The present model's utility is in illuminating some of the logical prerequisites and consequences of explaining the M + 1 rule in terms of strategic voting. As to prerequisites, the model shows that the degree to which strategic voting will winnow out weak candidates depends on how many instrumentally motivated voters there are and on how consistent their expectations about the relative standings of the candidates are. The empirical approximation of both these conditions plausibly depends on elite action and propaganda. American third-party movements (Ross Perot included) frequently emphasize future election outcomes: "We may have no real chance this time," they say, "but vote for us anyway, send a message, and help restructure American politics." The established party most hurt by the third party's appeals, in turn, is apt to emphasize the electoral here and now—the instrumental motivations highlighted in the present model. Similarly, elite actions determine how consistent voter beliefs are regarding who is winning and who is losing. If clear information about candidate chances is provided to voters, one can expect substantial strategic voting and a consequent reduction in the number of viable candidacies. If little (or conflicting) information is provided to voters, then greater amounts of sincere voting (or cross-cutting strategic voting) can be expected, and the tendency toward M+1 viable candidates will be weaker.

As to the logical consequences of explaining the M+1 rule in terms of strategic voting, the model provides specific and empirically testable predictions about what kind of exceptions to the M+1 rule one should expect. Neither Duverger nor Reed do this. Both readily admit the possibility of exceptions to their generalizations: Duverger's Law and Reed's extension of it are both stated as "tendencies." But neither says much about the nature of the exceptions. Here, the theoretically allowable exceptions to the M+1 rule have been characterized as various kinds of near ties, and a general implication of these exceptions (embodied in the bimodality hypothesis) has been tested with Japanese data.

There are of course other possible avenues to explore in explaining the M + 1 rule. Both Duverger and Reed appropriately suggest that elites may get into the act. Meehl (1977) and Riker (1982) argue that voters have too small a stake in elections to motivate strategic voting and emphasize elite actors even more strongly. Here I have noted that strategic reallocation of resources by outcome-oriented elite actors (activists, contributors, candidates) should produce many of the same aggregate patterns as identified in the voters-only model. My personal bias is strongly toward the elite-level hypotheses, as it is in the study of turnout (Cox and Munger 1989). I think strategic voting survives, both in theory and in practice, because one of the things outcome-oriented elites can do in close races to reallocate resources from nonmarginal to marginal candidates is to flood the mass media with wasted-vote arguments (including therein both the relevant evidence on candidate standings and the basic logic motivating a strategic vote). Finally, it should be noted that the basic model constructed here can be adapted to other electoral systems operating in multimember districts—in particular, to various forms of proportional representation. I intend to demonstrate this more fully in future work.

APPENDIX A: THE VOTER'S DECISION PROBLEM

I consider how a voter motivated solely by a desire to affect the outcome of the election decides whom to vote for, given that the voter votes. There are three parameters in the voter's decision (subscript is are suppressed): (1) the voter's preferences over the candidates, given by $u = (u_1, \ldots, u_K) \in U$; (2) the voter's expectations about how well each candidate will do at the polls, given by $\pi = (\pi_1, \ldots, \pi_K)$; and (3) the number of voters, n. I shall denote by $V(u; \pi, n) \subseteq K$ the optimal vote(s) of a voter of type u facing an electorate described by π and n. I shall show that the parameters identified are indeed sufficient to

yield a well-defined decision problem and reveal such of the technical details of solving this problem as are necessary for proving the theorem to come.

Letting V_j denote the number of votes received by candidate j from voters other than the focal voter, the focal voter views (V_1, \ldots, V_K) as K-nomial random variables with parameters π and n-1. Thus, each voter's beliefs about the probabilities of various events (such as ties or near ties among the candidates) can be calculated using standard multinomial formulas. The details of this are given in Appendix B. Here, I shall simply assume that the probabilities of all events mentioned are known by the voter.

In particular, the focal voter knows, for all j and $k:W_j$, the probability that candidate j wins a seat outright (i.e., $|\{h \in K: V_h \geq V_j \text{ and } h \neq j\}| \leq M-1$), and T_{jk} , the probability that candidates j and k are in a two-way tie for Mth place (i.e., $|\{h \in K: V_h > V_j\}| = M-1$ and $|\{h \in K: V_h = V_j\}| = 2$). Following Hoffman (1982) and Myerson and Weber (1993), I assume that for large n, the perceived probability of an r-way tie for Mth, r > 2, is infinitesimal in comparison to the probability of a two-way tie for Mth. With this assumption, the focal voter's expected utility from abstaining, given u, π , and n, can be approximated with infinitesimal error by

$$\sum_{j=1}^K W_j u_j + \sum_{j=1}^K \sum_{k>j} T_{jk} \left(\frac{u_j + u_k}{2} \right),$$

where various terms involving r-way ties for Mth, r > 2, that are asymptotically negligible in comparison to the largest T_{ik} , have been omitted.

By voting for candidate j, the focal voter can affect his or her utility in two ways (continuing to ignore r-way ties, r > 2)—by putting j into a tie with k (yielding a utility increment of $(u_j + u_k)/2 - u_k$) or by breaking a tie between j and k (yielding a utility increment of $u_j - (u_j + u_k)/2$). Assuming (following Hoffman 1982 and Myerson and Weber 1993) that the probability of the event "k is in k-mth place, tied with k-given k

$$\xi_j = \sum_{k=1}^K T_{jk}(u_j - u_k).$$

Thus,

$$V(u; \pi, n) = \underset{j \in K}{\operatorname{arg max}} \, \xi_j.$$

APPENDIX B: MULTINOMIAL PROBABILITIES

I shall use the following notation throughout:

R = the set of real numbers;

Q = the set of rational numbers;

 \overline{Z} = the set of nonnegative integers;

 ϕ = the standard normal density function;

 $K = \{1, \ldots, K\}$, the set of candidates; V_1, \ldots, V_K are K-nomially distributed random variables with parameters $\pi_1 \geq \pi_2 \geq \ldots \geq \pi_K$ and m; T_{jk} = probability that candidates j and k are tied for

Mth place, 0 < M < K; and $\Delta^{K-1} = \{(p_1, \dots, p_K) \in \mathbb{R}^K : 0 \le p_h \le 1 \text{ for all } h \& \Sigma p_h = 1\}$, the simplex in K-space.

I shall explore the asymptotic properties of the tie-probabilities T_{jk} . These probabilities can be expressed as follows. For any set $S \subseteq K$, |S| = M - 1, let up_j(S) denote the event $\bigcap_{i \in S} V_i > V_j$, down_j(S) denote the event $\bigcap_{i \in S} V_i < V_j$, and $E_{jk}(S) = \text{up}_j(S) \cap \text{down}_j(K \setminus S \setminus j, k) \cap V_j = V_k$. In words, $E_{jk}(S)$ is the event "the M-1 candidates in S finish ahead of j and k, j and k are tied for Mth, and all other candidates finish below j and k." With these definitions,

$$T_{jk} = \sum_{b=1}^{\binom{K-2}{M-1}} \Pr[E_{jk}(S_{jk}^b)],$$

where the sets S_{ik}^b all have M-1 elements and correspond to the different ways of selecting which M-1 candidates—from the K-2 that remain after removing j and k—will have vote totals exceeding j's. The only one of these sets that it is necessary explicitly to define is

$$S^1_{jk} = \begin{cases} \{1, \, \ldots \, , \, M-1\} & \text{if} \quad j \geq M \\ \{1, \, \ldots \, , \, M\} \backslash \{j\} & \text{if} \quad j < M < k \\ \{1, \, \ldots \, , \, M+1\} \backslash \{j, \, k\} & \text{if} \quad k \leq M. \end{cases}$$

than j and k. The next lemma is useful in showing that $\Pr[E_{ik}(S_{ik}^1)] \ge \Pr[E_{ik}(S_{ik}^b)]$ for all b.

LEMMA 1. Suppose K > M + 1 and consider any $A \subset K$ $\{j, k\}$ such that |A| = M - 1. Denote the complement of A in $K\setminus\{j,k\}$ by A^c . Suppose there exists $d\in A$, $u\in$ A^{c} , such that $\pi_{u} > \pi_{d}$, and define $B = A \setminus \{d\} \cup \{u\}$. Then $Pr[E_{ik}(A)] < Pr[E_{ik}(B)]$.

Proof. Note that

$$\Pr[E_{jk}(A)] = \sum_{v \in \bar{E}_{jk}(A)} P(v),$$

where

$$P(v) = m! \prod_{j=1}^{K} \frac{\pi_j^{v_j}}{v_j!}$$

and

$$\tilde{E}_{jk}(A) = \left\{ v \in Z^K: \bigcap_{h \in K} V_h = v_h \in E_{jk}(A) \right\}.$$

Consider any term $v \in \tilde{E}_{jk}(A)$. The vector v', defined by $v_h' = v_h$ for $h \notin \{d, u\}$, $v_d' = v_u$, and $v_u' = v_d$, is an element of $\bar{E}_{jk}(B)$. Moreover, $P(v)/P(v') = (\pi_d/\pi_u)^{V_d-V_u}$ ≤ 1 (recall that $\pi_d < \pi_u$ and $v_d > v_u$ since $v \in \tilde{E}_{ik}(A)$).

COROLLARY 1. $Pr[E_{ik}(S_{ik}^1)] \ge Pr[E_{ik}(S_{ik}^b)]$ for all b.

I omit the proof: it follows directly from Lemma 1. In what follows, I shall abbreviate $E_{ik}(S_{ik}^1)$ by E_{ik} .

LEMMA 2. Let a, b, and c be elements of K such that either (1) $a < M \text{ and } a < b \le M + 1 \text{ and } (b = M + 1 \rightarrow c)$ \leq M) and $\pi_a > \pi_b$ or (2) a > M + 1 and $M \leq b < a$ and $(b = M \rightarrow c \ge M)$ and $\pi_a < \pi_b$. Then $\lim_{m \to \infty}$

Proof. By Corollary 1, it suffices to show that $\lim_{m\to\infty}$ $Pr[E_{ca}]/Pr[E_{cb}] = 0$. The proof proceeds separately for the two cases. In case 1, note that a < M implies $a \in$ S_{cb}^1 . Note also that $b \leq M$ implies $b \in S_{ca}^1$, since

$$S^1_{ca} = \begin{cases} \{1, \dots, M\} \setminus \{a\} & \text{if } M < c \\ \{1, \dots, M+1\} \setminus \{a, c\} & \text{if } c \le M. \end{cases}$$

When b = M + 1, moreover, $c \le M$ by hypothesis, and hence $b \in S_{ca}^1$ again. In light of these observations, one can write $E_{ca} = E \cap (V_b > V_c = V_a)$ and $E_{cb} = E \cap (V_a > V_c = V_b)$, where $E = \text{up}_c(S_{ca}^1 \cap S_{cb}^1) \cap \text{down}_c(K \setminus S_{ca}^1 \cap S_{cb}^1) \setminus \{a, b, c\}$.

Let $\pi_{h,a} = E(V_h/m|E_{ca})$ and $\pi_{h,b} = E(V_h/m|E_{cb})$. Note that $\pi_h > 0$ implies $\pi_{h,a} > 0$ and $\pi_{h,b} > 0$. Let $\sigma_{h,a}$ and $\sigma_{h,b}$ denote the conditional standard deviations. Note that they are both finite and that both approach zero as m approaches infinity. Thus the distribution of $(V_1/m, \ldots, V_K/m)$, conditional on E_{ca} , collapses around its mean; that is, for any $\varepsilon > 0$,

$$\lim_{m \to \infty} \Pr[\bigcap_{h=1}^{K} (\pi_{h,a} - \varepsilon < V_h | m < \pi_{h,a} + \varepsilon) | E_{ca}] = 1.$$

A similar statement can be made for the distribution of $(V_1/m, \ldots, V_K/m)$ conditional on E_{cb} . Let *S* be the event

$$\bigcap_{h=1}^{K} \frac{V_h}{m} > \min \left\{ \pi_{h,a}, \ \pi_{h,b} \right\} - \varepsilon.$$

Note that because

$$S \supseteq \bigcap_{h=1}^{K} (\pi_{h,a} - \varepsilon < V_h/m < \pi_{h,a} + \varepsilon),$$

$$\lim_{m\to\infty} \Pr[S|E_{ca}] = 1.$$

Similarly.

$$\lim_{m\to\infty} \Pr[S|E_{cb}] = 1.$$

In light of these results,

$$\lim_{m\to\infty}\frac{\Pr[E_{ca}\cap S^c]}{\Pr[E_{ca}\cap S]}=\lim_{m\to\infty}\frac{\Pr[E_{ca}](1-\Pr[S|E_{ca}])}{\Pr[E_{ca}]\Pr[S|E_{ca}]}=0.$$

Thus since

$$\frac{\Pr[E_{ca}]}{\Pr[E_{cb}]} = \frac{\Pr[E_{ca} \cap S] + \Pr[E_{ca} \cap S^c]}{\Pr[E_{cb} \cap S] + \Pr[E_{cb} \cap S^c]},$$

it suffices to show that

$$\lim_{m\to\infty}\frac{\Pr[E_{ca}\cap S]}{\Pr[E_{cb}\cap S]}=0.$$

But

$$\frac{\Pr[E_{ca} \cap S]}{\Pr[E_{cb} \cap S]} = \frac{\Pr[E|S] \sum_{p \in \Xi} f_m(p)}{\Pr[E|S] \sum_{p \in \Xi} g_m(p)},$$

where

$$f_m(p) = \Pr[\bigcap_{h \in W} V_h = p_h m | E \cap S] \Pr[V_b > p_c m]$$

$$= V_a | \bigcap_{h \in W} V_h = p_h m],$$

$$g_m(p) = \Pr[\bigcap_{h \in W} V_h = p_h m | E \cap S] \Pr[V_a > p_c m]$$

$$= V_b | \bigcap_{h \in W} V_h = p_h m],$$

$$\Xi = \{ p \in \Delta^{K-1} \cap Q^K : \bigcap_{h \in W} V_h \}$$

 $= p_h m \subseteq E \cap S$ for some $m \in \mathbb{Z}$ and

 $W=K\backslash\{a,\,b\}.$

Fix $p \in \Xi$ and consider the limit as m approaches infinity of $f_m(p)/g_m(p)$. Note that

$$\frac{f_m(p)}{g_m(p)} =$$

$$\begin{cases} 0/0 & \text{if } p \notin \Xi_m \\ \Pr[V_b > p_c m = V_a | \bigcap_{h \in W} V_h = p_h m] \\ \frac{h \in W}{\Pr[V_a > p_c m = V_b | \bigcap_{h \in W} V_h = p_h m]} & \text{if } p \in \Xi_m, \end{cases}$$

where $\Xi_m = \{ p \in \Xi : p_h m \in Z \text{ for all } h \in W \}$. Note also that

$$\lim_{\substack{m\to\infty\\m\in\Xi^{-1}(p)}}\frac{f_m(p)}{g_m(p)}=0,$$

where

$$\Xi^{-1}(p) = \{ m \in \mathbb{Z} : p \in \Xi_m \},$$

This last result is derived as follows. Let $v_h = p_h m$ for all h. Conditional on $\bigcap_{h \in W} V_h = v_h$, V_a and V_b are distributed binomially, with parameters $\pi_a^* = \pi_a/(\pi_a + \pi_b)$, $\pi_b^* = \pi_b/(\pi_b + \pi_a)$, and $m^* = m - \Sigma_{h \in W} v_h$. If $2v_c$

 $\geq m^*$, then both probabilities are zero. Conditional on $2v_c < m^*$, we have $(V_a = v_c \rightarrow V_b > v_c)$ and $(V_b = v_c \rightarrow V_a > v_c)$, since $V_a + V_b = m^* > 2v_c$. Thus (the condition $m \in \Xi^{-1}(p)$ being understood in all that follows)

$$\lim_{m \to \infty} \frac{f_m(p)}{g_m(p)} = \lim_{m \to \infty} \frac{\Pr[v_c = V_a | \bigcap V_h = v_h]}{\Pr[v_c = V_b | \bigcap V_h = v_h]}$$

$$= \lim_{m \to \infty} \frac{\phi(v_c - m^* \pi_a^* / \pi_a^* (1 - \pi_a^*))}{\phi(v_c - m^* \pi_b^* / \pi_b^* (1 - \pi_b^*))}$$

$$= \lim_{m \to \infty} \exp \left\{ \frac{(v_c - m^* \pi_b^*)^2 - (v_c - m^* \pi_a^*)^2}{\pi_a^* (1 - \pi_a^*)} \right\}.$$

The last limit equals 0 because

$$\lim_{m\to\infty} v_c^2 - 2v_c m^* \pi_b^* + (m^* \pi_b^*)^2 - (v_c^2 - 2v_c m^* \pi_a^*)^2$$

$$+ (m^* \pi_a^*)^2) = \lim_{m \to \infty} (m^*)^2 (\pi_a^* - \pi_b^*) \left(\frac{2v_c}{m^*} - 1 \right) = -\infty.$$

The last equality follows because $m^* > 2v_c = 2p_c m$ and $p_c > 0$. Thus, $\lim m^* = \infty$, while the second term is a positive constant and the third term a negative constant.

In case 2, a > M + 1 implies $a \notin S_{cb}^1$. Moreover, since

$$S_{ca}^{1} = \begin{cases} \{1, \dots, M-1\} & \text{if } M \leq c \\ \{1, \dots, M\} \setminus \{c\} & \text{if } c < M, \end{cases}$$

 $b \ge M+1$ and b=M (since this entails $c \ge M$) both imply $b \notin S_{ca}^1$. In light of these observations, one can write $E_{ca}=E\cap (V_c=V_a>V_b)$ and $E_{cb}=E\cap (V_c=V_b>V_a)$, where $E=\operatorname{up}_c(S_{ca}^1\cap S_{cb}^1)\cap\operatorname{down}_c(K\backslash S_{ca}^1\cap S_{cb}^1\backslash a,b,c)$.

Given these definitions, the proof here proceeds identically to that in the first case if one substitutes the string $p_c m = V_a > V_b$ for every occurrence of the string $V_b > p_c m = V_a$ and the string $p_c m = V_b > V_a$ for every occurrence of the string $v_a > p_c m = V_b$. Somewhat larger changes are needed when one examines the limit of the ratio $f_m(p)/g_m(p)$, where, in this case,

$$\frac{f_m(p)}{g_m(p)} =$$

$$\begin{cases} 0/0 & \text{if } p \notin \Xi_m \\ \Pr[p_c m = V_a > V_b | \lim_{h \in W} V_h = p_h m] \\ \frac{h \in W}{\Pr[p_c m = V_b > V_a | \lim_{h \in W} V_h = p_h m]} & \text{if } p \in \Xi_m. \end{cases}$$

Conditional on $\bigcap_{h \in W} V_h = v_h$, V_a and V_b are distributed binomially, with parameters $\pi_a^* = \pi_a/(\pi_a + \pi_b)$, $\pi_b^* = \pi_b/(\pi_b + \pi_a)$, and $m^* = m - \Sigma_{h \in W} V_h$. If $m^* < v_c$ or $m^* \ge 2v_c$, then both probabilities are zero. Condi-

tional on $v_c \le m^* < 2v_c$, we have $(V_a = v_c \to V_a > V_b)$ and $(V_b = v_c \to V_b > V_a)$. Thus (the condition $m \in \Xi^{-1}(p)$ being understood in all that follows),

$$\lim_{m \to \infty} \frac{f_m(p)}{g_m(p)} = \lim_{m \to \infty} \frac{\Pr[v_c = V_a | \lim_{h \in W} V_h = v_h]}{\Pr[v_c = V_b | \lim_{h \in W} V_h = v_h]}$$

$$= \lim_{m \to \infty} \frac{\phi(v_c - m^* \pi_a^* / \pi_a^* (1 - \pi_a^*))}{\phi(v_c - m^* \pi_b^* / \pi_b^* (1 - \pi_b^*))}$$

$$= \lim_{m \to \infty} \exp\left\{ \frac{(v_c - m^* \pi_b^*)^2 - (v_c - m^* \pi_a^*)^2}{\pi_a^* (1 - \pi_a^*)} \right\}.$$

The last limit equals zero because

$$\lim_{m \to \infty} v_c^2 - 2v_c m^* \pi_b^* + (m^* \pi_b^*)^2 - (v_c^2 - 2v_c m^* \pi_a^*) + (m^* \pi_a^*)^2 = \lim_{m \to \infty} (m^*)^2 (\pi_a^* - \pi_b^*)(2v_c m^* - 1) = -\infty.$$

The last equality follows because $m^* < 2v_c$ (so the last term is a positive constant), the second term is a negative constant, and $m^* \ge p_c m > 0$ (so $\lim m^* = \infty$).

LEMMA 3. (a) If $\pi_1 > \pi_M$, then $\lim_{m \to \infty} T_{1j} / T_{M,M+1} = 0$ for all j. (b) If $0 < \pi_j < \pi_{M+1}$ for some j > M+1, then $\lim_{m \to \infty} T_{jk} / T_{M,M+1} = 0$ for all k.

Proof Part 1. By Corollary 1, it suffices to show that $\lim_{m\to\infty}\Pr[E_{1j}]/\Pr[E_{M,M+1}]=0$ for all j. And this follows because (1) $\lim_{m\to\infty}\Pr[E_{1j}]/\Pr[E_{1M}]=0$ for all j< M (by Lemma 2, case 1); (2) $\lim_{m\to\infty}\Pr[E_{1j}]/\Pr[E_{1,M+1}]=0$ for all j>M+1 (by Lemma 2, case 2); (3) $\lim_{m\to\infty}\Pr[E_{1M}]/\Pr[E_{M,M+1}]=0$ (by Lemma 2, case 1); and (4) $\lim_{m\to\infty}\Pr[E_{1,M+1}]/\Pr[E_{M,M+1}]=0$ (by Lemma 2, case 1).

Proof Part 2. By Corollary 1, it suffices to show that $\lim_{m\to\infty} \Pr[E_{jk}]/\Pr[E_{M,M+1}] = 0$ for all k. And this follows because (1) $\lim_{m\to\infty} \Pr[E_{jk}]/\Pr[E_{j,M+1}] = 0$ for all k > M + 1 (by Lemma 2, case 2); (2) $\lim_{m\to\infty} \Pr[E_{jk}]/\Pr[E_{jM}] = 0$ for all k < M (by Lemma 2, case 1); (3) $\lim_{m\to\infty} \Pr[E_{j,M+1}]/\Pr[E_{M,M+1}] = 0$ (by Lemma 2, case 2); and (4) $\lim_{m\to\infty} \Pr[E_{jM}]/\Pr[E_{M,M+1}] = 0$ (by Lemma 2, case 2). QED.

Proof of Theorem 1. Suppose $\pi_1 > \pi_M$ and consider a voter of type u. Let $X_j = \{k \in K: \pi_k = \pi_j\}$ and call any candidate in $X_M \cup X_{M+1}$ "marginal." Let h be the marginal candidate whom the voter ranks highest: $u_h > u_j$ for all $j \in X_M \cup X_{M+1} \setminus \{h\}$. (Such a candidate exists because all voters have strict preferences among candidates.)

Now consider ξ_h^n , the expected utility increment of voting for h rather than abstaining, when the electorate is of size n. From Appendix A. $\xi_h^n = \sum_{k=1}^{K} \frac{T_h^n(u_k - u_k)}{T_h^n(u_k - u_k)}$

given π). Note that $\xi_h^n>0$ for all n, because (1) in the limit, ties between marginal candidates dominate those involving nonmarginal candidates, by Lemma 2 and (2) $u_h-u_k>0$ for all $k\in X_M\cup X_{M+1}\setminus\{h\}$. Note also that $\lim \xi_1^n/\xi_h^n=0$, by Lemma 3. (One can assume without loss of generality that $h\leq M+1$, since $h\in X_M\cup X_{M+1}$.) Thus, in the limit, no voter will vote for candidate 1, given expectations π , which contradicts the expectation that $\pi_1>0$. This shows that for large enough n, π is not a rational expectation. But there may be rational expectations arbitrarily close to π .

In order to show that one cannot find any rational expectations "close" to π , consider a sequence $\{\pi^n\}$ of rational expectations that converges to π . The discussion leading up to Lemma 1, and the proof of that lemma, show that

$$T_{hk}^{n} = \sum_{b=1}^{\binom{M-1}{K-2}} \sum_{v \in E_{ak}(S_{hk}^{b})} (n-1)! \prod_{j=1}^{K} \frac{\pi_{j}^{v_{j}}}{v_{j}!},$$

where

$$\bar{E}_{hk}(S_{hk}^b) = \{ v \in Z^K : \lim_{h \in K} V_h = v_h \in E_{hk}(S_{hk}^b) \}.$$

Thus T_{hk}^n is a continuous function of π , and so is ξ_h^n . Let $\xi_h^n(\pi^n)$ be the expression resulting when one substitutes π^n for π in the expression for ξ_h^n . The continuity of ξ_h^n in π , together with $\pi^n \to \pi$ and $\xi_h^n > 0$ for all n, implies $\xi_h^n(\pi^n) > 0$ for all n sufficiently large. The ratio ξ_h^n/ξ_h^n is continuous in π , over the set of all π such that ξ_h^n is not zero. Thus $\lim_{n \to \infty} \xi_h^n(\pi^n)/\xi_h^n(\pi^n) = \lim_{n \to \infty} \xi_h^n/\xi_h^n = 0$, the latter equality having been established above; that is, in sufficiently large electorates with expectations sufficiently close to π , no voter will vote for candidate 1, contrary to the assumption that $\pi_1 > 0$. A similar proof works for the other part of the theorem.

Notes

I thank Skip Lupia, Barry Nalebuff, Tom Palfrey, and especially Roger Myerson for their helpful comments. Thanks also to Jonathan Katz for suggesting and coding the Silverman bimodality tests conducted in the fourth section. This work was supported by the National Science Foundation under grant number SES-9208753.

1. The Japanese and Taiwanese require that a candidate get above a legally defined minimum vote share in order to win a seat. The seat allocation rule, in other words, is not pure plurality. Nonetheless, this provision is of negligible practical importance.

2. The adjective strategic in strategic voting equilibria is really superfluous in that agents in the model conform to the usual postulate of rationality and are not distinctly more strategic than agents in any other rational choice model. The term strategic voting is well established in the literature, however, and I shall follow conventional usage.

3. Myerson and Weber (1993) suggest that considerations of dynamic consistency or persistence may eliminate the

- 5. Formally, the assumption is this. Let each voter order the candidates from largest π_{ij} to smallest. Let M(i) be the candidate ranked Mth in is ordering and M + 1(i) be the candidate ranked (M+1)st. Let $Mid(i)=\{j:\pi_{ij}=\pi_{iM(i)}$ or $\pi_{ij}=\pi_{iM+1(i)}\}$. Lead $(i)=\{j:\pi_{ij}>\pi_{iM(i)}\}$, and $Trail(i)=K\backslash Mid(i)\backslash Lead(i)$. Then, for all voters i and i, Lead(i)=Lead(i)and Trail(i) = Trail(h).
- 6. Because there are no atoms in the type distribution F_{r} nonsingleton $V(u; \pi, n)$ s do not require special handling.

7. I thank Roger Myerson and Barry Nalebuff for greatly

clarifying what this theorem ought to say.

8. Trailing candidates find allies in their attempts to avoid the logic of the wasted vote in front-runners who expect a net loss should the trailing candidacy go down the tubes (e.g., recall Ronald Reagan's support of John Anderson's candidacy in 1980) and foes in front-runners who expect a net gain of support (recall Jimmy Carter's persistent reminders to voters not to waste their vote on Anderson).

9. The data used in this analysis are from Steven Reed's compendium Japan Election Data: The House of Representatives 1947-1990 (Ann Arbor: Center for Japanese Studies, 1992).

10. Note that there is an artifactual expectation of precisely the opposite character. Looking at the mathematical definitions of excess and MARGIN given in the notes to Table 1, one sees that V_M , the vote percentage of the Mth-place candidate, appears with a negative sign in the expression for EXCESS and with a positive sign in the expression for MARGIN. Thus one expects an artifactual negative relationship. If the theoretically expected positive relationship nonetheless appears, one can be more confident that it corresponds to some real political effect.

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THE TOWER OF BABEL AS A COORDINATION GAME: POLITICAL LINGUISTICS IN GHANA

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The problem of choosing an indigenous official language for multilingual states in general and for Ghana in particular is treated as one of coordination in an n-person tipping game. Even with an assumption that the equilibrium outcome of all-English is deficient, the mechanisms for reaching an indigenous language solution are difficult to find. A lottery (a theoretically attractive approach to the solution of coordination games) is less attractive when applied to the issue of language coordination. Empirical data based on interviews from six different regions of Ghana show the limits and possibilities of the theoretic solution. Going back to theory, a mechanism for the successful promotion of an indigenous language outcome is proposed.

od's punishment for the audacity of building the Tower of Babel sets up a neat n-person coordination game for the world's scattered peoples. Individuals from each language group may want to communicate directly with members from other language groups, but they have to agree upon a common language to do so.¹

In world history, the Babelian curse has been negotiated in a variety of ways. The most common solution is based on simple market conditions: high levels of contact between peoples of different language groups induce new speech forms, permitting intergroup communication. Over a long expanse (e.g., of a river or through a subcontinent), linguists have discovered language chains. People at point A communicate effectively but haltingly with neighbors at point B, who themselves communicate with their neighbors at point C, and so on; but people at point A cannot communicate at all with people at point N. Dialect chains are not the only solution; often pidgins or lingua francas help overcome the Babelian curse.² All these solutions are based on the microdynamics of language coordination across space and time, without state-enforced contracts or legislation.

Market solutions are not always efficient. Consider two separated language groups that due to a technological change (say, the spanning of a river by a bridge) form a unified economic region. Suppose all people in this region are monolinguals in one of two languages, L and L1. It may be Pareto efficient (especially if transfer payments are permitted) for speakers of L to switch to L1, or vice versa. Even if it were agreed which language was a better coordination point, however, it might require a critical mass of people to change before it would be rational for any single person to change. This is the core insight in Schelling's "tipping game" (1978, chap. 7). Without any stimulus for organizing a critical mass of L or L1 speakers to switch, a class of professionals (whose learning costs are lower than the average for the population) will likely emerge and charge for translation services. Over a few generations this will be a costlier solution than early agreement by all on a common language.

Rulers have an interest in promoting efficient language outcomes. The failure of the market to overcome the inefficiencies of the Babelian curse adds to the transaction costs of rule, if only because governmental monitoring of compliance to its laws is more costly if the rules themselves must be translated into a number of languages. It adds further to the costs of rule if agents of the ruler need to translate local documents such as account books (for purposes of taxation) into the language of rule. Rulers interfere in the marketplace of languages not only because the transactions costs of rule increase when the language coordination problem is unsolved but also because lowering the transactions costs of business internal to the state raises the taxable surplus (Friedman 1977).³

This helps explain why numerous rulers of states and empires have sought to manipulate language use in the societies they rule. Rationalization—the authoritative imposition of a single language for educational and administrative communications—is only one possible strategy (Laitin 1988). Other forms of managed coordination, such as language zoning in Switzerland and an administratively and educationally induced lingua franca in the former Soviet Union, are state-influenced coordination outcomes.

Rulers have an interest not only in lowering transaction costs within their boundaries but in raising those costs across boundaries. Solving the Tower of Babel curse within a boundary may amplify that curse internationally. Despite the apparent efficiency of a single-world language, however, it may not be a superior equilibrium to one in which each country (or region) specializes in a single language. Taking cues from work on the optimal size of firms (Williamson 1986, chap. 3) and on the potential efficiencies of the development of particularistic corporate cultures (Kreps 1990), I give more weight here to the issue of creating national cultures and less to the issue of an efficient world-language regime.

Rulers of states that experienced colonial rule and received their independence in the twentieth century have faced a far more difficult task in rationalizing language within their boundaries than leaders who consolidated states in earlier centuries. These twentieth-century leaders of new states have found that language issues are particularly volatile when they become politicized. I shall explain why coordination on an indigenous national language has been difficult for such leaders. I will focus on the language situation in Ghana, but the dynamics of the situation are similar for many multilingual African countries that lack any indigenous lingua franca that could serve as common language of education, administration, and public life (e.g., Zaire, Nigeria, Senegal, and Ivory Coast).⁴

My goals here go beyond a strategic analysis of why state-induced coordination on a national language is difficult to achieve. Through a detailed case study of Ghana's linguistic history and a survey of language use and attitudes, I shall illustrate the difficulty of applying in the real world certain theoretically elegant solutions to the Tower of Babel curse. Finally, I shall propose a linguistic strategy that, while not yielding language rationalization, does indeed permit the institutionalization of an indigenous national language in countries like Ghana.

GHANA: SOCIOLINGUISTIC BACKGROUND

Ghana in the 1950s was the African country with the strongest and most publicized national movement. Kwame Nkrumah, the charismatic leader of the Convention Peoples party and of Ghana itself, was a committed pan-Africanist. He believed that true freedom demanded that the roots of European domination be dug out. Yet he, like virtually all African nationalists, had to rely on a European language to make his message understood by at least some persons in each part of his country. His first language was Nzema, a language spoken by only a small percentage of the people of Gold Coast (the colonial appellation for today's Ghana). What sort of language policy was possible under conditions that Nkrumah faced? Ghana, like nearly all African countries, seems to have been a principal victim of God's wrath aimed against those who constructed the Tower of Babel.

Ghana had a population of 14.5 million in 1980. The official language (i.e., the language used in educational institutions and for conducting government business) is English. Akan speakers make up 44.1% of the population. (Twi, Fanti, Brong, Akim, and Nzema, are the principal languages of the Akan family.) The Mole-Dagbani of the North and Upper regions constitute 16%, the Ewe of Volta Region 13%, Ga-Adangbe of Greater Accra and parts of the Eastern Region 8.3%, the Guang of the East and parts of the North 3.7%, the Gurma of the North and Upper regions 3.5%, and the remaining groups (including the Kasem in the North) 11.4% (Chazan 1983, 35). Very few Ghanaians consider themselves to be Hausas; yet the Hausa language is broadly understood throughout the northern regions, as well as across the west African savanna; it is probably the most widely understood language in west Africa today.

Linguists point to the broad zone of Akan languages in Ghana's southern and middle belts-an area in which there have historically been significant political, social, and economic interactions among peoples. There was, at least during the period of Asante dominance in the precolonial era, reasonable mutual intelligibility among speakers of these Akan languages. But missionary and colonial policies (at least up through World War II), by providing educational and administrative benefits based on tribal boundaries, gave incentives for local chiefs to emphasize linguistic differences from their neighbors. Hence, from the late nineteenth century, a spiral of language differentiation ensued. By 1950, Akan speakers were more differentiated linguistically than they had been 100 years earlier (Dickens 1953).

The colonial state's language policy was not fully coherent. On the one hand, and consistent with the Colonial Office's view that rule could be cheaply administered if colonial administrators ruled indirectly through native chiefs, the colonial state sought to encourage basic education in mother tongues. This is why it gave in to demands of local chiefs for the official recognition of their languages. On the other hand, ease of administration demanded that a class of Ghanaians be literate in English. Thus, a mixed policy developed. In the primary schools, the vernaculars usually served as the media of instruction; but in the secondary schools, the medium became English (Kitchen 1962, 334–36).

The independence movement sought both to overcome the reliance on foreign cultures and to induce a more unified African or Ghanaian culture. A search for a Ghanaian national language was thought to be a sure remedy. But the independent state could find no more coherent language policy than that of the colonial government. Whiteley nicely sums up the problem:

In 1961 a Private Member's motion was tabled in the Ghana National Assembly to the effect that "... in the opinion of this House it is desirable that the Akan language should be taught in all schools in Ghana." . . . The motion was defeated, but during the debate many points were raised which have appeared again and again . . whenever language matters are discussed. A majority were in favor of retaining English as the official language not only because of its international status, but also because of its unifying role within Ghana. Yet, even among those who supported its retention it was recognized that original contributions in, say, literature, might not be forthcoming so readily in English. . . . The Deputy Minister of Education, Mrs. Susannah Al-Hassan is reported to have summarized official policy in the following terms: ". . . it is the intention of Government to encourage the development of all our major national languages so that each of them may have an equal opportunity of attaining a standard which would improve its chances of being chosen as the national language when the time comes for such a decision. . . . In the meantime, I think honourable members will agree that we have enough on our plate to keep us busy for a few more years, without having to worry about a common language." (1971, 552-53).

Governments changed repeatedly over the next generation, but the language policy of Al-Hassan remained untouched. The staff at the Bureau of Ghana Languages in Accra has analyzed and developed a number of indigenous languages, but their books are usually neglected by school authorities, who often refuse to purchase or to assign them (*People's Daily Graphic* 1990).

In the early 1990s there was some movement on the official language front. Jerry Rawlings, Ghana's leader, inadvertently reactivated the language issue. In an attempt to reduce the power of Ghana's regions, Rawlings enhanced the powers of the districts and increased their number from 65 to 110. As a consequence of this change, nearly all districts are now linguistically homogeneous. In the first elections for district assemblies, candidates who had no facility in English were permitted to run. Political debate in the assemblies, especially in rural districts, began to take place in indigenous languages. Debates have been lively and participation active. This is the most important official use of indigenous languages in modern Ghana even if English remains the sole official language of law and administration.5

The vibrancy of political discourse relying on indigenous languages in the district assemblies reflects the sociolinguistic reality in which the indigenous languages of Ghana are used in many walks of life. Even in the schools, the indigenous languages have a semiofficial role. In a 1991 survey of Cape Coast, where English is extremely prestigious and widely used, Fanti remains a language of considerable importance in many social domains.6 To be sure, English was the official medium of instruction throughout primary, junior secondary, and senior secondary schools; yet in 95% of the primary schools surveyed, teachers reported using Fanti as a medium of instruction instead of English, at least some of the time. Most teachers reported that the children who begin school cannot understand basic directions or concepts in English. At junior secondary school, a few teachers report using Fanti to explain concepts, but English becomes the language of instruction and continues that way up through the grades. English dominates recreational reading at all levels largely because the only available works in Fanti are specialized language and literature texts.

But Fanti does not progressively get eliminated from the lives of advanced students. Through secondary school, Fanti predominates over English on the soccer fields and in informal singing. Furthermore, Fanti is a required subject for students all through their school careers. And teachers report almost unanimously that non-Fanti students have little trouble in learning Fanti. In our survey, no teacher reported any resistance of non-Fanti students in learning Fanti, and many reported that non-Fanti students often did better in the Fanti examinations than the Fanti students. Fanti remains the language of informal domains. Speaking English in those domains is considered overly formal and distant.

Pidgin has specialized functions in Cape Coast

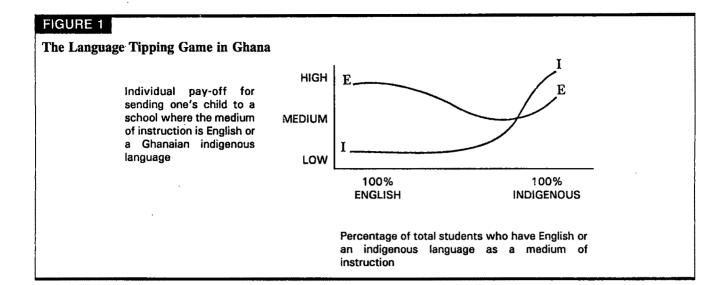
schools—to tell off-color jokes or to relate incidents in highly informal settings that are especially amusing.⁷ But teachers are so emphatic that they never-and their students rarely—use pidgin that one suspects otherwise. Many teachers put exclamation points after the no in answer to our question as to whether they use pidgin in the staff room. One teacher wrote that pidgin "is used by students when they want to express themselves without others understanding them." This comment reveals more about the teacher's values than it does about the use of pidgin. The project supervisor, however, observed interactions in these schools and claimed that there was no underreporting of pidgin. Results from a wider survey (to be described shortly) come to a similar result. When asked what they would do if a young person addressed them in pidgin, only 15% of our respondents said they would answer in pidgin. On the question of whether Ghanaian writing should rely on the Queen's English, a large number of respondents felt it important to keep to the international standard. "If we are learning someone's language," one respondent offered, "then it is better we learn it in its original form so that he will not have any advantage over us" (K11).8

If pidgin remains limited in use and low in prestige in Ghana today, there is a speech form that relies heavily on Akan-English code switching, and this form is evidently used far more widely than even its speakers recognize. In interviews, a number of speakers denied relying on it while using it extensively in those very interviews. The speech form plays an important role in interethnic communication and has no overtones of belonging to a particular group, despite its extensive reliance on Akan languages. Furthermore, linguistic analysis of this speech form suggests its emergence as a natural language (Forson 1979). I shall refer to this emergent language as Ghanaian. In contemporary Ghana, Ghanaian has a role not unlike that of "English" during the period of Falstaff and Prince Hal: it is a widely spoken language of popular culture whose speakers do not consider it a "real" language.

In contemporary Ghana, African languages remain in limbo. English is clearly the language of expertise, of central administration, of international business. Yet the African languages remain vibrant in many social domains, and since there are virtually no monolingual Ghanaians in English, there are no discernible trends pointing to the death of African languages. Nationalism seems strong because Ghanaians retain their ancestral languages with low economic pay-offs for doing so; yet it seems weak because Ghanaians put up little resistance to the everwider use of English in public life.

LANGUAGE COORDINATION AND THE TIPPING GAME

The "story" that motivates my theoretical models is as follows. Most Ghanaian citizens are convinced that



it is necessary to learn English to get a well-paid job in the modern sector and surmise from this that it is unrealistic to educate their children through the medium of an indigenous language. But most Ghanaians have inchoate feelings that continued reliance on English is tearing them from their own cultural roots. This reliance is seen as a remnant of colonialism. In terms of linguistic policy, Ghanaians are pulled in opposite directions.

This ambivalence can be portrayed graphically (see Figure 1) as a *tipping game* (Schelling 1978, chap. 7). The Babelian tipping game has six characteristics. First, the binary choice is for parents to enroll their children in English medium schools (where indigenous languages become restricted for family domains) or to seek out schools where a Ghanaian language is the medium of instruction (and where English or other international languages are taught only as subjects).

Second, virtually all schools in Ghana rely on English as the medium of instruction, and all jobs in the modern sector require a good working knowledge of English. The payoffs for educating your child in English when 100% of official business is conducted in English are far higher than the payoffs for educating your child in an indigenous language. This represents the present equilibrium.

Third, the game is one of coordination, not pure conflict. Each Ghanaian parent wants her children to be educated in the common language of official business and elite discourse. An outcome in which Ghanaians used languages that were not easily understood by large numbers of fellow Ghanaians for correspondence, petitions, and television programs would bring substantial disutility to all. (But at 100%-English, any mover to the indigenous language would suffer great losses, while the loss for n-1 Ghanaians would be small.)

Fourth, figure 1 indicates that if there were coordination in an indigenous language (i.e., 0% of official business conducted in English), the individual payoff for having been educated in that indigenous lan-

guage would be higher than the payoff for having learned English when 100% of official business is conducted in English. (This assumption will be given a plausibility test in the empirical data to be presented shortly.)

Fifth, calculating the value to an individual at 100%-indigenous is complicated by the fact that individuals do not know which indigenous language would be official. To calculate their value for education in an indigenous language, then, individuals must rely on calculations of expected utility. Suppose we were to ask an individual i about personal utility for an outcome that is at 100%-indigenous in a country with k languages. Individual i would sum the probability of each language in set k being designated as the official language multiplied by personal utility for that language at 100%-indigenous. Her calculation would be: 11

$$EU_i = \sum_{\forall k} P_k U_{ik}$$

Since the expected utility of individuals from different language groups differs, line *I-I* on Figure 1 represents the average payoff for all citizens, whose individual payoffs may diverge considerably from the mean. (The wide divergence, based on the different utilities for speakers of different languages, makes the Ghana case different from representations of regional revival movements—in Quebec, Flanders, and Catalonia for example—where there has been agreement about the language that would replace the status quo.)

Sixth, this tipping model captures two coordination problems. First, we have a game of pure coordination. We see that 100%-indigenous may be the Pareto superior equilibrium (i.e., at least as good as the other equilibrium for all and better than the other for at least one), but it would be irrational for anyone to move unilaterally from the status quo. This is because the status quo is a Nash equilibrium, which means

that no player has an incentive to leave unless coordinated with other players.

The second coordination problem resembles the game battle of the sexes. (In this game, both members of a couple prefer to be together but differ as to where it should be. Each would rather prevail but both would rather coordinate than separate. The problem is where to go.) The coordination problem here is in the specification of the indigenous language to be chosen as the official language of state business. Many Ghanaians would prefer to rely on a common indigenous national language but differ as to which it should be. Each would rather prevail, but many would rather coordinate on choosing an indigenous language than separate themselves from their cultural roots by relying forever on English as Ghana's official language.

EMPIRICAL EVIDENCE

The intuition behind this theoretical analysis is that there may be a Pareto efficient language policy in Ghana that has not yet been properly identified. But how are we to know whether an alternative dominates the status quo without knowing the subjective preferences of Ghanaians themselves? How are we to be sure that basic assumptions about language choice, embedded in the models, reflect social reality?

With these questions in mind, Edward Mensah and I drafted a survey and collected 39 responses from Ghanaians living in the Chicago area. The results were encouraging for the assumptions behind our models, and we reported them (Laitin and Mensah 1991). We redesigned the survey instrument and pretested it in Ghana in October 1990 while training graduate students to administer it. In December 1990, a revised interview schedule was administered to 300 respondents in six distinct regions of the country, so that we have interviews with 50 respondents each from the Dagbani, Ewe, Fanti, Ga, Kasem, and Twi language zones.

Interviewers (1) were indigenous to the area in which the interviewing was done, (2) introduced themselves alternately in the local language and in English and then asked the respondent which language he or she would prefer for the administration of the interview, (3) were given targets for people in certain social roles but without other provisions for representativeness, and (4) were encouraged—indeed, rewarded—for writing down in a separate book any side comments and criticisms that respondents offered during the interview.

The survey results give us a handle on some important issues in the political resolution of language coordination problems. I shall examine the assumption that the expected value of 100%-indigenous is higher than 100%-English and address the issue of side payments, a mechanism crucial for coordination games in general but apparently out of place in the realm of language choice.

Constitution of the Tipping Game

Establishing an individual payoff at 100%-indigenous that is higher than at 100%-English raises the thorny issue of calculating costs and benefits not only for learning a language, but in the sense of resentment that a majority of Ghanaians whose language is not chosen—and pride that the minority whose language is chosen-will feel if any indigenous language is chosen as Ghana's official language. The sense of pride in having an indigenous language as Ghana's official language must be weighed against the costs of developing sufficient teaching materials in a language that remains marginalized from international science and technology. And then there is the problem of administrative cost. The acceptance of English did not involve up-front costs for newly independent states. Meanwhile, the creation of mechanisms for reallocating costs in the imposition of an indigenous language is itself costly and should work to reduce the overall value of 100%-indigenous. For purposes of this preliminary discussion, the payoffs for a language choice are based only on the subjective assessment of the individual (or family) economic returns for having developed facility in a language and of psychological returns for having an official language that enhances social (or national) solidarity.

Respondents were invited to make these subjective calculations. Indirectly, a case can be made that ceteris paribus, the respondents would prefer to live in a Ghana under conditions of 100%-indigenous. However, there is some direct evidence against my assumption: on the simple question whether respondents preferred English to become the dominant language for official communication in Ghana or a single Ghanaian language to replace English, 60% chose English, and 40% chose the Ghanaian alternative

The reasons for the preference for English need, however, to be analyzed. Some respondents who chose English pointed to the situation for small and relatively poor states in the international political economy. Citizens of those states must compete, respondents argued, for international jobs, and a solid education in English is an essential skill for successful penetration in the job market. As one respondent put it: "It would not have been any problem if Ghana were to have a vibrant economy that can take care of our needs. . . . The Japanese or Germans for instance don't need to learn someone else's language because you can grow and work in the economy of your country without having to travel elsewhere. What English does for instance is to offer us the opportunity to seek greener pastures elsewhere if it becomes necessary" (K16).

Other respondents who chose English articulated a belief that indigenous languages are best used for home life, to create solidarity, and to express emotions that simply cannot be done in a language that you cannot consider your own. For their sense of rootedness, they said, there need be only a few

domains in which one uses one's indigenous language. A considerable number of respondents who chose to be interviewed in the language of home use found themselves unable to carry forward with the interview in that language and began using English. The interviewer of the Ga respondents noted that one person who chose to be interviewed in Ga "slipped" into English, and then "the answers started coming out" (G4). One Kasem respondent switched to English when he found the interview process to be "book long" and therefore more appropriately done in English (K32). Yet despite this loss of control over one's mother tongue for extensive conversation about national political issues, respondents would never consider giving it up for family functions. 12 The perspective from the international job market (favoring English) complemented by a view of the proper language for private domains (favoring one's mother tongue) would put a low value on the successful imposition of an indigenous language as the official language of Ghana.

But most of those who chose English as better than an indigenous language as the sole official language for Ghana did so because of practical problems in reaching a new equilibrium, not necessarily because the new equilibrium would be deficient. Many expressed the belief that no Ghanaian language is sufficiently "developed" in technical vocabulary or in scientific precision to play an important role in official communication. Others felt that although it would be good in the abstract, any language chosen would give its members too much "pride," which would be destructive to peaceful intergroup relations. The practical problems of implementation were so impressive as to color people's assessments of the payoffs should it be accomplished. As one respondent put it, "Even Kwame Nkrumah tried and it did not work so I do not think a single Ghanaian language can replace English as a dominant language for official communication" (G11).

Furthermore, the positive abstract support for a single indigenous language comes through clearly in the interviews. One respondent, before recounting the difficulties, said that "if there were a single Ghanaian as an official language its usage will have been ideal" (F23). Another pointed out: "One official language will identify us as a state. We have adopted English which has suppressed us" (G14).

Based on these data, I cannot provide an empirically justified expected value of 100%-indigenous and compare it with 100%-English, especially if values other than income are reckoned, such as pride and dignity and the chance to be citizens in a more powerful and coherent state. But I think it reasonable, given the reasons that respondents gave in their justifications for the choice of English as the sole official language of Ghana, to interpret these interviews as saying that transition costs aside, Ghanaians would prefer to live in a world of 100%-indigenous over 100%-English.

Will Side Payments Provide Incentives for Language Shift?

The usual way, theoretically, to induce movement from the status quo in an n-person tipping game is to provide selective incentives or side payments for particular individuals to change their language behavior (before the tipping point is reached) even though the average payoff for those who change will be lower than the average payoff for those who remain with the status quo. 13 Side payments involve compensating speakers of the nonchosen languages for their learning costs. Pool (1991) has demonstrated the theoretic possibility of accurately assessing costs (of learning and translation) so that the transition burdens can be made equitable. But he has not demonstrated that the costs of learning a new language and the costs of translation are the most salient costs in actual political cases. The psychological costs of personal complicity in the death of one's own language may be a principal concern of citizens.

The assessment of costs and benefits, our survey shows, takes place after the issue has been framed. Language shift can be framed as an efficiency measure or as a symptom of cultural death. Our data suggest that the big hurdle in implementing a policy through side payments has more to do with these "framing effects" than it does with getting the learning and translation costs right (Goffman 1974; Tversky and Kahneman 1986).¹⁴

In some frames, costs do not seem to matter. The costliest solution in Pool's analysis for "losers" is for them to learn the newly chosen language without compensation from either the winners or the state. Yet respondents had little trouble accepting as normal the costs when they were framed in a diffuse way. We asked: "If Ghana were to choose a single African language as the standard language for all official, educational matters and it was one from a region different from yours, would you (a) Seek to learn that language quickly to enable you to understand official speech? (b) Organize to get for your own language equal recognition? (c) Other (please specify)?" On this question, 226 out of 294 respondents (77%) said they would seek to 'learn' the language. Put this way, respondents make the arbitrary choice of an indigenous language as official look like a good (and cheap, for the state) idea.

Side payments suggest a different frame. When we proposed them, respondents were less positive about accepting the idea of the imposition of an indigenous language foreign to them as Ghana's official language than when we asked if they would learn or organize. We asked a series of questions as to whether they would be willing to accept a Ghanaian language other than their own as the medium of instruction in their children's school if the students were given job guarantees by the state for having made the language sacrifice. Only 26% of the respondents were willing to take any side payment offered; 31% were willing to take side payments only when considered useful; and 43% were adamantly unwilling. 15

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Many respondents could not take the job promises seriously. Either they had no trust that their government would fulfill such promises, or they calculated that the expected value of a government job (without knowing English well) was far less than the value of English literacy with the enhanced value of international migration. One respondent said, "If the child has no English he would be permanently handicapped in his search for a job particularly in the international labour market" (K21). A woman who spoke no English and was willing to accept tax transfers that virtually no other respondents could abide answered no to the job incentive: "To get a 'white man's' job to do, you must speak and write English and since that is what we all want our children to do, I will like him to study English well" (K43). Some respondents clearly saw that the international labor market offered better prospects than did the Ghanaian government, whose promises few respondents would take seriously. One interviewee responded to the job incentive by saying: "Your questions are funny. If he learns no Akan and the government does not give him a job, he will farm" (D2).

If job incentives had some takers, our proposal that tax transfers be used to compensate losers had virtually no takers at all. In these questions, we mentioned specific languages (Akan, Hausa, and Ewe) as chosen languages. Some respondents said no on expected value calculations. For example, a respondent from outside the Akan-speaking zone mentioned Akan earlier in the interview as the obvious choice for a national language. Yet he rejected the job offer for his children if they were to study in the Akan medium "because . . . the money involved in learning Akan will be greater than the tax payment" (K50). In a similar vein, another respondent claimed, "I will prefer paying the tax to learning any other language as it is more expensive to learn language" (G3).

Most respondents did not perceive this question in terms of utility but, rather, in terms of morality. They saw these questions in the frame of a nefarious bribe and refused even to calculate: "This is pure bribery, it can never work, not here alone, all over the country" (E25). Many respondents, after hearing this offer, excoriated the interviewer for even posing the question-or for working for a biased scientist or for supporting an underground project to impose a foreign culture on them: "Is the fellow who designed the questions an Akan? He is biased" (D12). Chauvinistic comments about the respondents' own languages were brought to the fore: "We can't allow our language to die; it will mean the death of our very essence" (K48); "To neglect my language for another person's language, over my dead body" (F8); "I don't want my language Fanti to die a natural death and lose my cultural identity" (F7). Complementing the chauvinism and showing a reversal of the easy toleration most respondents demonstrated when they agreed to learn the language of the chosen language on their own, some respondents showed a disdain for the mentioned languages:16 "Why do you force Akan on us. This is annoying" (D9); "I know they

want us to accept Akan. But let me tell you this is a dream. It is going to bring war if the government should force Akan on the people" (D14); "Even if they force Akan on us, we will not learn" (D28); "Akan is a thing I detest. I prefer English" (G40). When non-Ewes were questioned about Ewe, the reverse feelings were expressed. One Twi respondent argued that Ewe should not have been mentioned at all: "No Akans feel good about it" (T20). A Kasem respondent rejected the tax proposal for learning Ewe: "Ewe is not known here; what is more, the Ewe people are not liked by most tribes in Ghana" (K13). In sum, the frame that the tax question put around the language picture is graphically exemplified by a Ga respondent who pleaded, "I cannot sell my birthright" and then criticized the interviewer: "You just can't kill one language and use it as a manure for another" (G14).

Much more work in analyzing our present data and collecting new data will be necessary to understand how language incentives can be successfully framed. Our interview schedule did not allow us, for example, to distinguish whether it was the idea of a tax or the mention of a specific language that evoked strong emotional outbursts. None the less, the interviews demonstrate that people indeed make expected value assessments in regard to language but that framing effects are powerful. We have seen that people are more amenable to diffuse than to specific commitments in regard to language investments. Further, we have seen that people have clear notions about inappropriate incentives for changes in their language repertoires that are not guided by issues of price. Incentive schemes for language change that do not factor in these framing effects are likely to trigger popular responses that will appear irrational to policymakers.

CAN THE 100%-ENGLISH EQUILIBRIUM BE UNDERMINED?

The Market Place of Languages Without Direct Political Interference

The present equilibrium based on the Schelling diagram approaches 100%-English, where all the official government documents, schools, and big businesses operate in English. This, of course, is a rather stylized view of the language repertoires that individual Ghanaians must master if they are to negotiate their ways through job, family, and the marketplace. As we saw, vernaculars and code-switching speech forms make for a complex language scene. And despite the equilibrium at 100%-English, virtually no Ghanaians living in Ghana have brought up children who are ignorant of at least one Ghanaian vernacular. The costs of maintaining the vernaculars (at least as languages for family and personal matters), are low; and the risks of abandoning them, given the possibility that order could break down in the cities and the people go back into ancestral villages for protection, fairly high.

From the point of view of transaction cost analysis, this solution to the Tower of Babel curse does not look too bad. Ghanaians must know two languages (English and their own vernaculars) in order to communicate with most local residents and all formally educated citizens in the country. But English also gives them access to virtually all important international communication networks, so English helps kill two birds with one stone.

From the point of view of the creation of a national culture, however, this solution has a serious deficiency. Since language is often considered an essential element of a national culture, reliance on English for intracountry communication gives Ghanaians the sense that while other countries (Sweden, Netherlands, Spain, etc.) have national cultures, they lack one. An internal (to the state) solution to the inefficiencies of a multilingual society would, in the eyes of many articulate nationalists, be preferable to the status quo.

Work on the optimum size of firms suggests there may be an economic as well as a cultural logic supporting the investment in indigenous national languages (Williamson 1986). First, organizational decentralization permits greater levels of experimentation and competition. And a world of languagespecific countries allows for a greater degree of decentralized decision making than would be the case if international transaction costs were equal to intranational transactions costs. Second, for a country making an investment in national education, languagespecific curricula lower the probability of "brain drain," as it encourages highly trained personnel to stay in their country to work, so that their children can get a firm grounding in the parents' language. Third, a reputation for having a legitimate national culture with a loyal workforce is also useful in attracting international investment. 17 To be sure, there are other ways than language specialization to establish a loyal "brand-name" national culture (Switzerland and the United States have succeeded here), but language specialization is an extremely powerful method to differentiate a national population from foreigners.

The Lottery: A Standard Microeconomic Solution to Coordination Dilemmas

However rational for the country, coordinating on an indigenous official language is irrational for the individual. First, as the tipping model suggests, it is irrational for any Ghanaian parent to seek education for a child in an indigenous language unless a great majority of other parents already had done so. A second reason is that even if the government did choose an indigenous official language, speakers of the nonchosen languages would have a dominant strategy to subvert the policy, and speakers of the

TABLE 1

The Akan-Dagbani Coordination Game with Akan as the Chosen National Language

DAGBANI PARENT'S CHOICE AS MEDIUM OF INSTRUCTION	AKAN PARENT'S CHOICE AS MEDIUM OF INSTRUCTION FOR CHILD			
FOR CHILD	ENGLISH	AKAN		
English Akan	5, 5 1, 4	4, 2 3, 8		

chosen language, knowing this fact, would follow suit. Table 1 illustrates why this is the case.

Table 1 (simplifying the situation by assuming only two competing indigenous languages) portrays the strategic situation for an Akan and a Dagbani parent in the face of an announcement of a standardized Akan as Ghana's official language and the medium of instruction in all public schools. This program presents a dilemma for Ghanaians. Should they enroll their children in government schools or pay a higher tuition and enroll their children in one of the private, English-medium schools that begin to appear in every town? How should they calculate their payoffs for each choice? If all send their children to the government schools, the route toward cultural independence will have been paved. But if all send their children to English-medium private schools, the government program (however popular in the abstract) would be subverted. But if any parent sends children to the government school while most others send their children to private school, that parent will have lost the game of coordination.

For Table 1, let us assume a utility of 5 if both send their children to a private, English-medium school. If they do not coordinate, the one who learns English gets a 4, while the one who gets educated in one's own indigenous language gets a 2, and the one who learns in a foreign indigenous language gets a 1. With Akan being the chosen language, we give an 8 for the Akan in the "Akan-Akan" cell. Let us say that for the Dagbani parent, education in Akan, under conditions of coordination, would yield 3, which is higher than noncoordination but lower than coordination in English. Under these circumstances the Dagbani player would have a dominant strategy to choose English, and the Akan player would rationally follow and choose English. The unique equilibrium is therefore "English × English." Given this situation, it would be impossible for a government to get noncoerced compliance for the promotion of any indigenous official language.

For coordination problems of this sort, game theorists have devised lotteries. For example, in the battle of the sexes, each player would prefer a coordinated coin flip to avoid the possibility of an uncoordinated outcome. Because game theorists rely on lotteries to solve coordination dilemmas, it is useful to think how such a device would work to solve the Tower of Babel

TABLE 2 The Akan-Dagbani Coordination Game under Conditions of a Lottery for National Language Choice

DAGBANI PARENT'S RESPONSE TO LANGUAGE LOTTERY	AKAN PARENT'S RESPONSE TO LANGUAGE LOTTERY			
	REJECT	ACCEPT		
Reject	5, 5	5, 5		
Accept	5, 5	(1 - p)y + p3, px + (1 - p)3		

Note: p = probability of Akan's winning lottery; 1 - p = probability of Dagbani's winning the lottery; x = utility to Akan speakers if Akan won the lottery; y = utility to Dagbani speakers if Dagbani won the lottery.

curse. Consider Table 2, where the Dagbani and the Akan parent are each asked to enter a language lottery in which a single Ghanaian language would be chosen as official. (In this case, the government would put chits into an urn, each labeled with a language, in proportion to the percentage of the population, and then choose a chit blindfolded.) Players can either accept or reject the lottery. If either rejects, the status quo of all-English is retained, in which case each player receives a score of 5.

The probability (p) that Akan will be chosen, given the higher percentage of Akan speakers, is greater than the probability (1 - p) that Dagbani will be chosen. However, the utility if Dagbani is chosen for a Dagbani (y), given the low percentage of Dagbani native speakers and the large employment opportunities available for those who have facility in the official language of state, is greater than the utility for an Akan (x) if Akan is the lottery winner. Assume a score of 3 for the lottery loser, which is lower than coordination in English, but higher than a failure at coordination. The expected utility for an Akan for playing the lottery is px + (1 - p)3. The expected utility for a Dagbani for playing the lottery is (1 - p)y+ p3. The lottery would be a rational outcome if both expressions yielded payoffs greater than 5. (If p = .7, x = 6, and y = 10, for example, players should rationally enter the lottery.)¹⁸

The problem with this lottery solution is that it assumes lottery players will respect its results. But how can either player credibly commit to accepting the lottery outcome? Why should any player believe that the loser would not subvert the lottery outcome after its announcement? Even if parents committed their children to matriculation in a government school as a precondition for playing the lottery, these parents could send future children to English language schools and subvert the lottery outcome in the long run. While the lottery principle is useful to unhook us from seeing the status quo as inevitable, it does not provide a compelling answer to the problem of coordination on an indigenous language in multilingual countries.

The 3 ± 1 Language Repertoire as a Politically Induced Equilibrium

Perhaps the dream of indigenous language promotion is quixotic? The logic of the tipping game, the probable backfire of economic incentives aimed to induce language shift, and the difficulty of assuring commitments in a language lottery all suggest the power of the status quo. Ghanaians, it seems, will continue to rely upon English for administrative, technical, and international communication, while using their vernacular languages for communication in informal domains with other Ghanaians who share that language.

Yet the models so far presented all assumed that there was a binary choice between English and some indigenous language. None of the models examined the strategic possibilities of multilingualism. For most of Africa, unlike America, multilingualism is the norm (Kashoki 1977). The de facto solution to the Tower of Babel curse has been the investment in multilingual repertoires. The final question that I shall address is whether a Ghanaian government could, in the context of a multilingual policy, promote a single indigenous language as official. 19

Indeed, respondents favored a multilingual context and were receptive to policies that promoted different languages in different domains of use. To be sure, they did not want a form of multilingualism where there would not be a common means of communication. When asked if they preferred for each group in Ghana to promote its own native language or whether Ghanaians should agree on a single Ghanaian language as the major language of official business, a majority (59% to 39%) chose the latter. But vernacular development, along with English as a lingua franca, was highly favored. When asked whether they preferred for English to become the dominant language in all walks of life or for English and the indigenous languages to share importance, a significant majority (89% to 9%) chose the latter. These respondents had a clear notion of how language domains should be ordered. For district council political administration, 72% favored the vernaculars as the official medium, where only 28% chose English. For medium of instruction in primary school, however, 87% favored English, while 10% favored the vernacular. While the political yearning for a unique national language could not be realized in the framework of a one-language state, perhaps it could be realized within a multilingual framework?

In light of these preferences, a plausible multilingual model for Ghana is that of India (Laitin 1989). Throughout postcolonial India, English remains the principal language of education, business, and political administration. However, the all-union (i.e., federal) government promotes Hindi, and this language is important for interstate official communications and popular culture (films, music, television). The state governments each promote state languages, and these languages are media of instruction in primary education and are used extensively in state-level

administration. Citizens need to know English, their state language, and Hindi to move in a variety of social, political, cultural, and economic circles. Those whose states rely on Hindi need to learn only two languages; and those who speak protected minority languages within states that have a different language as official need to learn four. This outcome can thus be called a 3 ± 1 formula.

Ghana, as we have seen, does not have a language such as Hindi to serve as a credible national language. Countries such as Indonesia (with Bahasa) or Kenya (with Swahili) are more obviously able to follow the Indian model. Pidgin might serve that role for a number of West African states but not (due to its limited use and extremely low prestige) in Ghana. Ghanaian, however, if given recognition by linguists, authors, and national politicians, might play such a role.

If Ghanaian is not seen as threatening to any group—with district leaders getting rents from the promotion of the regional vernaculars (by appointing officials literate in the vernacular giving vernacular speakers chances to provide translation services, and by advertising tenders in that language) and parents getting education in English for their children at government schools—the seeds might get planted for an indigenous official language. If Ghanaian were recognized, in a context in which district languages had important roles and where English remained crucial in technical and administrative domains, the macro outcome would be called a 3 + 1 formula (not 3 ± 1 , since no district language would be Ghanaian). To be sure, there is now no literature, no texts, and no name for this language; Ghanaian could not be accepted as a substitute for English in a binary choice atmosphere. It would have to emerge out of a long succession of micro choices.

Ghanaian would displace neither English nor the district languages in the short term but would be an outlet for artists, novelists, television scriptwriters, and translators, all of whom could be subsidized for their literary production. If Ghanaian is really the language of the growing urban popular classes, its chances of evolving into a national symbol are greater. It would need its Dante, or Webster, or perhaps Shakespeare for ultimate legitimation; but the government could set the stage for the emergence of such a figure.²⁰

This 3 + 1 equilibrium would give all Ghanaians the ability to communicate efficiently (without translators) with each other, and Ghanaians would study English sufficiently in school so that they could communicate internationally as well. The education costs for this equilibrium would be higher than the equilibrium at 100%-English, but there would be gains in human dignity (people using their own language in high-status domains) and in the creation of a national culture, which Ghanaians say they want, even though coordination dilemmas have up until now forestalled any effort to envisage how that culture could be created.

The Electoral Basis for a 3 + 1 Outcome

The 3 + 1 outcome could not result from a free market of language investments, without government interference. Even if it could be demonstrated that it was a Pareto superior outcome to the status quo (something that has not been demonstrated here), there would still need to be a government with the will and motivation to act. What evidence is there that such political engineering is likely, given the high incentive for any government to avoid the risk of intergroup antagonism by keeping the language issue off the political agenda?

In fact, the data in our survey point to the risks of such a program rather than its likelihood. Even those respondents whose children were disadvantaged by the all-English curriculum (in that children whose parents already spoke English had an easier time learning in school) wanted English as the medium of instruction in their children's school. They were willing to allow their vernaculars to be used in private domains, so that the strong respondent support of English along with vernaculars over the sole use of English reflects a desire not for equality between languages but rather for the symbolic recognition of the vernaculars. And most people fear the consequences of the arbitrary choice of one vernacular as the sole official language of state. English plays a role in Ghana similar to that of foreign kings invited to rule over small European states in the nineteenth century: it assures neutrality.

Nonetheless, support for an indigenous national language is a natural element in a populist opposition program. Populism can exploit a general feeling throughout a population that the only people who are profiting from the government are the bureaucrats and executives in multinational corporations and international agencies, all of them distinguished by their excellent command of English. Under these conditions, a populist contender might find that a program of cultural nationalism can capture the imagination of disgruntled voters. The dominant message would be a return to cultural and linguistic roots of Ghana. To be sure, populism in Africa for the most part has been associated with the political movements that celebrate the separate identity of particular language groups. Would the politicization of language issues in Africa promote just this sort of populism where first provinces and then districts would seek national sovereignty, leading in the limit to the absurdity of each idiolect's having its own country? Maybe so. However, there may exist populist strategies supporting the promotion of indigenous languages that are not divisive.

Suppose a populist challenger wins a national election in part on a platform of cultural independence and the need for a Ghanaian national language. Ghanaians might well vote for such a program (just as many Americans supported prohibition) with the full intention of subverting it privately. Such subversion would assure an English-medium education for their own children while all other children are

receiving a curriculum with an indigenous language playing a central role. Their own children would then be well equipped to land the best white-collar jobs in Ghana. The cumulative effect of such reasoning—which might be called the private subversion of a public good—would be the failure of the national program.

To implement such a program without mass subversion, the populist leader would need to assure district leaders that their languages would not be undermined by the promotion of a single indigenous language and to assure parents that English would remain central to the curriculum. One possible plan would be for the new leader initially to introduce a curriculum in the state schools that included English and district vernaculars as dual media of instruction. Those parents (constituting 89% of our respondents) who preferred English along with the vernacular over English alone would not likely withdraw their children from state schools as long as English remained central to the curriculum. Furthermore, district leaders would be pleased to implement the program if only because the promotion of district vernaculars would give local leaders patronage power (e.g., in purchasing text books) and the ability to keep local administration in the hands of the people who speak the district vernacular.

Meanwhile, the national leader could begin supporting the development of pedagogical materials in a national language, let us say Ghanaian. (This includes more than textbook production. National programs to promote a literature in Ghanaian would need to be implemented, say, by subsidizing books and newspapers that use it.) After a period in which the vernaculars had been institutionalized in district schools and materials developed in Ghanaian, the leader would be in a position to introduce the proposed national language into the curriculum. District leaders, who may have fought such an introduction in schools that relied only on English and fought the simultaneous introduction of a national language and vernaculars in fear that the latter would dominate the former, would be less inclined to fight against the introduction of Ghanaian when the schools were already promoting district languages. Parents who feared the development of district languages without a national language (59% of our respondents) would find this development a healthy one, as long as English remained dominant as a medium of instruction. And since Ghanaian represents no particular subgroup in Ghana, the communal animosities that appeared when we asked respondents about Akan or Ewe as a national language would be side-stepped. Few parents would have an incentive to take their children out of the state school system.

The long-term result of this strategic introduction of languages into the national educational curriculum could be a universally understood indigenous Ghanaian national language, one that was introduced without coercion, without intercommunal conflict, and without mass subversion of public institutions.

Much of my thrust here has been to demonstrate

the difficulty of attaining what may be a Pareto superior outcome for countries such as Ghana, where there is a strong popular desire for the implementation of a national language but an inability to do so, in large part due to the difficulty of reaching a consensus as to which of many vernaculars could serve as a national language. Even if the national language does not (at first) have high status, part of the solution must be in the choice of a neutral national language, largely because the use of a lottery to choose an already-established vernacular as the national language will break down. As a populist leader develops a neutral language as the national language, that leader can strategically work for its acceptance. The political program I have illustrated shows that the inchoate desires of many Ghanaians for an indigenous national language are not hopeless.

CONCLUSION

The curse of the Tower of Babel afflicts African states in general, and Ghana in particular, with great force. I have sought to think systematically about a solution that improves upon the market-induced outcome, which, from the point of view of many Ghanaians, is inefficient. I have assumed (reasonably, as the surveys show) that Ghanaians themselves would prefer coordination on a language from within the country over one of a previous colonial regime. I have shown the underlying reasons why coordination is such a problem. By doing so, I was able to suggest alternative strategies for reaching a coordination point preferable to the one that the status quo now offers. This is the 3 + 1 language outcome, in which an indigenous national language gets promoted and institutionalized, along with district vernaculars—all in the context of continued reliance in many domains on the language of former colonial rule. This outcome would be favored over the status quo by many people in countries like Ghana, where there is societal multilingualism and a coordination problem for choosing a national language. I have provided a strategic analysis of how coordination problems can be overcome to attain a 3 + 1 outcome. However, I have not here offered a definitive solution to the curse of Babel but rather provide an analytical representation of the problem that makes it more susceptible to solution.

Notes

This paper was originally prepared for the annual meeting of the American Political Science Association, Washington, 1991. The Spencer Foundation financially supported the fieldwork for the empirical research reported herein. Edward Mensah is my collaborator in the larger project concerning language in Ghana, of which this paper is a part. The survey project in Ghana was managed by A. Afrifa and Pwasanga Ataogye, and their expertise in psychology added depth to our interviews. James Fearon provided invaluable technical and theoretical support. Ronald Rogowski and Duncan Snidal

commented usefully on earlier versions. Eric Budd and Stathis Kalyvas provided research assistance.

1. It is unfortunate for me that Brams did not consider the Tower of Babel in his stimulating *Biblical Games* (1980). On issues of coordinating on an official language, Pool's models (1991) have been pioneering.

A lingua franca is a language of a certain group appropriated for intergroup communication. A pidgin is a mixed language that is not native to any group and is not passed on

intergenerationally as a first language.

- 3. Transaction cost concerns not only help us interpret the behavior of state rulers but also help explain language investments of individuals. In situations where there are pockets of monolingualism in a language different from the language of rule, certain individuals, by investing in an expanded language repertoire, have reduced transaction costs for their own affairs and have taken advantage of transactions barriers that others face by providing useful translation services (De Swaan 1988).
- 4. For the reasons why twentieth-century state building provides greater difficulties for rationalization, see Laitin 1992, 38–45. For the set of multilingual African countries without a common indigenous lingua franca, see Laitin 1992, 121–32.

5. Interview conducted by David Laitin with Kwamena Ahwoi, Provisional National Defense Council secretary for

local government, Accra, 29 October 1990.

- 6. For purposes of this study, I commissioned K. K. A. Anti to undertake a survey of 22 schools at all levels in Cape Coast, a city that often prides itself on having the most modern educational system in Ghana and one that they feel should serve as a model for all of Ghana. The wide variety of elite Ghanaians who send their children to boarding school there attests to the national acceptance of their claims.
- 7. Pidgin in West Africa is heavily influenced by English, but relies on indigenous language structures and vocabulary as well. It is socially perceived as an indigenous speech form.
- 8. Respondent numbers are indicated in parenthesis. The letter refers to the initial of the sample group's vernacular, to be identified later on in the text.
- 9. Code switching, according to Haugen is "the alternate use of two languages, including everything from the introduction of a single, unassimilated word up to a complete sentence or more into the context of another language" (1978, 21).
- 10. Schelling's style is to present interesting-looking payoff curves and then match the pictures with actual situations, even if "there is no guarantee that a pair of real choices exists somewhere that corresponds to some pair of curves that we might adopt on heuristic or architectural grounds" (1978, 219, 234). My approach is to draw the payoff curves consistent with my sense of the subjective utility of actors in a specific situation, even if the curves lack Schelling's cleanness. Two aspects of the curves in Figure 1 have interesting features: (1) the tipping point k is closer to 100%I than 100%E; and (2) in the movement toward the 100%I equilibrium, the declining marginal return for English bottoms out, and the value of E begins to rise as the society moves toward 100%I. Neither directly affects my argument here; I include these features in the figure as a reminder that the game is more complex than what can be portrayed here.
- 11. That is, the expected utility for speaker i is the sum for all members of k of the utility of each language if it were chosen as official multiplied by the probability of its being chosen. For Figure 1 to be empirically based, we would need data on expected utilities for all individuals at each point on the horizontal axis.
- 12. Dorian (1981) has developed a "tipping model" showing how as the number of domains (e.g., work, family, recreational reading, poetry) in which a language is used decreases, a dynamic leading to "language death" will likely be set in motion. Among the Ghanaian respondents to the survey recounted here, the tenacity concerning one's indigenous language (especially in the domains of family and community life) makes the "tip" toward death highly un-

likely. More than feelings of local pride are at stake. Spouse selection, ability to cement support networks in cosmopolitan centers, and the opportunity to get a chief's title in one's hometown are all in part contingent on maintaining facility in one's home language. The observation that local languages and dialects can serve well-demarcated needs over generations, without facing death, has been made by Fernandez (1986).

13. Another theoretical tack is to introduce sequencing, building on the assumption that certain individuals have higher-than-average payoff for I or lower-than-average payoff for E at some point to the left of E. Any individual who, by virtue of "switching," pushes the game closer to E could "trigger" the move of a second individual, whose payoff for E now is higher than that for E. A version of a sequencing model will be presented in the final section.

14. I use the word framing somewhat more broadly than do Tversky and Kahneman: They require that the alternatives have the same expected utility. Some of the respondents in the survey reconfigured the utility for certain options based on factors external to the situation presented to them. For this reason, my use of the frame concept is closer to Goffman's

than to Tversky and Kahneman's.

- 15. I do not here address the issue that while side payments may equalize costs, the provision of government sinecures as a side payment may have the effect of reducing significantly the gross national product and thereby reducing the average payoff for 100% I. Nor do I address the issue of the administration of side payments that may take on characteristics of welfare bureaucracies and thereby enrage the population. The specter of bureaucratic-linguistic tyranny could raise the subjective utility of the status quo at 100% E.
- 16. Rarely did these emotions surface until interviewers got to the question, at the end of the interview, about the tax proposal. But one exception was a Dagbani respondent who, in response to the question asking for a suggestion as to which language might serve as an indigenous national language, responded in anger. "For a long time now, the Akans think that we northerners are less human. If we adopt their language, our position is going to worsen" (D30).

17. This notion is by analogy from David Kreps's (1990) arguments providing a rationale for the investment in distinct

corporate cultures.

18. If utility is modeled as a linear function of group size, it would be possible to predict whether any group would rationally enter the lottery. The slope of the function would determine whether the risk were superior to the status quo for each group. Some functions would favor groups on the extremes (very large and small populations); other would favor groups that had population percentages closer to the median group.

19. In Laitin 1992, the model that follows is formalized and applied to a large set of postcolonial states.

20. Weinstein (1979) refers to such types as "language strategists."

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CIVIC CULTURE AND DEMOCRACY: THE QUESTION OF CAUSAL RELATIONSHIPS

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causal model of relationships between structural properties of states, civic culture attitudes of the general public, and change in level of democracy is tested with cross-national data. The model permits inferences about the possibility of unidirectional or reciprocal causation between civic culture attitudes and democracy, controlling for macrosocietal variables such as economic development, income inequality, and subcultural pluralism. Most civic culture attitudes do not have any significant impact on change in democracy. One of them, interpersonal trust, appears clearly to be an effect rather than a cause of democracy. The exception is the percentage of the general public that prefers gradual reform of society instead of revolutionary change or intransigent defense of the status quo. Support for gradual reform has a positive impact on change in democracy, and it is unrelated to a country's years of continuous democracy—findings that support the hypothesis of a unidirectional civic culture effect on democracy.

n important unresolved question in the study of democratization is whether attitudes of the general public have a major causal effect on the establishment and stability of democratic regimes. The theory of civic culture proposed by Almond and Verba (1963) and further elaborated by Inglehart (1988, 1990) postulates that the viability of democratic institutions is affected powerfully by attitudes such as belief in one's ability to influence political decisions, feelings of positive affect for the political system, and the belief that other citizens are basically trustworthy. Countries with high levels of these civic culture attitudes are expected to be more likely to adopt and sustain democracy over time than countries with low levels, regardless of socioeconomic factors such as level of economic development. An alternative possibility is that civic culture attitudes are an effect rather than a cause of democracy. According to this line of argument, democracy typically is established for reasons other than civic culture attitudes of the general public, and the successful persistence of democracy over time is likely to cause increases in levels of civic culture attitudes because high levels of subjective political competence, pride in the political system, and interpersonal trust are a rational, learned response to the experience of living in a country that has a stable democratic regime (e.g., see Barry 1978, 50-52; Schmitter and Karl 1991, 82-83).

The hypothesis that democracy causes civic culture would seem a priori to be as plausible as the hypothesis that civic culture causes democracy. If so, causal models of the relationship between civic culture attitudes and democracy should be formulated to take into account the possibility of reciprocal causation between these variables. Until recently, it has been difficult to assess the relevance of attitudes of the general public for the establishment of stable democratic regimes because of lack of sufficient cross-

national data on attitudes. The pioneering study in this regard was conducted by Inglehart (1988, 1990), who was the first to compile data on attitudes of the general public for a sample of countries that was large enough to permit multivariate statistical analysis of the relative influence on democratization of micro political culture attitudes as compared with macro socioeconomic factors. Inglehart's causal model is grounded, however, in the assumption of unidirectional causation—that civic culture has an effect on democracy and that democracy does not have an effect on civic culture.

Inglehart's model consists of four variables: (1) a country's level of economic development in 1950, as measured by its gross national product (GNP) per capita; (2) the percentage of the labor force employed in the tertiary sector (no date given), which is interpreted as an indicator of the size of the middle class; (3) a composite measure of civic culture over 1981–86 that reflects an average for the general public of its level of life satisfaction, interpersonal trust, and lack of support for revolutionary change; and (4) a country's years of continuous democracy from 1900 to 1986. The model stipulates that economic development is a predetermined variable (which may have causal effects on the other three variables) and that the percentage of the labor force in service occupations (the tertiary sector) and the average level of civic culture are intervening or mediating variables, caused by variation in level of economic development, which, in turn, are causes of variation in a country's years of democracy. Estimates of the effects of the predetermined and intervening variables on democracy showed that economic development had no significant direct effect, that the labor force in services variable had a direct effect of +.36, and that the civic culture variable had a direct effect of +.74 (Inglehart 1990, 44). These results led Inglehart to

infer "that over half of the variance in the persistence of democratic institutions can be attributed to the effects of political culture alone" (p. 46). And he concluded more generally that this evidence "tends to confirm the basic thesis of *The Civic Culture*" (p. 48).

If Inglehart's causal inferences are valid, explanations of democratization that emphasize political culture attitudes must be given primacy over explanations that emphasize the importance of macro socioeconomic conditions. The problem is that the possibility of an effect of years of continuous democracy on civic culture attitudes is ignored. A proponent of the alternative hypothesis that democracy causes civic culture attitudes could reasonably argue that the supposed "effect" of civic culture on democracy is really an effect of democracy on civic culture. Indeed, because of the fact that a country's years of continuous democracy are calculated from 1900 up to the time of the measurement of the civic culture attitudes during 1981-86, this means that the temporal status of the years of continuous democracy variable is prior to the civic culture measurements. It would thus be more straightforward and plausible to interpret a strong association between civic culture 1981-86 and years of democracy 1900-1986 as reflecting the possibility of a strong causal effect of the persistence of democratic institutions on a country's level of civic culture.

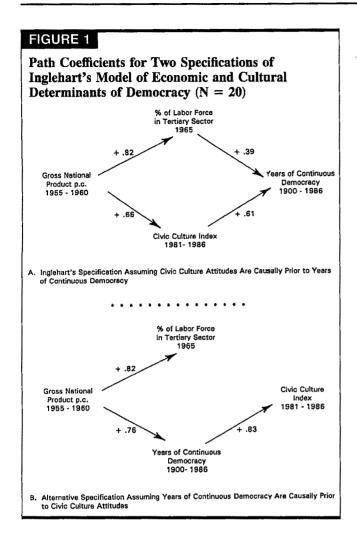
A second problem with Inglehart's analysis involves the conceptualization of civic culture, defined operationally by a composite index of life satisfaction, interpersonal trust, and lack of support for revolutionary change. Since life satisfaction is not a component of the civic culture concept originally proposed by Almond and Verba and since Inglehart does not provide a theoretical justification for why life satisfaction should be assumed to be part of a unidimensional syndrome of civic culture attitudes, one could argue that life satisfaction should be analyzed separately from interpersonal trust and lack of support for revolutionary change. Moreover, since these latter variables also are conceptually distinct and cannot be assumed on definitional or empirical grounds to be unidimensional, it would be useful to analyze them separately, too. Therefore, the more appropriate measurement strategy would be to "unpack" the composite index of civic culture and test hypotheses using each of the attitudinal variables

A potential third problem is that Inglehart's model may be subject to specification error. Inglehart assumes that the percentage of the labor force in service occupations is the only relevant macrosocietal determinant of democracy apart from level of economic development. Given the fact that the *labor force in services* variable has never been included previously in cross-national macro research on determinants of democracy, this strong assumption seems dubious. Omitted variables for which a compelling case can be made theoretically, following the analysis of Dahl (1971), include income inequality and subcultural

pluralism. Empirically, research focusing specifically on the stability of democracy over time has found that the extent to which income is distributed unequally is an extremely powerful negative influence that mediates the positive effect of economic development (Muller 1988). And research on determinants of crossnational variation in level of democracy has found that subcultural pluralism is one of the relevant noneconomic influences (Bollen and Jackman 1985). Income inequality and subcultural pluralism may thus be relevant omitted causes that could affect the estimates of causal influence in Inglehart's model.

Although we are critical of Inglehart's model specification and measurement procedures, we share his concern with assessing the relevance of attitudes of the general public for the establishment of stable democratic regimes. The step that needs to be taken now is to formulate a research design that enables one to address more effectively the question whether unidirectional or reciprocal causal relationships may obtain cross-nationally between civic culture attitudes and democracy, controlling for the influence of macrosocietal determinants. We shall test an alternative causal model in which the dependent variable is change in a country's average level of democracy from the 1972–80 period to the 1981–90 period. If civic culture attitudes have significant effects on change in democracy, such that high levels of civic culture attitudes are associated with increases in democracy and low levels are associated with decreases in democracy, then at least the possibility of civic culture causing democracy will be supported. By contrast, if civic culture attitudes do not have significant effects on change in democracy, then one can be skeptical about claims that civic culture attitudes are an important cause either of transitions from authoritarian rule to democracy or of the persistence of democracy over time. The possibility of a causal effect of democracy on civic culture attitudes will be taken into account by including a country's long-term experience of democracy in the model as a predetermined variable that could have an influence on civic culture attitudes independent of other macro socioeconomic conditions.

We begin our analysis by testing for causal relationships between civic culture attitudes and democracy using Inglehart's sample of primarily European (or European-heritage) countries. Then we test an alternative causal model with an expansion of Inglehart's data set. We have collected measurements of system affect (as inferred from attitudes about societal change) and interpersonal trust (elements of civic culture common to the conceptualizations of both Almond and Verba and Inglehart) from representative surveys conducted during 1990-91 of urban populations in the six Central American states: Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama (see Appendix B). These data are added to the measurements used by Inglehart from surveys conducted during 1981–86 (see Appendix A).



CAUSAL MODELS OF ECONOMIC AND CULTURAL DETERMINANTS OF DEMOCRATIZATION

Estimates of causal path coefficients for versions of Inglehart's model of economic and cultural determinants of democracy are shown in Figure 1. The measure of civic culture is a composite index of life satisfaction, interpersonal trust, and opposition to revolutionary change constructed for 20 countries with information on all three variables.2 The results obtained for model A closely reproduce those reported by Inglehart (1990, fig. 1-6).3 In this model, the index of civic culture measured with data from 1981 to 1986 is assumed to have a unidirectional effect on a country's years of continuous democracy since 1900. Given that assumption, the path coefficients indicate that civic culture attitudes are by far the most important determinant of years of continuous democracy, that the percentage of the labor force in service occupations (the tertiary sector) has a much smaller direct effect on years of continuous democracy, and that the influence of level of economic development is entirely mediated by civic culture and the percentage of the labor force in services.

Model B shows the results when the temporal

ordering of civic culture and years of democracy is corrected so that years of continuous democracy from 1900 to 1986 precedes civic culture attitudes observed for 1981 to 1986. This model shows strong support for the alternative hypothesis that the experience of stable democracy produces high levels of civic culture. When the civic culture index is regressed on years of continuous democracy, percentage of the labor force in services, and economic development, only years of continuous democracy has a statistically significant effect. 4 Moreover, when years of continuous democracy is regressed on labor force in services and economic development, only GNP per capita has a significant effect. Model B thus corresponds to a very different pattern of causal inference than model A. From his estimates for model A, Inglehart concluded that "economic development per se does not necessarily lead to democracy. Only insofar as it brings appropriate changes in social structure and political culture does it enhance the viability of democratic institutions" (1990, 44). By contrast, if one makes a single change in the causal ordering of the variables, a change that reflects more correctly the temporal ordering of the measurements, one then finds that economic development enhances the viability of democratic institutions directly and that social structure is irrelevant. Moreover, the effect of economic development on civic culture attitudes becomes indirect, transmitted through the influence of economic development on producing stable democracy.

As they stand, we believe that model B is more defensible than model A, but neither model addresses the possibility of a causal effect of civic culture on change in level of democracy—the question that is at the heart of the civic culture thesis. If civic culture attitudes of the general public are a relevant determinant of democratization, then high levels of civic culture should produce increases in a country's level of democracy and low levels of civic culture should inhibit the development of democracy. In other words, civic culture attitudes should have a positive effect on change in level of democracy over time.

Currently, the most widely used indicators of the extent to which a country's political system is democratic or repressive are the ratings of political rights and civil liberties on seven-point scales developed by Raymond D. Gastil for Freedom House and reported annually since 1972. The political rights and civil liberties scales are based on check-lists that cover 11 and 14 different criteria, respectively (see Gastil 1991); and they appear to have good reliability as suggested by very high correlations between them and other conceptually similar measures available for particular years only (cf. Bollen 1986, 588; Banks 1986; Coppedge and Reinicke 1991). We use the average political rights and civil liberties ranking, converted to a scale of 0-100, as an index of a country's level of democracy in a given year and then compute a mean level of democracy score over two intervals, 1972–80 and 1981-90 (see Appendix A).

The regression equations reported in Table 1 test for the possibility of an effect of civic culture attitudes

TABLE 1										
Effects of (Civic Cultu	re Attitude	es and	Macros	societal	Variables:	Europ	ean-orie	nted S	ample

	EQUATIONS EXPLAINING LEVEL OF DEMOCRACY,					1981-90
INDEPENDENT VARIABLES	1.1	1.2	1.3	1.4	1.5ª	1.6
Intercept	37.98	64.98	30.22	95.30	69.06	58.62
Level of democracy, 1972-80	+.60* (.17)	+.75* (.10)	+.69* (.11)	+.16 (.18)	+.34* (.17)	+.52 (.20
Civic culture index, 1981–86 ^b	+.05 (.54)				***************************************	
Level of life satisfaction, 1981–86	ADDINO	-5.16 (3.34)				
% interpersonal trust, 1981–86	weaterasts		+.06 (.23)			_
% support revolutionary change, 1981–86		_		-2.45* (.73)	68 (.88)	
ln% support revolutionary change, 1981-86		_				6.50 (7.01
Adjusted R ² Number of cases	.58 20	.74 24	.71 23	.76 21	.54 20	.63 21
-	1.7	1.8°	1.9	1.10°	1.11	1.12
Intercept	35.44	38.09	-11.15	-34.19	-22.06	113.52
Level of democracy, 1972–80	+.76* (.13)	+.80* (.14)	+.68* (.10)	+.64* (.10)	+.84* (.25)	+.21 (.11)
% defend society against subversives, 1981-86	38 (.31)	69 (.44)		H	 -	
% support gradual reform, 1981–86	*******		+.64* (.28)	+.99* (.33)	+.61* (.29)	+.58 (.17
Years of continuous democracy, 1900-1986					033 (.17)	******
% labor force in tertiary sector, 1965	********	-			+.20 (.56)	
Gross national product per capita, 1955-60	***************************************	_		•	0068 (.0072)	
Top 20% income share, 1970–80	*****	_				1.79 (.37
Ethnolinguistic fractionalization, 1960–65	- COLUMN TO THE	_				15 (.06
Adjusted R ² Number of Cases	.64 21	.65 20	.70 21	.74 20	.67 21	.90 19

Note: Entries are unstandardized regression coefficients (standard errors in parentheses).

on change in democracy from the 1972–80 interval to the 1981–90 interval. The dependent variable is a country's mean annual level of democracy during 1981–90; and since a country's mean annual level of democracy during 1972–80 is included in the equation, the effects of the civic culture variables are effects on change in democracy, that is, the level of current democracy *independent* of its previous level.

The equations in the upper panel of Table 1 test for

effects on change in democracy of the composite civic culture index and each of its components analyzed separately.⁶ Equation 1.1 shows that the composite index of civic culture has no significant effect on change in level of democracy. (Indeed, the coefficient on the composite index is smaller than its standard error.) Thus, if one uses only the composite index of civic culture, the causal inference of model B is supported: democracy has a strong positive effect on

^a South Africa excluded.

^b Mean of life satisfaction, interpersonal trust, and opposition to revolutionary change.

^c Norway excluded. *p ≤ .05, one-tailed.

the development of civic culture attitudes and civic culture attitudes have no effect on democracy.

When the civic culture index is unpacked, however, one finds that the components have different effects on change in democracy. Equation 1.2 shows that life satisfaction, which is conceptually distinct from Almond and Verba's notion of civic culture, has a negative effect on change in democracy. But the coefficient on life satisfaction is not sufficiently larger than its standard error to be statistically significant at the .05 level for a two-tailed test (or a for a one-tailed test), so we cannot reject the null hypothesis of no effect. Equation 1.3 shows that interpersonal trust has no significant effect on change in democracy. By contrast, Equation 1.4 shows that the percentage of the general public in favor of revolutionary change does have a significant negative effect (the expected direction) on change in democracy.

Inspection of the distribution of scores on support for revolutionary change in Inglehart's Europeanoriented sample reveals the presence of an extreme outlier: South Africa (see Table A-1). Support for revolutionary change in this country is at the 25% level, almost twice as great as the other relatively high scores of 14%, 13%, and 12% for Portugal, Argentina, and Mexico, respectively. South Africa also has the lowest levels of democracy for both intervals and the most negative change, so it is obviously a potentially influential case. Equation 1.5 shows that when South Africa is deleted, the coefficient on support for revolutionary change is radically reduced in size to a nonsignificant value that is smaller than its standard error. However, since there is neither a good theoretical reason for deleting South Africa nor any reason to suspect that the score of 25% is due to measurement error, deletion of South Africa is arbitrary. A preferable remedy is to reduce the influence of South Africa's extreme score by logging the scores on the revolutionary change variable. Equation 1.6 shows that across all cases in the European-oriented sample, logged (base e) revolutionary change has no significant effect on change in democracy. So one may conclude that the significant negative effect of revolutionary change in equation 1.4 is an artifact of the extreme distance between South Africa's score and the other scores.7 Consequently, analysis of the components of the civic culture index only reinforces the inference that civic culture attitudes do not have a causal effect on democracy.

There is still one more avenue to explore, however, with the available cross-national data on civic culture attitudes. Support for revolutionary change is one of three options that respondents were asked to choose between in describing their attitude about social change (see Appendix A). The other choices were to defend the status quo against subversive forces or to improve society gradually by reforms. The problem with using support for revolutionary change as an indicator of a civic culture attitude is as follows. First, one has to ask what kind of civic culture attitude is being measured by support for revolutionary change. Although Inglehart (1988, 1990) does not address this

question, it is presumably system affect: support for revolutionary change implies a strong rejection of the existing political system and a preference for achieving societal change by the use of force and violence. Since violent rebellion has hardly ever resulted in the establishment of a democratic regime and since high levels of political violence are incompatible with stable democracy support for revolutionary change clearly is an attitude that is not supportive of democratic political institutions (Huntington 1984, 213-14; Powell 1982, 168-70). But lack of support for revolutionary change does not necessarily imply support for democratic institutions. Respondents who do not favor revolutionary change may prefer the option of defending their society against subversive forces. This kind of extremely conservative attitude about societal change implies support for repression of dissent, which could inhibit a country from establishing or maintaining a high level of democracy. Thus, the expected inverse relationship between support for revolutionary change and democratization will be weakened by the fact that people who oppose revolutionary change may not necessarily support democracy. Indeed, as can be seen from the distribution of scores on the defend society against subversives option (see Table A-1), sizable minorities of them endorse this conservative option; and this kind of attitude implies support for repressive measures by the state to suppress dissent, which would hinder democratization.

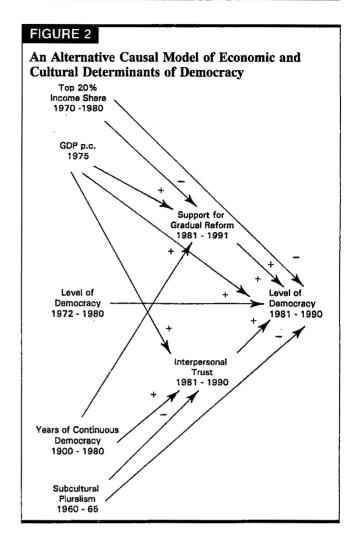
The *support* for gradual reform option is the attitude about societal change that in our view is most unambiguously supportive of democratic procedures. We assume that citizens who support democratic norms and institutions will prefer the gradual improvement of society by reforms over either revolutionary change or intransigent defense of the status quo. Therefore, we think that the system affect component of the civic culture concept is measured best by the percentage of the general public that supports gradual reform. A high level of support for gradual reform among the general public necessarily implies relatively low levels of support for the radical option of revolutionary change and the conservative option of defending society; a low level of support for gradual reform necessarily implies that either or both support for revolutionary change and intransigent defense of the status quo are relatively high. Therefore, we hypothesize that support for gradual reform will have a positive effect on change in democracy and that this positive effect will be much sharper than the negative effects of support for revolutionary change and defending the status quo.

The regression equations in the lower panel of Table 1 take the other attitudes about societal change into account. We focus first on the percentage of the general public that favors defending society against subversive forces. We expect that this attitude about societal change will have a negative but relatively weak effect on change in level of democracy. Equation 1.7 shows support for our hypothesis. Norway, however, is an extreme outlier (see Table A-1). An

unusually large proportion (49%) of the Norwegian general public favors defending society against subversive forces. This seems odd given the fact that only 16% of Swedes and 28% of Danes expressed support for the defend society option, so one might wonder if Norway's exceptionally high score is a result of measurement error. Therefore, equation 1.8 reestimates equation 1.7, with Norway excluded. Although the coefficient on defend society with Norway deleted almost doubles in size, it still is not significant for a one-tailed test (the t-ratio is -1.57). Thus, despite the possibility of measurement error in the Norwegian case, the negative relationship between support for intransigent defense of the status quo and change in democracy remains weak and nonsignificant.

Equations 1.9–12 include support for gradual reform as a determinant of change in level of democracy. In equation 1.9, the coefficient on support for gradual reform is positive and statistically significant, which supports our hypothesis. Although inspection of the distribution of scores on support for gradual reform does not reveal any extreme outliers (see Table A-1), Norway has the lowest score (49%). The relatively low level of support for gradual reform in Norway is due to the very large proportion of Norwegians who favor defending the status quo against subversive forces. If this is a reflection of measurement error, then inclusion of Norway may exert a downward bias on the regression coefficient for the support for gradual reform variable. Norway is therefore excluded in equation 1.10. As expected, the coefficient on gradual reform is larger (by approximately 50%) with Norway deleted, and its t-ratio is 3.0 instead of 2.29 with Norway included. Thus, exclusion of Norway favors our hypothesis even more strongly.

In equations 1.11 and 1.12, we introduce macrosocietal variables and retain Norway, since it would be arbitrary to exclude Norway on the suspicion of measurement error without having examined the actual wording in Norwegian of the question on attitude about societal change. Equation 1.11 shows that none of the macrosocietal variables in Inglehart's model has a significant effect on change in democracy, whereas the positive effect of support for gradual reform remains significant. Equation 1.12 includes income inequality (see Appendix B) and an indicator of the concept of subcultural pluralism, the index of ethnic and linguistic fractionalization (Taylor and Hudson 1971; tbl. 4.15, col. 3; original scores expressed as fractions between 0 and 1 are multiplied by 100). These two macrosocietal variables were omitted from Inglehart's model, but they have significant negative effects on change in democracy, as expected.⁸ The coefficient on support for gradual reform remains significant when income inequality and ethnolinguistic fractionalization are taken into account. The results of equations 1.11 and 1.12 thus show that the positive effect of support for gradual reform on change in democracy is not a spurious reflection of macrosocietal causes.



An Alternative Macro-Micro Causal Model

An alternative to Inglehart's causal model of economic and cultural determinants of democracy is shown in Figure 2. The important differences between Inglehart's model (Figure 1, model A) and ours are as follows. First, Inglehart's dependent variable (years of continuous democracy) is a predetermined independent variable in our model. This avoids the untenable assumption that civic culture attitudes measured in the 1980s cause previous years of continuous democracy. Our dependent variable is change in a country's average level of democracy from 1972-80 to 1981-90, that is, level of democracy during the 1980s independent of level of democracy during the 1970s. It is plausible to assume a priori that level of civic culture attitudes in the 1980s could be a cause of change in level of democracy from the 1970s to the 1980s. And the possibility of a reciprocal causal effect of experience of democracy on civic culture attitudes is incorporated into our model by inclusion of the years of continuous democracy variable as an antecedent cause of civic culture attitudes.

Second, the concept of civic culture is measured only by attitude about societal change and interpersonal trust. Life satisfaction (which is neither an attitude of specifically political culture nor a component of Almond and Verba's conceptualization of civic culture) is excluded. Also, instead of combining civic culture attitudes into a composite measure, we allow for the possibility that the relationships between these variables and change in democracy might differ in magnitude and significance. Furthermore, we replace support for revolutionary change with support for gradual reform because we think that the latter attitude about societal change is a much more unambiguous indicator of the kind of system affect that is most conducive to democracy.

Third, the percentage of the labor force employed in the tertiary sector is not included in our model because (1) we are skeptical about its validity as a proxy for the size of the middle class, (2) there is no evidence from large-sample quantitative cross-national research to indicate that it is a relevant determinant of variation in level of democracy, and (3) it is irrelevant either as a predictor of years of continuous democracy in model B (see Figure 1) or as a predictor of change in level of democracy (see equation 1.11). Instead, the alternative model includes—in addition to level of economic development-income inequality and subcultural pluralism, the two macrosocietal variables that we hypothesize are relevant causes of democratization omitted from Inglehart's model (equation 1.12 supports this hypothesis). Income inequality and subcultural pluralism are predicted to have negative effects on change in level of democracy. Also, support for gradual reform is expected to be higher in countries with lower levels of income inequality, while interpersonal trust is expected to be higher in homogeneous than in heterogeneous societies. And we include the possibility that level of economic development may have a positive effect on each of these civic culture attitudes.

Fourth, we treat the macrosocietal variables that are antecedent in time to level of democracy in 1981-90 as predetermined variables. Since our sample is limited by the small number of cases with information on civic culture attitudes, it is inappropriate for addressing the question of causal relationships between the macrosocietal variables, and this question is not relevant to our concern with linkages between structural characteristics of societies, civic culture attitudes, and democracy. The macroeconomic variables—income inequality and gross domestic product (GDP) per capita—are measured for the 1970s (see Appendix A). The ethnolinguistic fractionalization indicator of subcultural pluralism is available only for 1960-65. Years of continuous democracy is Inglehart's index dated to 1980. This variable should be highly correlated with level of democracy over 1972-80, as well as with the predetermined macroeconomic variables; but we do not expect it to have a direct influence on change in level of democracy that is independent of the other determinants of change in level of democracy.

The hypotheses to be tested are represented by the arrows of causation between the predetermined macrosocietal variables, the civic culture attitudes, and change in level of democracy. Support for Inglehart's

(1988, 1990) assumption that civic culture attitudes are a powerful unidirectional determinant of democracy will be found if support for gradual reform and/or interpersonal trust have strong effects on change in level of democracy, while long-term experience of democracy (years of continuous democracy, 1900-1980) does not have an effect on civic culture attitudes. Support for the hypothesis of reciprocal causation between civic culture and democracy, which Almond (1980) argues was the original hypothesis of The Civic Culture (Almond and Verba 1963), will be found if long-term experience of democracy has a positive effect on civic culture attitudes, and civic culture attitudes, in turn, have positive effects on change in level of democracy. Support for Barry's (1978) hypothesis that civic culture attitudes are primarily an effect rather than a cause of democracy will be found if long-term experience of democracy has a positive effect on civic culture attitudes and civic culture attitudes have no effect on change in level of democracy.

For the sake of simplicity, our causal model entails the assumption that there is no simultaneous causal effect of level of democracy during 1981–90 on civic culture attitudes during 1981–91. The validity of this assumption can be tested, however. If we find evidence of an effect of a civic culture attitude on change in democracy, then we will test for the possibility that the effect on change is inflated by simultaneity bias.

Determinants of Change in Level of Democracy

During the 1980s democracy expanded dramatically in the world. Many countries either inaugurated a democratic regime for the first time or else reestablished democratic rule after a previous lapse, and countries that had previously established stable democratic rule maintained it. Our data set-limited as it is mainly to European, North American, and Latin American countries—nevertheless reflects the global trend of the decade (see Appendix A). Four countries in our sample (Argentina, Greece, Spain, and Portugal) established stable democracy during the 1980s. In this group, the mean level of democracy during the 1970s had ranged from 42 to 68, and the mean level of democracy during the 1980s increased to a range of between 79 and 92. A fifth country, Honduras, came close to reaching the upper quarter of the democracy range (75 to 100), as its mean score changed from 43 over the 1970s to 73 over the 1980s. In addition to these instances of substantial gains in democratization, 16 countries maintained a high, stable mean level of democracy in the range of 83 to 100, with change in the range of less than ± 10 points. Only 6 countries did not either achieve or maintain a relatively high level of democracy. One of them, Panama, registered a gain in mean level of democracy of more than 10 points, but in this instance the change was from a very low level of democracy during the 1970s (20) to a relatively low level during the 1980s (39). The other five countries that remained at intermediate-tolow mean levels of democracy during the 1970s and

TABLE 2

Effects of Civic Culture Attitudes and Macrosocietal Variables: Expanded Sample

	EQUATION	QUATIONS EXPLAINING LEVEL OF DEMOCRACY, 1981–90					
INDEPENDENT VARIABLES	2.1	2.2	2.3	2.4ª	2.5 ^b	2.6	
Intercept	-18.29	88.22	94.12	104.55	80.45	93.13	
Level of democracy, 1972-80	+.57* (.19)	+.38* (.17)	+.32* (.15)	+.32* (.14)	+.32* (.14)	+.32* (.13)	
% support gradual reform, 1981-91	+.61* (.30)	+.63* (.25)	+.56* (.26)	+.53* (.24)	+.75* (.30)	+.62* (.22)	
% interpersonal trust, 1981-91	+.17 (.25)	+.14 (.24)	**************************************		_	_	
Years of continuous democracy, 1900-1980	07 (.15)	10 (.14)	******				
Top 20% income share, 1970-80		-1.68* (.52)	-1.54* (.52)	1.65* (.50)	1.56* (.50)	-1.60* (.39)	
Gross domestic product per capita, 1975	+.0028* (.0016)	0003 (.0015)	0006 (.0020)	0003 (.0014)	00008 (.00144)	_	
Ethnolinguistic fractionalization, 1960–65	23* (.11)	−.15 (.10)	18* (.09)	17* (.09)	15* (.09)	−.17* (.08)	
% labor force in services, 1977		_	03 (.31)		_	_	
% protestant population, 1964	_	_	06 (.08)		_		
Adjusted R ² Number of cases	.77 27	. 85 25	.85 25	.85 26	.86 24	.87 25	

Note: Entries are unstandardized regression coefficients (standard errors in parentheses).

1980s were El Salvador, Guatemala, Mexico, Nicaragua, and South Africa, where slight decreases occurred. The largest decrease was in South Africa, which declined from a mean democracy score of 34 over the 1970s to a mean of 27 over the 1980s.

We use the expanded sample of 27 countries to test the hypotheses of our alternative causal model. Inclusion of the six Central American countries substantially increases variation on the measures of democracy, since, with the exception of Costa Rica, these countries have low-to-intermediate scores on level of democracy during 1972–80 and 1981–90, and no years of continuous democracy from 1900 to 1980. Variation on the macroeconomic variables is also increased substantially, especially in regard to GDP per capita, since all of the Central American countries have lower levels of this indicator of economic development than any of the countries in Inglehart's sample.

The predictions of the alternative causal model about determinants of change in level of democracy are tested by the regression equations reported in Table 2.9 Equation 2.1 includes all of the explanatory variables except for income inequality, which is omitted because of missing data for Greece and Luxembourg. First, in regard to the civic culture attitudes, the coefficient on support for gradual reform is posi-

tive as expected and statistically significant. By contrast, the coefficient on interpersonal trust is not significant. These results are consistent with the findings from Inglehart's smaller European-oriented sample of no effect of interpersonal trust on change in democracy (equation 1.3) but a positive effect of support for gradual reform (equations 1.9-12). Variation in the percentage of the general public with high interpersonal trust thus appears to be irrelevant for democratization; whereas democratization is facilitated by the extent to which the general public favors gradual reform. 10 Second, in regard to the macrosocietal variables, years of continuous democracy does not have a significant direct effect on change in level of democracy, as we expected; but the coefficients on GDP per capita and ethnolinguistic fractionalization are significant and in the expected direction. The finding of a significant positive effect of level of economic development on change in democracy for the expanded sample differs from the finding of no effect for the European-oriented sample (see equation 1.11). This difference appears to be due to the fact that the expanded sample has greater variation in level of economic development than the Europeanoriented sample.11

Equation 2.2 includes income inequality. The coefficient on this variable is negative as expected; and it

[&]quot; Greece included.

^b Norway excluded.

^{*} $p \le .05$, one-tailed.

is more than three times as large as its standard error (t-ratio of -3.23), indicative of a strong inverse influence of income inequality on change in level of democracy. Also, when income inequality is taken into account, the coefficient on GDP per capita is reduced effectively to zero. There is a strong negative correlation between GDP per capita and income inequality (r = -.838); and the results of equation 2.2 indicate that income inequality is the variable with direct causal influence on democratization, while the effect of GDP per capita is indirect through its association with income inequality. This inference appears to be valid, despite the high correlation between these variables. If multicollinearity were a serious problem, then both of the parameter estimates would be imprecise and nonsignificant because their standard errors would be high. This is clearly not the case in regard to equation 2.2, however, since the coefficient on income inequality is much higher than its standard error. Another consequence of multicollinearity (if it is a problem) is that parameter estimates are unstable in the face of slight variations in the data matrix due to addition or deletion of observations or variables. This possibility is addressed in equations 2.3-5, which also focus on other concerns.

The two obviously irrelevant variables, interpersonal trust and years of continuous democracy, are deleted in equation 2.3; and we add two omitted variables, the percentage of the labor force in service occupations and the percentage of the population that is Protestant. 12 This equation is a test for the possibility of omitted-variable bias. The labor force in services variable was excluded a priori for the reasons given in our discussion of the alternative causal model (see Figure 2). Also, although Protestantism historically has been associated positively with a country's level of democracy (see Bollen and Jackman 1985), we do not think that Protestantism is relevant for explaining change in democracy from the 1970s to the 1980s because by this time all Protestant countries were stable democracies and the Catholic Church had become quite supportive of democracy. The results of equation 2.3 support our assumptions about the irrelevance of the labor force in services and Protestantism, since these variables do not have significant effects on change in level of democracy. Moreover, inclusion of them in the equation does not alter the pattern of effects observed in equation 2.2 for the highly correlated variables, income inequality, and GDP per capita.

Equation 2.1 was estimated across the full sample of 27 cases, but equation 2.2 could be estimated for only 25 cases because Greece and Luxembourg are missing data on income inequality for circa 1970–80. Equation 2.4 addresses the question of whether the absence of these cases affects the parameter estimates. Since income distribution in Luxembourg has never been measured to our knowledge, it is not possible to include that case. But in the case of Greece it is possible to substitute a 1957 measurement of the share received by the upper quintile of households

(49.5%) reported by Paukert (1973). We observe that when the circa 1960 income inequality score for Greece is included, the coefficient on income inequality is significant and the coefficient on GDP per capita is not significant, just as in equations 2.2 and 2.3. Also, support for gradual reform again has a significant positive effect on change in democracy. Thus missing data does not seem to be affecting the parameter estimates.

Equation 2.5 tests for the possibility that the parameter estimates could be affected by the case of Norway, where the comparatively very low score on support for gradual reform may be a reflection of measurement error. When Norway is deleted, the coefficient on gradual reform is higher than in the equations that include Norway (as expected), but the difference is relatively slight; and the other parameter estimates are quite similar. The possibility of measurement error in the Norwegian case thus has no significant influence on the results.

The irrelevant GDP per capita variable is deleted in equation 2.6, which is the best "trimmed" equation for predicting change in democracy from the 1970s to the 1980s. It explains 87% of the variance in change in level of democracy. The coefficients on the explanatory variables appear to be reliable estimates, since they are all more than twice their standard error and do not appear to be sensitive to multicollinearity, omitted variable bias, or measurement error. The most interesting substantive finding is the positive effect of the percentage of the general public that supports gradual reform on change in level of democracy, which supports the inference that variation in civic culture, at least as measured by this kind of attitude, may have causal influence on democratization. Now we need to address the question of whether the magnitude of this effect might be inflated by the possibility of a simultaneous reciprocal effect of level of democracy during 1980-90 on level of support for gradual reform during 1981-91. For level of democracy during 1981-90 to have an effect on level of support for gradual reform during 1981-91, a significant positive correlation must obtain between these variables. But that is not the case. The correlation between support for gradual reform during 1981-91 and level of democracy during 1981-90 is very close to zero (r = +.058). Therefore, the assumption that we made of no simultaneous reciprocal causation is supported.

Another important question to address in regard to the trimmed equation is the robustness of the direct effects of support for gradual reform, income inequality, and ethnolinguistic fractionalization on change in level of democracy. Given the relatively small number of cases even in the expanded sample, it is possible that the ordinary least squares (OLS) parameter estimates could be unreliable due to nonnormal residuals reflecting the presence of outliers, otherwise influential cases, and heteroscedastic distributions on the variables. Diagnostics can be used to detect the presence of specific cases that might be unusually influential. And there are alternative esti-

COUNTRY*	STANDARDIZED RESIDUALS	
	-3.0 0.0 3.0	DFITS
United States	······································	.1978
Canada		.5479
Norway	• •	.6627
Denmark	* :	2193
France	**	0472
Sweden	. :	5026
Germany	•	.0108
Netherlands	* <u>:</u>	1471
Belgium	•	0066
Australia	•	.1278
Britain	••	0402
Japan	* <u>*</u>	1650
Italy	•	.1290
Spain	•	.5139
Argentina		.6891
ireland	* •	3189
South Africa	*:	6642
Mexico	5 -	1436
		.4837
	• •	
	•	
	•#	
	*	
El Salvador		
Honduras		4 6070
Portugal Costa Rica Nicaragua Panama Guatemala El Salvador		.4837 .8492 -1.0634 .0896 5196 7464 1.6879

mation techniques that correct for violations of OLS assumptions.

A casewise plot of the standardized residuals from equation 2.6 is shown in Figure 3 along with each country's DFITS value. Velleman and Welsch suggest

that for small samples, a case with a DFITS value greater than three times the absolute magnitude of the square root of the ratio of parameters to cases is potentially influential (1981, 236–37). This cutoff point for equation 2.6 is 1.34. The only case with a DFITS value above 1.34 is Honduras (1.69). An alternative criterion is a change in fit greater than the absolute magnitude of the standard error (1.0) of fitted values (see Moon and Dixon 1992, 208). In addition to Honduras, Nicaragua has a DFITS value (-1.06) that exceeds this criterion. 13 These cases have the highest standardized residuals (2.04 and -2.06, respectively); but they are not severe outliers and in fact are relatively well predicted, since their residuals are very close to the range of ± 2 . The other cases are quite well predicted by equation 2.6.

The equations reported in Table 3 compare the OLS results for 2.6 (col. 1) with a variety of alternative estimation methods. First, we calculate the coefficients and their standard errors by bootstrapping, a nonparametric method of estimation that does not depend on the distributional assumptions of OLS (see e.g., Dietz, Frey, and Kaloff 1987; Golden 1993). The bootstrapped estimates derived from 200 replications are reported in equation 3.1. They are either identical or very similar to the OLS estimates. Second, we adjust for the possibility of heteroscedasticity by using a heteroscedasticity-consistent covariance matrix to calculate standard errors of the OLS regression coefficients (see Moon and Dixon 1992). The standard errors calculated by this method are reported in equation 3.2. They are quite similar to the OLS standard errors. Third, we use the least absolute error method for calculating "robust" estimators that are insensitive to nonnormal residuals (see, e.g., Dietz, Frey, and Kaloff 1987; Moon and Dixon 1992). The least absolute error estimators are reported in equation 3.3. Here we observe that the coefficients on

TABLE 3					
Tests of Robusti	ness of the Coeff	icients for the	Trimmed	Equation	2.6

	EQUATION	EQUATIONS EXPLAINING LEVEL OF DEMOCRACY, 1981–90					
INDEPENDENT VARIABLES	2.6	3.1ª	3.2 ^b	3.3°	3.4 ^d		
Intercept	93.13	90.97	93.20	110.93	126.67		
Level of democracy, 1972–80	+.32*	+.32*	+.32*	+.29*	+.25*		
	(.13)	(.13)	(.11)	(.06)	(.12)		
% support gradual reform, 1981-91	+.62*	+.65*	+.62*	+.42*	+.51*		
	(.22)	(.21)	(.17)	(.10)	(.20)		
Top 20% income share, 1970-80	-1.60*	-1.60*	-1.60*	−1.67*	-1.99*		
	(.39)	(.41)	(.37)	(.18)	(.36)		
Ethnolinguistic fractionalization, 1960-65	17*	−.17*	17*	−.13*	21*		
	(.08)	(.08)	(.06)	(.04)	(.07)		

Note: Entries are unstandardized regression coefficients (standard errors in parentheses). N = 25.

product per capita.

Ordinary least squares with bootstrapped estimates (200 replications).
 Heteroscedasticity-consistent covariance matrix.

^c Least absolute error estimators.

^d Bounded influence estimators.

^{*} $p \le .05$, one-tailed.

		TIONS EXPLA RT GRADUAL		EQUATIONS EXPLAINING % INTERPERSONAL TRUS		
INDEPENDENT VARIABLES	4.1	4.2	4.3ª	4.4	4.5	4.6
Intercept	72.30	83.0	86.51	44.56	29.51	32.07
Level of democracy, 1972-80	+.09 (.14)	+.09 (.15)	+.03 (.13)	09 (.16)	13 (.16)	
Years of continuous democracy, 1900-80	+.02 (.10)	02 (.10)	+.03 (.09)	+.28* (.11)	+.35* (.11)	+.18* (.05)
Top 20% income share, 1970–80	*****	17 (.47)	17 (.40)		+.27 (.51)	
Gross domestic product per capita, 1975	0016 (.0011)	0019 (.0013)	0015 (.0011)	0014 (.0012)	0007 (.0014)	
Ethnolinguistic fractionalization, 1960-65	+.04 (.08)	+.04 (.0 9)	01 (.07)	07 (.09)	09 (.09)	
Adjusted R ² Number of Cases	04 27	08 25	11 24	.20 27	.31 25	.31 25

support for gradual reform and ethnolinguistic fractionalization are reduced somewhat; but since these coefficients are still much larger than their standard errors, the differences are inconsequential substantively. Fourth, we use bounded influence estimators that reduce the influence of cases with high DFFITS values (see Moon and Dixon 1992). The bounded influence estimators reported in equation 3.4 are slightly smaller than the OLS estimators for support for gradual reform and ethnolinguistic fractionalization, and they are somewhat higher for income inequality and ethnolinguistic fractionalization. But again there are no substantive differences. In sum, the results of equations 3.1–4 indicate that the OLS parameter estimates of equation 2.6 are robust.

Determinants of Civic Culture Attitudes

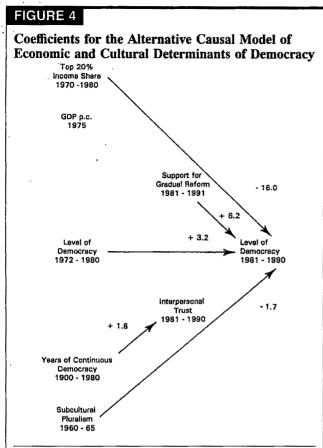
We now turn to the question of the influence of democracy on civic culture attitudes. The predictions of the alternative causal model about determinants of civic culture attitudes are tested by the regression equations reported in Table 4. These equations assume that there are no simultaneous causal relationships between support for gradual reform and interpersonal trust. That assumption is supported by the fact that these variables are uncorrelated both in the full extended sample (r = -.153, N = 27) and in the extended sample of 25 cases with information on all variables (r = -.122).

The first prediction equation for support for gradual reform (equation 4.1) is estimated across all 27 cases and excludes income inequality because of missing data. None of the macrosocietal variables have significant effects on support for gradual reform. This is because there are no significant bivariate correlations between any of the macrosocietal variables and support for gradual reform. ¹⁴ Equation 4.2 shows that the macrosocietal variables also do not have significant effects on support for gradual reform when income inequality is included as a predictor. Again, this reflects the absence of significant bivariate association between the macrosocietal variables and support for gradual reform. ¹⁵ Equation 4.3 excludes Norway due to the possibility of measurement error, but deletion of Norway has no appreciable effect on the results.

Interpersonal trust, by contrast, is correlated significantly and positively with level of democracy (1972–80) and years of continuous democracy (1900–80) in the full extended sample; ¹⁶ and it is correlated significantly and positively with these variables and significantly and negatively with income inequality in the extended sample of 25 cases. ¹⁷ Equations 4.4 and 4.5 show that in the multivariate context the only significant predictor of interpersonal trust is years of continuous democracy. Equation 4.6 is the trimmed equation (nonsignificant variables deleted) for interpersonal trust estimated across the sample with information on all the variables in the causal model.

Estimates of Causal Impact

Figure 4 shows the estimates of causal impact for our causal model of relationships between macrosocietal structural variables, civic culture attitudes of the general public, and democracy. ¹⁸ The results of testing this model of change in level of democracy do not support the argument that civic culture attitudes are the most important determinant of democratization.



Note: The coefficients are the ordinary least squares regression coefficients for equations with variables having nonsignificant coefficients deleted, where each coefficient is multiplied by 10 to show the impact of a change of 10 units in the independent variable on the dependent variable.

For this sample, the most important determinant is income inequality, which has a strong negative effect on change in level of democracy; that is, an increase of 10 percentage points in the size of the upperquintile income share is associated with a decrease of 16 points in level of democracy. The other macroeconomic variable—level of economic development as measured by GDP per capita—has no direct effect on change in level of democracy. Since there is a strong inverse correlation between GDP per capita and the size of the upper quintile income share, economic development is relevant to democratization indirectly, through its association with income inequality. The other structural variable that is directly relevant to democratization—ethnolinguistic fractionalization —has a small negative effect on change in level of democracy; that is, an increase of 10 points in a country's level of ethnolinguistic fractionalization is associated with a decrease of only 1.7 points in its level of democracy.

Our argument about the importance of analyzing the two civic culture attitudes separately is strongly supported. The percentage of the general public that supports gradual reform is empirically unrelated to the percentage with high interpersonal trust; and these variables also have very different relationships with democracy. Support for gradual reform has a positive effect on change in level of democracy (i.e., an increase of 10 percentage points in the general public supporting gradual reform is associated with an increase of 6.2 points in a country's level of democracy) and is unrelated to long-term experience of democracy. The findings regarding support for gradual reform thus support the hypothesis of a unidirectional effect of civic culture on democracy. By contrast, the findings regarding interpersonal trust support the hypothesis of a unidirectional effect of democracy on civic culture. This is because interpersonal trust is unrelated to change in level of democracy but long-term experience of democracy has a positive effect on interpersonal trust; that is, an increase of 10 years of continuous democracy is associated with an increase of 1.8 percentage points in the proportion of the general public with high interpersonal trust.

CONCLUSION

We addressed the question of the relevance of attitudes of the general public for the establishment of stable democratic regimes by testing a causal model of macrosocietal and microattitudinal determinants of change in level of democracy. The model was designed to enable us to draw inferences about the possibility of unidirectional or reciprocal causation between civic culture attitudes and democracy. Although our sample of countries for testing the model is restricted to a maximum of 27 cases with data on civic culture attitudes, it nevertheless contains reasonably large variation on change in democracy that corresponds to the global trend of the 1970s and 1980s. So, despite its limitations, our data set affords an opportunity to go beyond speculation and actually test hypotheses about the relative importance of civic culture attitudes for democratization.

Interpersonal trust is one kind of civic culture attitude that has been assumed by many scholars to be an important attitudinal prerequisite of the establishment of stable democracy (e.g., Almond and Verba 1963; Dahl 1971; Inglehart 1988, 1990). However, we found that variation in the percentage of the general public with a high level of interpersonal trust is unrelated to change in a country's level of democracy. Low levels of interpersonal trust thus do not appear to be an impediment to democratization: Argentina, Portugal, and Spain registered substantial increases in level of democracy from the 1970s to the 1980s despite relatively low interpersonal trust levels of 21%, 28%, and 35%, respectively; and Belgium, France, and Italy were able to maintain high levels of stable democracy despite low interpersonal trust levels of 29%, 26%, and 27%, respectively. Relatively high levels of interpersonal trust also do not necessarily promote democratization. Neither Guatemala nor Panama were able to move above intermediate levels of democracy despite above-average interpersonal trust levels of 40% and 43%, respectively. (The

average level of interpersonal trust for all 27 countries was 39%.)

Interpersonal trust is not unrelated to democracy, however. A country's long-term experience of democracy (as measured by its years of continuous democracy since 1900) is estimated to have a positive effect of moderate magnitude on the percentage of the general public with a high level of interpersonal trust. In our view, this is not surprising. Democracy works in practice through the peaceful collective action of groups of citizens; and because peaceful collective action is grounded in a spirit of cooperation, the institutional opportunities for peaceful collective action afforded by democratic regimes could be expected to promote relatively high levels of interpersonal trust.

Although interpersonal trust appears to be a product of democracy rather than a cause of it, there is one kind of attitude that supports the civic culture thesis. This is the percentage of the general public that prefers to change society by gradual reform instead of changing it by revolutionary action or defending it staunchly against subversive forces. Support for gradual reform has a positive effect on change in level of democracy, and it is unrelated to long-term experience of democracy. Thus the strong assumption made by Inglehart (1988, 1990) of a unidirectional causal effect of civic culture on democracy is supported in this instance.

There is an interesting implication of the fact that support for gradual reform not only is not a function of prior experience of democracy but also is not associated with any of the other structural variables in our causal model. The level of support for gradual reform in a country is independent of (and so is not constrained by) its level of economic development, the extent to which income is distributed unequally, and the subcultural heterogeneity of its population. Therefore, a high level of support for gradual reform could compensate for the presence of levels of these structural variables that are not conducive to democratization. A case in point is Honduras. Honduras has the lowest level of economic development of all the countries in our sample, and it has the highest level of income inequality. But in Honduras, 85% of the general public prefers gradual reform to revolutionary change or defending the status quo, which is the highest level of support for gradual reform of all the countries in our sample. It is possible that the very high level of support for gradual reform among the Honduran general public could have had a strong or even decisive influence on the transition from authoritarian rule to democracy in this country whose prospect for democracy from the perspective of macroeconomic preconditions was quite poor.

Although support for gradual reform appears to be an important civic culture attitude because it can promote democratization even in countries that lack conducive structural properties, nevertheless, in evaluating the civic culture theory one must keep the following caveats in mind. First, the magnitude of the positive effect of support for gradual reform on change in democracy is relatively small in comparison with the magnitude of the negative effect of income inequality. Second, the other variables included by Inglehart under the rubric of civic culture do not have statistically significant effects on change in democracy. Thus, overall, the results of our analysis of causal linkages between levels of civic culture attitudes and change in level of democracy are not supportive of the thesis that civic culture attitudes are the principal or even a major cause of democracy.

Our conclusion about the causal relevance of civic culture attitudes for democracy is thus at odds with Inglehart's (1988, 1990) conclusion. Since we are confident that our findings are robust statistically, we doubt that relationships more supportive of the civic culture thesis will be found with larger and more geographically diverse samples. However, it is certainly possible that the single-item indicators of civic culture attitudes used by us and by Inglehart are too crude and that stronger causal relationships might be found with potentially more reliable multiitem indexes.

The single most important explanatory variable in our causal model of determinants of democratization is not an attitude of the general public but rather a macroeconomic variable—income inequality. The size of the income share of the richest quintile of households in a country was found to have a relatively strong negative effect on change in level of democracy from the 1970s to the 1980s. The transitions from authoritarian rule to stable democracy (so far) in Argentina, Portugal, and Spain appear to have been facilitated by the existence of relatively egalitarian distributions of income (upper-quintile income shares in the range of 41% to 50%). By contrast, the continuation of relatively low levels of democracy during the 1970s and 1980s in El Salvador, Guatemala, Mexico, Nicaragua, Panama, and South Africa has gone hand-in-hand with extremely inegalitarian distributions of income (upper-quintile income shares in the range of 57% to 61%). We think that there is a compelling causal logic for why high levels of income inequality would make it difficult to sustain a high mean level of democracy for very long. High levels of income inequality are likely to produce either a high level of rebellious political conflict (Muller and Seligson 1987) or else the perception among elites of a threat of rebellious political conflict and lower-class revolution. Therefore, executive or military coups to quell mass rebellion and preserve elite privileges are likely to occur in countries with inegalitarian distributions of income that attempt to establish de-

In future macro-micro research designs a useful new direction to take may be to shift the focus at the micro level from attitudes of the general public to attitudes of elites. Since elites have greater opportunity and ability than the general public to influence the kind of regime a country will have, their attitudes should be given special emphasis in political culture models. Dahl (1971) attributes great importance to the attitudes of political activists and leaders as a cause of the establishment and stability of democracy. Higley

and Burton (1989) make the even stronger argument that the single critical determinant of the stability of democratic regimes is consensus among elites in general on support for democratic institutions and values.

Empirical support for an emphasis on elite attitudes is provided by the pioneering study of Gibson (1988), which is limited however to secondary analysis of relatively old data collected in the mid-1950s for only a single country, the United States. Gibson tested a causal model of relationships between attitudes of political tolerance in the general public, political tolerance of elites, and repressive political legislation of state governments. He found statistical support for the inference that variation in the extent of repressive legislation against communists during the late 1940s and 1950s was an inverse function of the level of elite political intolerance and that the level of political intolerance among the general public was not directly relevant (although it was highly correlated with the level of elite intolerance). In the context of the causal model tested here, an important open question is whether change in the repressiveness of the political system, as measured by change in level of democracy, is affected by the level of support for gradual reform among the general public when variation in elite support for gradual reform is taken into account.²¹ Thus, a final caveat in regard to the civic culture thesis is that we do not know whether the apparent positive influence on change in democracy of support for gradual reform among the general public is a genuine effect or is a spurious reflection of association between general public attitudes and elite attitudes, which might be the operative causal variable.

APPENDIX A: DATA

Scores on the democracy, macroeconomic, and civic culture variables are reported in Table A-1 for the 27 countries with information on attitudes about social change and interpersonal trust. The countries are listed in descending order of their level of gross domestic product per capita in 1975. The six Central American countries all have gross domestic product per capita scores below that of Portugal, the poorest country in Inglehart's European-oriented sample,

						% Defend			
Country	Level of Democracy, 1981–90	Level of Democracy, 1972–80	Top 20% Income Share, 1970-80	GDP per Capita, 1975	% Support Revolutionary Change, 1981-91	% for Gradual Reform, 1981–91	Society against Subversives, 1981–91	% Interpersonal Trust, 1981–91	
European-oriented				- At.					
sample									
U.Ś.A.	100.00	100.00	41.4	10197	5.0	73.0	22.0	41.0	
Canada	100.00	100.00	40.5	10147	5.0	74.0	21.0	49.0	
Luxembourg	100.00	95.56		8949	5.0	70.0	25.0	32.0	
Norway	100.00	100.00	37.8	8580	2.0	49.0	49.0	60.0	
Denmark	100.00	100.00	38.6	8374	3.0	69.0	28.0	52.5	
France	91.67	93.67	43.9	8358	9.0	72.0	19.0	25.5	
Sweden	100.00	99.11	39.4	8351	4.0	80.0	16.0	57.0	
Germany	92.59	97.22	42.6	8067	3.0	59.0	38.0	32.0	
Netherlands	100.00	100.00	39.6	8031	3.0	71.0	26.0	45.0	
Belgium	100.00	100.00	37.9	7917	7.0	72.0	21.0	29.0	
Australia	100.00	100.00	43.0	7739	4.0	73.0	23.0	48.5	
Britain	100.00	100.00	39.6	7082	5.0	72.0	23.0	43.0	
Japan	100.00	92.81	39.3	6518	3.0	71.0	26.0	42.5	
Italy	98.15	90.04	45.2	5685	8.0	73 .0	19.0	27.0	
Spain	91.67	53.59	41.1	5478	8.0	82.0	10.0	35.0	
Argentina	78.70	41.78	50.3	4214	13.0	77.0	10.0	21.0	
ireland	100.00	96.44	39.4	4157	4.0	76.0	20.0	42.0	
S. Africa	26.85	34.44	58.0	4095	25.0	54.0	21.0	29.0	
Greece	87.04	68.44		3770	9.0	63. 0	28.0	50.0	
Mexico	53.70	55.44	60.9	3586	12.0	77.0	11.0	18.0	
Portugal	90.74	62.89	49.1	3057	14.0	74.0	12.0	28.0	
Central America									
Costa Rica	100.00	100.00	49.8	2646	.5	71.0	28.5	45.1	
Nicaragua	35.19	38.89	59.0	2461	10.1	72.7	17.2	31.9	
Panama	38.98	20.19	60.8	2417	7.8	65 .0	27.2	43.4	
Guatemala	50.93	57.26	56.5	1717	3.1	78. 5	18.4	39.9	
El Salvador	56.48	63.04	58.7	1482	3.3	82.3	14.4	35.4	
Honduras	73.15	42.74	63.5	895	.9	85 .0	14.1	50.4	

which dramatically increases the variability on gross domestic product per capita.

The measurements of mean level of democracy over 1981-90 and 1972-80 are given in columns 1 and 2. These scores are derived from annual Freedom House rankings of political rights and civil liberties, converted to a scale of 0-100 (see Freedom House publications Freedom at Issue, Jan./Feb. 1990 and Freedom Review 22, no. 1 [1990]; see also Gastil 1986, 1988).²² The 1980s were a decade of expansion of democracy throughout the world, and our sample of primarily European and Latin American states reflects this trend. The expansion trend began in the late 1970s with transitions from authoritarian to democratic rule in Portugal and Spain. In Latin America, where almost all states were governed by authoritarian regimes in the mid-1970s, there was an extraordinary shift to democracy in the 1980s, so that by 1990 almost all Latin American states were under democratic rule or close to it. The old European and European-heritage democracies (plus Japan) in our sample maintained very high levels of democracy during the 1970s and 1980s; the Southern European states Portugal and Spain increased their level of democracy substantially from the 1970s to the 1980s; and the level of democracy in two Latin American states, Argentina and Honduras, also increased substantially. Declines in democracy of five points (rounded) or more occurred only in El Salvador, Germany, Guatemala, and South Africa.

Scores on the macroeconomic variables are listed in the third and fourth columns of Table A-1. Income inequality is measured by the size of the income share received by the upper quintile of households, averaged for circa 1970 and circa 1980.²³ Greece and Luxembourg are missing data on income inequality. Economic development is measured by real gross domestic product per capita in 1975 (Summers and Heston 1988).

Data on attitude about societal change are listed in Table A-1, columns 5-7. The percentage of respondents in each country who chose the first of three options is listed in the column 5, the percentage that chose the second option is listed in the column 6, and the percentage that chose the third option is listed in the column 7. The options were presented as follows:

On this card are three basic kinds of attitudes concerning the kind of society we live in. Please choose the one that best describes your own opinion:

- 1. The entire way our society is organized must be radically changed by revolutionary action.
- 2. Our society must be gradually improved by reforms.
- Our present society must be valiantly defended against all subversive forces.

Support for revolutionary change among the general public is very low in most of the countries in our sample. It is below the 10% level in 20 countries, marginally above 10% in four countries (Nicaragua, Mexico, Argentina, and Portugal), and reaches a level of 25% only in the case of South Africa. Support for gradual reform is quite high in Honduras (85%), El

Salvador (82.3%), Spain (82%), and Sweden (80%); it is comparatively low in Panama (65%), Germany (59%), South Africa (54%), and Norway (49%). Support for defending the status quo against subversive forces ranges from a low of 10% in Argentina and Spain to highs of 38% and 49% in Germany and Norway, respectively.

Table A-1, column 8 is the percentage of respondents who believe that most people in their country are either fairly or very trustworthy. There is considerable cross-national variation in levels of interpersonal trust. Interpersonal trust is highest in the Scandinavian countries (Denmark, Norway, and Sweden), where the level of trust in one's compatriots ranges between 52.5% and 60%. The lowest levels of interpersonal trust are found in Argentina, France, and Mexico, where one-quarter or less of the general public think that others in their country are fairly or very trustworthy.

APPENDIX B: THE CENTRAL AMERICAN PUBLIC OPINION PROJECT

The Central American Public Opinion Project of the University of Pittsburgh²⁴ was designed to measure the opinion of Central Americans on a wide variety of issues and included the interpersonal trust and system affect questions used here. The samples drawn from each country were designed to be representative of the urban population.²⁵ The decision to focus on urban areas was made in order to avoid the high cost of interviewing in rural areas, which, in these countries, are often not easily accessible.

Country samples were of area probability design. In each country, the most recent population census data were used. Within each stratum, census maps were used to select, at random, an appropriate number of political subdivisions (e.g., districts), and within each subdivision, the census maps were used to select an appropriate number of segments from which to draw the interviews. Within the household, all voting-age residents were eligible for selection, and one was chosen at random (using either the "next birthday system" or a sex/age quota system).

Costa Rica was designated as the country for the pilot test of the survey items. That sample was gathered in fall 1990. The surveys in the other five countries were then conducted during the summer of 1991 and the winter of 1991–92. The design called for samples of at least 500 up to a maximum of 1,000 respondents from each country. The sample sizes for each country are as follows: Guatemala, 904; El Salvador, 910; Honduras, 566; Nicaragua, 704; Costa Rica, 597; and Panama, 500.

Notes

An earlier version of this paper was presented at the 1993 meeting of the Midwest Political Science Association, Chicago, and as a public lecture in Managua, Nicaragua that same

year sponsored by the Centro de Educacion para la Democracia. We are grateful for the helpful comments that we received at these presentations. We also would like to thank Hortense Dicker, coordinator of the Nicaragua Education for Democracy Project of the American Federation of Teachers, for her assistance in arranging the lecture in Managua.

1. In responding to Barry's counterhypothesis, Almond emphasizes that in *The Civic Culture* the relationship between civic culture and democracy was conceptualized as entailing reciprocal causation: "political culture is treated as both an independent and a dependent variable, as causing structure and as being caused by it" (1980, 29).

- 2. Inglehart's article reports the number of cases as 24 (1988, 1219); but his book reports the number of cases as 22 (1990, 45). From the data reported in the book we find only 20 countries with scores on all three components of civic culture. These are the first 21 countries listed in Table A-1, except for Mexico (which is missing information on life satisfaction). We constructed the composite civic culture index by (1) subtracting scores on the support for revolutionary change variable from 100 so that scores are the percentage who oppose revolutionary change, (2) multiplying life satisfaction scores (originally on a 0-10 scale) by 10 so that the range of the scale is the same as for opposition to revolutionary change and interpersonal trust (0-100), and (3) computing the mean of the three variables. Gross national product per capita is the average for 1955 and 1960 if data are available for both years; otherwise it is either 1955 or 1960. Gross national product per capita and the percentage of the labor force in the tertiary sector in 1965 are from the data tape for the second edition of The World Handbook of Social and Political Indicators (Taylor and Hudson 1972).
- 3. Inglehart used LISREL to estimate path coefficients, whereas we use ordinary least squares regression. The path coefficients are standardized regression coefficients or beta weights.
- 4. The coefficient on years of continuous democracy is \pm .16 with a standard error of .04 (t-ratio of 3.83), the coefficient on labor force in the tertiary sector is \pm .16 with a standard error of .20 (t-ratio of \pm .81), and the coefficient on GNP per capita is \pm .0018 with a standard error of .0025 (t-ratio of .74). When the nonsignificant variables are trimmed from the equation, the standardized regression coefficient of \pm .83 for years of continuous democracy is the same as the bivariate correlation between years of continuous democracy and civic culture.
- 5. The coefficient on labor force in the tertiary sector is 1.40 with a standard error of 1.08 (t-ratio of 1.30) and the coefficient on GNP per capita is +.024 with a standard error of .013 (t-ratio of 1.85). The coefficient on GNP per capita is significant at the .05 level for a one-tailed test.
- 6. The number of cases for the separate components of the civic culture index is greater than 20 because an additional five countries have information on one or two (but not all three) of the components. Austria and Switzerland have information on life satisfaction only, Finland and Hungary have information on life satisfaction and interpersonal trust, and Mexico has information on interpersonal trust and support for revolutionary change. Thus the number of cases is 24 for life satisfaction, 23 for interpersonal trust, and 21 for support for revolutionary change.
- 7. South África's logged support for revolutionary change score is 3.22, and the other countries with support for revolutionary change of 10% or more have logged scores of 2.64 (Portugal), 2.56 (Argentina), and 2.48 (Mexico). The negative effect of support for revolutionary change washes out when logged scores are used because in contrast to the decline in level of democracy experienced by South Africa, the other countries with high logged scores that are close to that of South Africa experienced either substantial increases in democracy (Portugal and Argentina) or little change (Mexico).
- 8. The number of cases is 19 because Greece and Luxembourg are missing data on income inequality.
- 9. Since level of democracy 1972–80 is included in these equations, the effects of the other independent variables on level of democracy 1981–90 are effects on change in democ-

racy (i.e., the level of current democracy independent of its previous level).

10. We also tested for the possibility that support for revolutionary change might have a negative effect on change in level of democracy. If support for gradual reform is replaced by support for revolutionary change in equation 2.1, the coefficient on support for revolutionary change is -.27, and its standard error is .76; support for revolutionary change thus has no significant effect on change in democracy in the larger sample, even with the extreme score of South Africa included. Alternatively, if logged support for revolutionary change is used, the coefficient on this variable is extremely small relative to its standard error (-.22 and 6.60, respectively). These findings for the larger sample reinforce our conclusion from the results of equations 1.5 and 1.6 that variation in the level of support for revolutionary change has no significant effect on change in level of democracy; and they thus support the assumption of our alternative causal model that the attitude about social change relevant for democratization is support for gradual reform rather than support for revolutionary change.

11. If equation 2.1 is estimated only for the 21 cases in the European-oriented sample, the coefficient on GDP per capita in 1975 is not significant (.05 level, one-tailed test), whereas the coefficients on support for gradual reform and ethnolinguistic fractionalization remain significant. Therefore, it must be the difference in variation of GDP per capita between the two samples that is responsible for the different effects instead of differences in model specification or in the indicator of level of economic development (GDP vs. GNP) or its timing (1975 vs. 1955–60).

12. Data on the percentage of the labor force in services for 1977 is from Taylor and Jodice 1983 vol. 1, tbl. 6.3. Data on the percentage of the population that is Protestant circa 1964 is from Taylor and Hudson 1972, tbl. 4.16.

13. If equation 2.6 is reestimated with Honduras deleted, adjusted R-squared increases to .90 and all of the coefficients on the explanatory variables remain significant at the .05 level (one-tailed) and are greater than twice their standard error. If Nicaragua is deleted, adjusted R-squared is approximately the same (.88), and the explanatory variables again remain significant and greater than twice their standard error. The only notable consequence of deleting Honduras is that the coefficient on income inequality becomes 5, instead of 4 times its standard error, while the only notable consequence of deleting Nicaragua is that the coefficient on ethnolinguistic fractionalization becomes 3 instead of 2 times its standard error. Thus these cases do not appear to be substantively influential.

14. The correlations for N=27 between support for gradual reform and level of democracy, 1972–80; years of continuous democracy, 1900–1980; GDP per capita 1975; and ethnolinguistic fractionalization 1960–65 are -.116, -.150, -.283, and +.007, respectively.

15. The correlations for N = 25 between support for gradual reform and level of democracy, 1972–80; years of continuous democracy, 1900–1980; top 20% income share; GDP per capita 1975; and ethnolinguistic fractionalization 1960–65 are –129, –189, +.191, –.320, and –.025, respectively.

16. The correlations for N=27 between interpersonal trust and level of democracy, 1972–80; years of continuous democracy, 1900–1980; GDP per capita, 1975; and ethnolinguistic fractionalization, 1960–65 are +.351, +.480, +.222, and -.196, respectively.

17. The correlations for N=25 between interpersonal trust and level of democracy, 1972–80; years of continuous democracy, 1900–1980; top 20% income share; GDP per capita 1975; and ethnolinguistic fractionalization, 1960–65 are +.397, +.582, -.347, +.294, and -.188, respectively.

18. Since the determinants of level of democracy, 1981–90, are measured by scales that range theoretically from zero to one hundred, their relative importance can be compared using the OLS regression coefficients from equation 2.6. Since none of the predetermined macrosocietal variables are correlated significantly with support for gradual reform, no impact coefficients for them are shown. The impact coefficient for the

effect of years of continuous democracy on interpersonal trust is the OLS regression coefficient from equation 4.6.

19. In addition to interpersonal trust, life satisfaction (analyzed for the European-oriented sample only) and support for revolutionary change (analyzed for the European-oriented sample and the extended sample) were found to be irrelevant to change in level of democracy.

20. Muller (1988) found a very strong relationship between income inequality and the likelihood of a country maintaining stable democracy during 1961-80 for a sample of 33 countries but no significant relationship between income inequality and the likelihood of a country inaugurating democracy during 1945-61 for a sample of 27 countries. In regard to the latter sample, only four of these countries had relatively egalitarian distributions of income (an upper quintile share of 50% or less). Three of them did inaugurate democracy (Italy, Malaysia, and West Germany) and one did not (Chad). By contrast, among the 13 countries with very inegalitarian distributions of income (an upper quintile share of 55% or more), six did not inaugurate democracy (Colombia, Gabon, Iraq, Madagascar, Mexico, and Senegal) but seven did inaugurate it (Brazil, Costa Rica, Guatemala, Jamaica, Lebanon, Panama, and Peru). The absence of a relationship between income inequality and the likelihood of inaugurating democracy is therefore due primarily to the fact that a majority of the countries with very inegalitarian distributions of income nevertheless did attempt to establish democratic regimes. Most of them were unsuccessful, however, as breakdowns of democracy due to military coup and/or civil war occurred in Brazil, Lebanon, Guatemala, Panama, and Peru. Thus the inauguration of democracy may be facilitated by relatively low levels of income inequality, although there can be exceptions-perhaps especially if a country is very poor, like Chad. High levels of income inequality do not necessarily appear to inhibit the inauguration of democracy; however, they do make it unlikely that a country will maintain a high, stable level of democracy for very long.

21. There are several cases in our sample that suggest the possibility that the level of elite support for gradual reform might be as important as (or even more important than) the level of support for gradual reform in the general public. The countries with the lowest levels of support for gradual reform are Norway, South Africa, and Germany, where gradual reform is supported by 49%, 54%, and 59% of the general public, respectively. In the case of Norway, the comparatively very low level of support for gradual reform is due to an unusually high level of support for defending the status quo against subversive forces, which could of course reflect measurement error. Alternatively, in a stable democracy like Norway, elite support for gradual reform might be much higher than the level found in the general public. In Germany, the relatively low level of support for gradual reform among the general public also is due to a relatively high level of support for defending the status quo against subversives; and the stability of German democracy also might depend more on elite attitudes. In the case of South Africa, the very low level of support for gradual reform in the general public coincided with a low level of democracy during the 1970s that declined to an even lower level in the 1980s; but South Africa currently is in the process of attempting to establish a democratic regime. We doubt that support for gradual reform among the general public recently has undergone any sudden, dramatic increase. Rather, our guess is that there must be considerably greater support for gradual reform among South African elites than among the general public and that elite preference for gradual reform over revolutionary change or staunch defense of the status quo is the driving force behind the movement for democracy in South Africa, which also could have the effect of gradually raising the level of support for gradual reform among the general public. Thus, if we had information on elite support for gradual reform, it is possible that this variable could have an independent positive effect on change in level of democracy that might be stronger than that of support for gradual reform in the general public and consequently would provide stronger support for the role of attitudes as a determinant of democratization.

22. In Freedom in the World, ratings listed for years 1973–82 are for the previous year, and ratings listed for 1983 to 1987 are for the current year (Gastil 1986, tbl. 6; idem 1988, tbl. 6).

23. The sources of these data are the World Bank (Ahluwalia 1976; World Bank 1979, 1984, 1989, 1990); the United Nations Economic Commission for Latin America (Comisión Económica para América Latina 1983); the Organization for Economic Development and Cooperation (Sawyer 1976); and the International Labor Organization (Lecaillon et al. 1984)-W, U, O, and I, respectively. Data for circa 1970 encompass the interval 1964–74, and data for circa 1980 encompass the interval 1975–84. If multiple observations were available, the one for the year closest to the midpoint of the interval was used. The sources and years (with circa 1970 and circa 1980 scores in parentheses in the case of averages) are as follows for each country: Argentina, W:70 (no data for circa 1980); Australia, average of W:66–67 (38.8) and W:75–76 (47.1); Belgium, average of W:74–75 (39.8) and W:78–79 (36.0); Canada, average of W:69 (41.0) and W:81 (40.0); Costa Rica, average of U:70 (50.6) and U:80 (49.0); Denmark, W:81 (no data for circa 1970); El Salvador, average of U:70 (50.8) and U:80 (66.6); France, average of W:70 (46.9) and W:79 (40.8); Germany, average of O:70 (45.6) and W:78 (39.5); Guatemala, average of U:70 (58.8) and U:80 (54.1); Honduras, average of U:70 (67.7) and U:80 (59.3); Ireland, W:73 (no data for circa 1980); Italy, average of W:69 (46.5) and W:77 (43.9); Japan, average of W:69 (41.0) and W:79 (37.5); Mexico, average of W:69 (64.0) and W:77 (57.7); Netherlands, average of W:67 (42.9) and W:81 (36.2); Nicaragua, average of U:70 (60.0) and U:80 (58.0); Norway, average of W:70 (37.3) and W:82 (38.2); Panama, average of U:70 (61.2) and U:80 (60.3); Portugal, W:73-74 (no data for circa 1980); South Africa, I:65 (no data for circa 1980); Spain, average of W:74 (42.2) and W:80-81 (40.0); Sweden, average of W:72 (37.0) and W:81 (41.7); United Kingdom, average of O:72 (39.4) and W:79 (39.7); United States, average of W:72 (42.8) and W:80 (39.9).

24. This project is funded by the Andrew Mellon Foundation, the Tinker Foundation, the Howard Heinz Endowment, the University of Pittsburgh Central Research Small Grant Fund, and the Instituto de Estudios Latinoamericanos. The collaborating institutions in Central America were, for Guatemala, Asociación de Investigación y Estudios Sociales; for El Salvador, Instituto de Estudios Latinoamericanos; for Honduras, Centro de Estudio y Promoción del Desarrollo and Centro de Documentación de Honduras; for Nicaragua, Centro de Estudios Internacionales and the Escuela de Sociología, Universidad Centroamericana; for Costa Rica, Universidad de Costa Rica; and for Panama, Centro de Estudios Latinoamericanos "Justo Arosemena." Collaborating doctoral students in Political Science at the University of Pittsburgh were Ricardo Córdova (El Salvador), Anabelle Conroy (Honduras), Orlando Pérez (Panama), and Andrew Stein (Nicaragua). Collaborating faculty were John Booth, University of North Texas (Nicaragua and Guatemala), and Jon Hurwitz, University of

Pittsburgh (Costa Rica).

25. Official census definitions are of little help in defining urban areas, because they include population concentrations so low as barely to make them distinguishable from rural villages. For example, in some countries populations over twenty-five hundred are considered urban when, in fact, these places are at best no more than very small towns. We sought to narrow our definition of urban to include the areas of major population agglomeration. In Guatemala, this meant Guatemala City, Esquintla, Quezaltenango, and other major concentrations. In El Salvador, it meant greater metropolitan San Salvador, including the city of San Salvador (divided into 14 zones) and the eight surrounding municipios: Soyapango, Cuscatancingo, Ciudad Delgado, Mejicanos, Nueva San Salvador, San Marcos, Ilopango, and Antiguo Cuscatlán. In Honduras, it meant the nation's two large metropolitan areas, Tegucigalpa (the capital) and San Pedro Sula. In Nicaragua, this definition included Managua (the capital) and the regional cities of Leon, Granada, and Masaya. In Costa Rica, the

sample covered the greater metropolitan region, incorporating San José (the nation's capital) and the provincial capitals of the meseta central (Cartago, Heredia, and Alajuela). The Panama sample was confined to the metropolitan Panama City атеа.

26. In Central America, census bureaus divide the census maps into small areas designed to be covered by a single census taker. The maps are sufficiently detailed to show all of the dwelling units. In places like Panama City, where there are a large number of apartment buildings, lists are available that show the number of dwelling units within each building. In the larger buildings this sometimes results in more than one census segment per building. In El Salvador census maps completed two years prior to the survey were available, but the census itself had not yet been taken. The maps were not of sufficient detail to show each dwelling unit, but they did show the major streets and landmarks (e.g., churches) and did provide a housing count for each census segment.

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ON THE IDEA OF THE MORAL ECONOMY

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he moral economic school, which has flourished among anthropologists, economic historians, and classicists, has received only limited attention from political scientists. This is perplexing, since at its core is to be found an intersection of debates over rational choice theory, the character of modern and premarket societies, and the normative standing of the market—in other words, over issues of formidable importance to our discipline. I seek to correct that neglect by mapping out and critically analyzing the moral economists' conception of modernity, their critique of the economic approach to human behavior and institutions, and their attempt to formulate an Aristotelian theory of the economy. These projects, though flawed, together are more than rich enough to provide fertile ground for political scientists and philosophers. I conclude with a discussion of the moral economists' effort to develop a normative theory of the economy together with a related critique of the market.

Though familiar territory among anthropologists, economic historians, and classicists, the cluster of theories grouped under the heading moral economy is as yet largely uncharted terrain for political scientists and political philosophers. This is puzzling, for there is much to recommend them to our attention. They (1) offer a characterization of modernity—and of the transition from traditional societies to the present epoch—that meshes well with and fills out such provocative readings as Habermas on system and "lifeworld"; (2) are engaged in the controversy over the "economic approach to human behavior," over rational choice and related theories, and especially over the understanding of nonmarket societies; (3) emerge in part from the Gemeinschaft critique of modernity and offer a non-Marxist critique of the liberal valuation of market society and thereby an economic extension of the communitarian/liberal dispute; and (4) can be read as an attempt to sketch the outlines of a (loosely) Aristotelian economic theory in opposition to the rights-based arguments that at present command the horizon of normative economic theorizing.2 It is obviously not feasible on the small stage of an essay to enter into the full detail of each of these areas. I propose, therefore, to reconstruct and criticize the core elements of the moral economic approach and then develop its normative dimension more extensively. First, I sketch the ideas of the embedded and disembedded economies along their institutional, explanatory, and normative dimensions. Then I critically analyze this approach while also seeking to assess, at least provisionally, its normative heart.

THE EMBEDDED ECONOMY

The theoretical center of the moral economic approach is the defining dichotomy of the embedded economy and the disembedded, or autonomous, market. This is played out as (1) the basis of an historical account distinguishing between two grand

classes of economic regime, two ways in which the economy is integrated into society; (2) the core of a higher-order debate over the proper theoretical path to be taken in understanding market and nonmarket societies; and (3) the foundation of a critique of modernity in the light of certain properties of the preor nonmarket world and as the framework for an alternative approach to the normative valuation of economic regimes. The idea of the embedded economy is the master concept uniting these varied facets of the theory of the moral economy, and it will serve throughout as my guiding thread.

At the most basic level, the claim that premarket economies were embedded means that the human interchange with nature was "submerged" in social relations (Polanyi 1957c, 46; idem 1960, 329; idem 1968, 65). Unfolded further, this amounts to the assertion that historically the provisioning of humans—the securing of their livelihood—was located in, or integrated through, noneconomic institutions (e.g., kinship, religion, and politics). According to Evans-Pritchard's study, The Nuer, for example, Nuer economic relations "always form part of direct social relationships of a general kind" (1940, 90). The specific forms of economic integration may vary (reciprocity, redistribution, exchange), but what they have in common is that they are molded, in their ends and instruments, by noneconomic forces. Where the economy is embedded, it is Gemeinschaftthe institutions, traditions, and norms of the community—that governs it. We could put this differently by saying that before modernity, the securing of human livelihood had no separateness—no boundary line that marked it out as distinct from the enveloping society's institutions and values (Polanyi 1977, 55; see also Polanyi 1957a, 70; idem 1957b, 250; Hopkins 1957, 299). The economy is thus limited not in its substantive signification (that humans must produce and distribute the means of their livelihood) but in its independence and so in its distinctiveness and visibility as well. We can see the sum of these propositions concerning the theoretical indistinctness of the

economy—its embeddedness in direct social relations "of a general kind" and the normative gloss on those relations—in Habermas's observation that "in the nonmonetarized economic activities of archaic societies, the mechanism of exchange has so little detached itself from normative contexts that a clear separation between economic and noneconomic values is hardly possible" (1987, 163).

Consider these two illustrations of how the moral economic argument deploys the principle that before modernity much of the provisioning of human beings was embedded in noneconomic institutions and values. First the koinōnia, or community, encroached so heavily upon the lives of the ancient Greeks that "economic" phenomena such as investment, land acquisition, exchange, and trade were indissolubly ethical and political matters (Austin and Vidal-Naquet 1977, 113-14; Finley 1974, 32, 49; Polanyi 1977, 145–276). For example, the low level of productive reinvestment in Athens is explained by the demands of full-time citizenship and by the inferior status of those engaged in business; the limited market in land by the demands of community cohesiveness; and the hostile attitude toward trade by an anticosmopolitanism born of a fear for the solidarity of the community. Seemingly economic activities were not purely such but rather were infused with the demands of status, citizenship, and so on (Austin and Vidal-Naquet 1977, 9; Humphreys 1978, 137, 143-44; Polanyi 1957a, 79; Vidal-Naquet 1965, 136; Will 1954b, 17-18).4 The oikos, or household, with its noneconomic social gradations and purposes (both rooted in its sense of justice and the good) was the ideal type in the light of which the city economy was understood. The city's emporium, though something needful, was an object of suspicion and contempt, controlled and kept away from the heart of community: "Athens had need of merchants and slaves. But neither merchants nor slaves were Athens; they were an elsewhere of democracy, an elsewhere which made it possible" (Vidal-Naquet 1990, 15; see also Mossé 1983, 58–62; Vidal-Naquet 1981, 214).

Similar principles are applied by James Scott to the study of modern peasant villages. Scott's work focuses on the organization of these communities around the objective of securing the subsistence needs of their members. The peasant village is here said to be constituted so as to minimize risks to its members' (uncertain) subsistence. With that starting proposition, Scott's analysis could easily be cast as presenting the economic form of the village as a type of risk insurance, reciprocity and charity as devices useful for evening out the fluctuations of the family's food supply, and institutions such as the commons as a rational response to ecologically generated vulnerabilities (1976, 3, 6, 24, 29, 40–41). Yet Scott appears to reject this sort of material welfare explanation, opting instead for something closer to the moral economic argument. The economy of these villages, he says, is embedded in a moral universe in which justice (construed as the right to subsistence) is central, as is also the notion of a just price. It is this moral universe-

and the location of the economy within it-that accounts for "peculiar" village practices and for the performance standards imposed by the community on its wealthy members. There is, in other words, a moral expectation based on the right to subsistence that leads to redistribution within the community. The market challenges this governing right-to-subsistence ethos, together with its norms of reciprocity and charity, and in so doing brings on a revolutionary response (ibid., 4, 42, 167, 170, 176-77, 189, 192). In short, all important aspects of the village economyits use of land, patterns of redistribution, and ultimately its response to exogenous forces of changeare to be explained by reference to its being embedded in a noneconomic, moral universe of solidarity and the right to subsistence. From the moral economic vantage point, these otherwise disparate communities—the oikos and the peasant village—have in common the dominance of noneconomic norms and institutions.

I now wish to move to some of the key explanatory claims of the moral economic approach. I begin here by picking up the threads of the idea of the embedded economy. Central to that notion is the relative invisibility of the embedded economy. Invisibility means roughly that core aspects of the economy (division of labor, patterns of distribution, agent motivations and purposes) are so intermingled with noneconomic values and institutions as not to constitute a distinct theoretical sphere. More exactly, it suggests that because there are no theoretically visible boundary lines demarcating that realm from its surrounding social locus, "to speak of 'the economy' of a primitive society is an exercise in unreality. Structurally, 'the economy' does not exist. Rather than a distinct and specialized organization, the 'economy' is something that generalized social groups and relations, notably kinship groups and relations do" (Sahlins 1972, 76, emphasis original; see also Finley 1974, 49; Polanyi 1957a, 71; idem 1977, 44, 55; Vidal-Naquet 1965, 136; idem 1981, 211-221). What we take to be an economic category is there "charged with a plurality of unquantifiable, incommensurable functions, among which the economic function is never isolated and constituted as such"; consequently, it has a determination that "escapes the realm of the economy" and so cannot constitute a part of an economic discourse (Bourdieu 1979, 43; Tribe 1978, 33–36).

Thus when Socrates discusses oikonomikē (household management, or economics) with Ischomachos (in Xenophon's Oeconomicus), they converse about matters such as shirking and discipline, but in a manner that makes them inseparable from ethical questions of the good life, from gender-based divisions, and from a concern with rulership and politics. The concept of the division of labor provides a good illustration of this. To be sure, labor in the ancient economy was divided, its tasks distributed, but the principles and purposes governing that allocation indicate (to the moral economist) that it cannot be considered as an "economizing" practice. Principles dictating the allocation of function typically included

gender (e.g., the norm of *erga gunaikōn*, or women's work) and citizenship status, and its purpose was likely to be the creation of something more beautiful or the preservation of the appropriate order of the human community, household or city, rather than the production of more things or of things more cheaply—as in the division of labor in Adam Smith's pin factory (Finley 1982, 186–87; Hodges 1970, 216; Vernant 1969, 211) It is the institutional and value-embeddedness of the economy—the fact that it is lost in the totality of social phenomena—that renders it theoretically indistinct.

This (not, as Schumpeter maintained, the primitive and pretheoretical cast of ancient economics) explains the remarkable ethical-political quality of those writings. It invites the most basic questions of cultural interpretation: How are we to interrogate these societies about our word-concept the economy, which would scarcely be translatable into their idiom. Thus arises the debate with rational choice and similar approaches, for the moral economists hold that the cluster of concepts that together constitute the groundwork of modern economic theory are radically distorting when applied outside of market societies. Such applications, it is said, will produce a picture of institutions and behavior that is profoundly false, transforming persons into homo oeconomicus on the market model and their society into a prop for that economy (as it is in our age). What is in fact invisible will be given the illusory appearance of visibility: we will think we see in ancient Greece or in the peasant villages of the developing world something called the economy, with suitably motivated actors and with the institutions that sustain their activities. The moral economist's claim, then, is that modern economic theory, born of market society, cannot be used to analyze nonmarket communities (Dalton 1961, 1, 7; Polanyi 1966, xviii; idem 1977, 19-20). The reasons given are various but of a common stock. Scott, for example, argues that approaches that are too materialistic and individualistic, focusing on peasant budgets and goals, miss the vital component of the village economy, which is its moral vision, its sense of justice, its sense of subsistence rights, obligations to charity, and so forth. And analysts concerned with the ancient world maintain that economic concepts are singularly ill suited to understanding economic life in classical Greece because of the "heavy encroachment" there of the political with the resulting absence of an autonomous economic sphere (Finley 1965, 33; Scott 1976, 165-67, 188-89; Will 1954a, 209).

Polanyi, Sahlins, and others push these claims one step further: in so far as economic theory (and the economic approach to human behavior) rests on assumptions of economizing behavior—of scarcity-driven optimizing choices—it is for that reason, too, the product of our society, faithfully mirroring it but causing us to see nonmarket societies as if in a camera obscura (Bourdieu 1979, 1–7; Sahlins 1972, 2–39; Dalton 1961, 7; Pearson 1957, 320; Polanyi 1957b, 243–44; idem 1977). This amounts to the assertion that scarcity, surplus, and the economizing rationality that

they are said to induce, the "formal" idea of the economy (and other social phenomena), are not universals but historically and culturally bounded. The range of formal theory is thereby limited to one era, the one that first gave rise to it. In other times, there may have been no conception of available choice among means, the logic of maximizing calculation may have been displaced by the demands of solidarity, or the definition of the human condition as one marked by scarcity and the endless growth of needs may have been explicitly rejected (as it was by Aristotle and is, according to Sahlins [1972, 2, 13, 37], in the "original affluent" societies). Surplus, scarcity, and economizing choices are useful analytical tools only for societies that name them as such, where they can constitute an economic discourse and that, the moral economists tell us, has not been the case for much of human history (ibid. 2, 13, 37; see also Polanyi 1957b, 243; idem 1977, 29-30). What they call the "substantive" economy-human beings provisioning themselves in an ongoing interchange with nature—converges with the "formal" conception of society and economy only in modernity.

So far, we have seen the idea of the moral, or embedded, economy as it plays out along two dimensions; institutional and explanatory. Now I also observed that the moral economic argument has a third, normative, side. Indeed, the very appellation under which this group of writings is gathered points directly to that ethical dimension. Reserving a full discussion of these facets, I shall for now simply mention some of the normatively oriented properties that these authors attribute to nonmarket societies. Once more the idea of the embedded economy will provide us the guiding thread, and following Polanyi, I shall privilege its (Aristotelian) "householding" variant: "[Aristotle's] famous distinction of householding proper and money-making . . . was probably the most prophetic pointer ever made in the realm of the social sciences. . . . In denouncing the principle of production for gain 'as not natural to man', as boundless and limitless, Aristotle was, in effect, aiming at the crucial point, namely the divorcedness of a separate economic motive from the social relations in which these limitations inhere" (Polanyi 1957c, 53-54). What draws the moral economist to this idea of the household is already suggestive of a normative and critical intent, for the household is seen as a nonmarket community, antiacquisitive, the "highest form of economic sociability," in which "man [is] the end of production" (Sahlins 1972, 76, 84, 86, 94; see also MacIntyre 1984, 227).

One ready way to construe that valuation is that in the embedded economy, the production and distribution of the means of human livelihood are subordinated to the pursuit of the good life broadly understood, a subordination that expresses the economy's governed character. The invisibility of the economy and of its characteristic ends is an index of this governance and of the extent to which these other goals dominate. The latter goals might include Aristotle's notion of the good life, the possibility of which

presupposes the containment of provisioning activity; the bonds of community against the atomism of market society; Scott's moral universe of the peasant village's subsistence rights and corresponding sense of justice; Thompson's related observations on the English crowd; and altruism and gift economies (Mauss 1967, 66; Polanyi 1957a, 71; idem 1977, xlvii; Scott 1976, 5, 13, 33, 42, 167, 176-77, 189; Thompson 1971; Titmuss 1971). What unites them is the thesis that these humane attributes are a function of the subordination (embeddedness) of the economy in premarket societies. This estimation is sometimes qualified, as in Scott's warning against idealizing village practices and Polanyi's observations on gender-based divisions of labor and the importance of status in premodern communities. Even with these reservations, however, there is no mistaking the conclusion that for the moral economists, the move from Gemeinschaft to Gesellschaft entailed a loss of a certain vital human quality that typified earlier societies. As Evans-Pritchard observes, Mauss and others are telling us "how much we have lost, whatever we may have otherwise gained, by the substitution of a rational economic system for a system in which the exchange of goods was not a mechanical but a moral transaction" (Evans-Pritchard 1967, ix).

THE WORLD OF THE MARKET

Central to the moral economic account is that the transition from the embedded economy to market society marked a radical watershed in human history: "[We] go into Gesellschaft as into a foreign place." (Tönnies 1979, 4). Polanyi argues that nothing comparable to market society has ever before been witnessed. Of course, markets predate modernity, but for the most part, they were marginal, often heavily administered. In domestic markets, we still find the dominance of nonmarket institutions and relations. Thus exchanges within the community are governed by the same "general social relationships" that mold that association as a whole: the exchangers are not equal, commodity-owning ciphers but members of a particular household, male or female, young or old, and so on, with each attribute carrying a social meaning that bears upon all their transactions. Markets did not integrate society or even that part of it taken up with provisioning human beings (Dalton 1961, 9; Polanyi 1977, 7; see also Evans-Pritchard 1940, 91).6

Till now we have discussed only the embedded economy. In the following pages, I want to move into some of the institutional, explanatory, and normative detail of the "great transformation" that modernity is said to have wrought. At the core of this idea of a watershed change are two related claims: (1) that in modernity, the economy is disembedded, that is, it is an autonomous sphere governed by laws of its own and (2) that the disembedded economy presses outward, tending to become integrative not only of those domains concerned with our livelihood but of society

as a whole. The first of these ideas is the institutional thesis, fundamental for the moral economists, of the autonomous, "self-regulating" character of markets in modernity (Hopkins 1957, 299; Polanyi 1957a, 68; Polanyi 1957c, 41, 71; Polanyi 1977, 10, 47). That, they argue, is the most striking feature of modern markets—and so it must appear, given their background theory of the embedded economy. For the first time in human history, the economy is not located in the enveloping and determining noneconomic context of the *oikos*, the city, the royal household, and so on. Institutionalized in the self-regulating market, it breaks free from the embrace of the sovereign community, becoming autonomous and therefore purely economic.

The second component of the idea of the disembedded economy is that of the market as pervasive. We saw that the moral economist holds that earlier societies were integrated through noneconomic institutions. Characteristic of modernity on the other hand is that the market threatens to become the dominant mechanism integrating the entirety of society (Polanyi 1957c, 57, 69; idem 1966, xvi; idem 1977, 9). The magnitude of the market's reach can be measured by the extent of commodification, by the range of goods subject to commercial traffic. In particular, the case for the pervasiveness of the market in modernity rests on the commodification of land and labor. When nature and persons, the world and human activity, are made into objects of sale—when that is the dominant mode for their transaction—the remaining barriers to marketization must be frail indeed. The former world is turned inside out, and now a whole society becomes embedded in the self-regulating mechanism of its own economy; the economy envelops society, refashioning its ethos and relations after its own image.

Unlike its predecessors, the disembedded economy is theoretically visible. Thus with modernity was born the concept of the economy (Polanyi 1977, 6). Before our age there was no "separateness" to much of the economy, no condition of existing apart from politics or other noneconomic institutions. Consequently, it could have no separate phenomenology. With the emergence of the disembedded economy as an autonomous sphere and with institutions, motives, and laws that are specifically economic, a lawlike and independent science of the economy construed as the "allocation of material goods to satisfy material wants" becomes possible for the first time (Jonas 1958; Polanyi 1977, 12, 47). But something else happens as well, for with market society comes the possibility of economics extending its explanatory range beyond the economy as just defined. Drawing on the "basic analytical tools" of rational maximizing behavior, markets, and prices, an economic approach to the whole of human behavior becomes feasible, one that makes visible the "implicit economic structure" of even such seemingly affectional communities as the family: monogamy is understood as "the most efficient marital form," and children as commodities 'presumed to have modest price elasticities because

they do not have close substitutes" (see Becker 1974, 300–301, 305; idem 1981, 106; idem 1976, 5–8, 14; Coase 1978, 207–8; idem 1988, 1–3; Posner 1983, 1, 3, 5).

With society embedded in the self-regulating market, economic society and homo oeconomicus are not distorting overlays but rather the most adequate expression of modernity. In a mirror image of the premodern world, the economy, wrenched from its (former) embedded location, merges once again with society, now not because of its saturation by the noneconomic norms and institutions of the community but as a result of its very pervasiveness, a condition captured in the phrase "market society." Assumptions concerning the pursuit of material gain, calculation, and a certain atomism or selfishness are privileged by this approach—and rightly so, for market society does generate ever new wants, giving the impression that there never can be a state of sufficiency, and induces economizing behavior of just the sort depicted in formal theory. But the formal or economic approach goes further, on this account of it, and argues that in all times and places human beings face and act on the basis of scarcity constraints that engender economizing behavior (Dalton 1961, 4-5; Pearson 1957, 320, 330; Polanyi 1957b, 244; idem 1968, 64; idem 1977, 19-25). The pretension that these concepts are of universal applicability is, as we shall see, a major part of what the moral economist faults it for. Here-but here alone-that approach has explanatory power.

The moral economist's reflection on modernity has its institutional and theoretical dimensions, but it is powerfully motivated by evaluative issues as well. What Evans-Pritchard says of Mauss speaks also for the approach as a whole: "[He] is asking himself not only how we can understand these archaic institutions but also how an understanding of them helps us the better to understand our own, and perhaps to improve them" (1967, ix). What sort of society, the moral economist asks, would be amenable to such an explanation? A society in which the economy but not the community is self-regulating; in which contract becomes the cement of an atomized world; in which no ends other than (market) economic ones are pursued. Once more, the key notion is the disembedded economy, understood in the first instance as self-regulating and therefore purely economic. When the moral economists assert that the modern market is self-regulating and all-embracing, they mean, among other things, that it has escaped the control of the community. That opening move in the moral economist's normative argument can branch out along an array of critical avenues, including: (1) a quasi-Marxian critique of the autonomy of the market as a source of unfreedom; (2) a Gemeinschaft-type, communitarian sense of the loss of solidarity in market society and its replacement by an economically generated atomism; (3) a critique based on the idea of the good and its radical displacement in market modernity. Let me say a few words about each of these.

Critique 1. We saw that the notion of the self-regulating market was central both to the moral economist's description of the characteristic institutional arrangements of modernity and to the claim that only in our era is a lawlike account of strictly economic phenomena possible. More radically still, market society makes the entirety of human behaviors and institutions available to the economic approach. In earlier times, the economy was an instrument of society's purposes—the subsistence of its members or the possibility of a good life. It was a tool and, like all tools, subordinate to its users' ends. Today, conversely, it is the economy and its self-generated ends that govern and with a power such as to transform society's order and purposes and to substitute for them the logic of calculation and maximization. Not only is it extraordinarily thorough in dissolving the traditional community and its bonds, but its sway is so great that it reaches down into even the micro level, inducing choices and behavior consistent with its ends. Never has an economic system compelled individuals so universally and with such necessity (Wolf 1971, 278-79; Polanyi 1957c, 73; idem 1968, 62-63). The market, in short, extends the reach of the economy in controlling individuals, and that in turn is made possible by its disembedded, self-regulating quality. One measure of this transformation is the array of perverse effects attributed to the pervasive market, for example, effects on subsistence, labor, and the environment.8

Critique 2. There is a group of ideas broadly related to the Gemeinschaft view of modernity as the withering of community, an argument associated with the writings of, among others, Tönnies.9 Here the critical anchor is the concept of market society as "economized," as one in which the relations and goals associated with the acquisitive economy triumph over higher ends and more human bonds, for among those things that have been lost in the great transformation is the tightly woven fabric of community, the mesh of obligations and expectations that overrode self-interest and made each person a part of a greater whole, whether as a member of a household or kinship group or as a citizen. In its stead emerged an atomized society in which the interdependency of individuals was not mediated through political, social, or religious institutions but via the market and contract. The noneconomic bonds of the world before modernity were supplanted by those forged from economic self-interest, creating thereby something very much like the community of traders, the rootless, floating world of the cash nexus, so much despised in ancient societies (Mauss 1967, 63-81; Wolf 1971, xviii, 279, 282-83; on atomism and individualism, see Polanyi 1957c, 163).

Critique 3. The contrast between economic and other (presumably superior) goods is suggested by the dominant motif of the moral economic approach, that is, the juxtaposition of economized modernity and the noneconomic premodern world. Need, fear, and

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acquisitiveness are the forces that here govern behavior to an extent previously unheard of: everything and everyone must be viewed as a possible object of gain or as a competitor for goods. Little room is left thereby for purposes other than those stamped in the mold of the market. The market, in other words, through its extensive presence and its capacity to structure choices, creates a situation in which pleonexia, the ancient malaise of the acquisitive soul, becomes a systemic property driving out goods other than its own. From that vantage point, the moral economic history of modernity can be read as a reaction to these perverse effects, as a struggle between the market and society's efforts to regain governance, to assert goods other than those of the self-regulating market. For the moral economist, modernity as market society is virtually from the moment of its birth a contested form, to be queried in its theoretical expression (the economic approach to human behavior) and challenged politically. The former we see in the work of the moral economists themselves and above all in their efforts to keep alive the memory of an economic order different from our own; the latter is evident in peasant rebellions against the encroaching market-rebellions sometimes seeking the preservation of the structures of traditional society and at other times venturing novel forms of the embedded economy (Wolf 1971, 282). It is also expressed in the emergence of both socialist politics and antimodern movements on the Right and in the creation of institutions that reassert society's governance over both the production and distribution of the means of its own livelihood and the consequences of the market for its other concerns. Such projects for reembedding the economy flow naturally from the picture of market society as ruled over by a disembedded, self-legislating economy coupled with the critical estimation of that society. They are the fitting terminus of a theory that offers us an account of nonmarket societies, not out of antiquarian or anthropological interest alone but as pointing to alternative ways in which the economy could be integrated into society. Perhaps because the idea of the moral economy has in our times been developed, for the most part, by anthropologists and others for whom the most pressing task was the Methodenstreit, the normative facets of their argument cannot be unfolded directly from the texts we are considering. I therefore postpone my discussion of it until the concluding section of the essay, where I draw on other writings related to this tradition.

EVALUATING THE MORAL ECONOMY

I now want to turn to an assessment of the moral economic approach, then to a reconstruction of its normative center. First, as even their critics are prepared to allow, the moral economists' most important achievements are their underscoring that markets have not always been the dominant institutional mode of economic transaction and their offering of an

alternative typology of economic regimes (Hechter 1983, 173; North 1977, 706). The challenge to the moral economic school turns around the explanation of these nonmarket forms. Polanyi, we recall, argued that these societies resist analysis in terms of scarcity and an economizing rationality, a form of analysis that must inevitably dislodge their economy from its informing social nexus. This claim faces a number of competitors and counters. One maps a theory thatwhile rejecting an anachronistic employment of theories rooted in contemporary society—nevertheless accords primacy to economic forces broadly construed in the explanation of nonmarket societies. Marxism is one such possibility, and it employs the familiar conceptual tools of technology, scarcity, classes, states, and exploitation (see North 1981, 61–63). A second approach begins with the presence, even in market society, of institutions whose internal working are not marketlike (e.g., firm, family, government). A theory that can account for that presence may also be of use in explaining entire societies organized in a similar manner. On this view, the existence of nonmarket economies, contemporary or historical, may be understood in the light of the transaction costs associated with markets. Because of enforcement, measurement, information, and other costs, markets may, under certain conditions, simply be more expensive than nonmarket regimes. This approach is also able to provide a causal story for economic regime change as a consequence of (among other things) altered costs. 10

And that points to one of the major inadequacies of the moral economic model, namely, that it has only thin theoretical resources with which to explain economic change—why economic regimes come into being and pass away. In particular, it is hard to see what non-ad hoc explanation would be used to account for the emergence of markets out of embedded economies. And while it certainly offers an explanation for the demise of the entirely unfettered market, it does not tell us the causal story behind the failures of the "invisible hand" (Godelier 1974, 1371-72; Hechter 1983, 183, 187; North 1977, 715). The moral economic approach is, in other words, oriented toward the static state and is therefore less valuable when the question is of change across time. That in turn awakens the suspicion that its account even of stable states may be inadequate, for if it is unable to explain their demise, it may well not have an adequate understanding of the sources of their stability either.

Let me pause here to focus on the extension of the transaction cost approach. This is a natural target for the moral economist, who might answer that in using the theory of the firm as our foundation, we are led to read back into the premodern world a sort of implicit market made up of just those kinds of institutions and motivations that were virtually absent from it, for the manager of the firm can be seen as driven by market-generated imperatives to regulate the internal workings of his organization in the most efficient manner. And because that behavior-inducing struc-

ture is absent in nonmarket communities, theories of the firm will give a misleading picture of the foundations of the premodern rejection of markets, making them appear more economic than they in fact were. This retort offers a useful cautionary note, telling us that we must attend to the institutional specificity of the economy and to how it is understood by those for whom it is a part of their enveloping horizon. In short, it tells us that both in its being and in the type of explanation it calls for, a firm whose command-type hierarchical arrangement is shaped by market pressures is something quite different from, for example, the redistributionist kingly households of the archaic Mediterranean world or, for that matter, a modern family.

But perhaps the moral economic argument adopts too narrow a conception of the economic and economizing, which, in turn, is the result of its attempt to deny the choice-theoretic approach access to nonmarket societies by challenging the usefulness of notions of scarcity, scarcity-induced choice, and so on. To say that market-type scarcity, surplus, and the sorts of behavior that they help to structure are largely absent before modernity is, even if accurate, not to prove the case that economizing behavior does not exist there at all or that it is not an important explanatory device for understanding those societies. The economic approach may well provide an interpretative key to such societies so long as it is attentive to their defining institutional and motivational differences (Bates 1983, 139–40). 11 Scarcity, surplus, and economizing are wrongly defined if they are restricted to their market form and if that is the barrier to understanding important institutions and behaviors in nonmarket societies as economic at their root, then what is needed is to go beyond the too-pinched conception of the economic offered by the moral economists (Hechter 1983, 165n; LeClair 1962, 1180-1185; Smelser 1959, 176–77).

Indeed, when we consider core aspects of the behaviors they put forward as illustrative of the noneconomic character of these societies, we see the extent to which they lend themselves to retranslation into the vocabulary of the economic approach. 12 Take Scott's argument that peasant villages are moral economies because their governing objective is not individual maximization of wealth but the protection of the community as a whole against a collapse of subsistence. The end is "moral" and so, presumably, inaccessible to economic reasoning. Yet if we look at Scott's analysis of these villages, it becomes apparent that many of their behaviors and institutional forms are economizing responses to a situation of severe risk in regard to subsistence goods. They may well subscribe to the proposition that justice demands that all be preserved from hunger, but it is the scarcity of means to ward off this ill that produces their economizing decisions, including their resistance to the intrusions of the market with its threatening fluctuations and unacceptable distributive outcomes. Alter those scarcity constraints (through, e.g., state-led social assistance programs), and the structure of the village becomes redundant. The village, in short, is governed by risk aversion (a stance, Scott notes, not absent from capitalist corporations), and it economizes around its principal constraints. The specification of the end is, to be sure, a matter of "moral vision," of a certain perception of justice. But given that end and the scarcity situation that confronts the village, the resulting choices are amenable to economic analysis (broadly understood), as also is the study of their unintended consequences. Scott himself seems virtually to invite this sort of retranslation, saying that "peasant economics is better understood as a special case of what standard microeconomic theory would predict" (Scott 1976, 14).¹³

Alternately, we might consider that other focal point of the moral economic argument, classical Greece. Here again, the economy and its typical behaviors are arguably quite visible: scarcity and economizing are fully in evidence, though located in a nonmarket context. To see this, we might reflect on one of the most extensive ancient discussions of labor discipline and the division of labor—that set out in Xenophon's Oeconomicus. Socrates wants to learn how Ischomachos, a wealthy estate owner, has acquired the means to be able to engage in the affairs of his city and friends. In other words, how has Ischomachos arranged it so that he is not bound down by penury or excessive need? Ischomachos is not seeking to maximize wealth without limit but rather to be leisured, to find a way to escape from economic activity altogether. Much of their discussion centers on the managerial role of the wife in the oikos economy, the need for overseers, and (in an ancient version of the principal-agent problem) the issue of how to motivate the wife and overseer to do the master's work efficiently. The Oeconomicus, then, abounds in discussions of shirking, discipline, and motivational problems within the household that would not seem out of place in a modern text, with one exception. What leads Ischomachos to reflect carefully on the internal organization of his economy, to employ his work force most efficiently, is an ethical-political conception of the good life that requires him to maximize one very scarce good—time for the sake of a life given over to free praxis. Scarcity, maximizing, choice of technique: all can be discerned in that distant conversation between Socrates and Ischomachos, and we should not be blinded to them by too cramped an understanding of the economic as identical with its market expression.14

Still, even if it seems that this issue can readily be brought to closure (as it arguably can if the moral economist's case rests solely on an excessively restrictive understanding of the economy), there nevertheless remains a concern that in this sort of translation of the idiom of the nonmarket world into our own, it may happen that "things get lost"—perhaps some very important things (Geertz 1983, 10). My tone has suggested that we could readily speak about, say, Ischomachos's maximizing rationality as expressed in the use of his wife and servants. But now turn the table and consider what Ischomachos would have

answered to a query about his oikonomike, his economic prowess. He might well have explained it as part of the "total social phenomena" of his household—tied it to his status as a citizen of this Athenian polis and to his being male and of a certain age. Were we to answer him, "No, we mean tell us about your economic skills," his response would be the same, and he would be bewildered at our pressing him to hive off whatever this "economy" is from the rest of the relations of his oikos and its place in the city. Or, still within the "total social phenomena" of his world (i.e., looking from within its ethical and political architecture), he might, like Odysseus take offence, thinking that we were calling him a trader, one of those marginal people, the metics, whose only fatherland is profit, and who therefore are the "elsewhere" of the city (Odyssey 8.158-68). Evans-Pritchard's Nuer would probably be similarly perplexed were we to speak about exchanges apart from (for example) kinship ties. The (arguably) jarring and counterintuitive quality of the account of the family as a community of traders intimates something of this loss, as (on a wider canvas) does Émile Benveniste's observation that "everything having to do with economic notions is bound up with much greater representations that call into play the ensemble of human relations, or of relations with the gods" (1969, 202).

Would Ischomachos' and the Nuer's perplexity be diminished if we were to inform them that what we meant by economic was a "science of human choice," of choices made by rational, self-interested maximizers-if, that is, we replaced the cramped concept of the economy by a more expansive, choice-theoretic approach? For we who live in a world suffused by calculation and by a rationality disposed toward a future as a realm of possibilities to be exploited, such a view of choice may come as naturally as the air we breathe. But in the moral economic account, that is a disposition "insensibly acquired . . . and taken for granted," which is peculiar to us and foreign to members of other civilizations for whom these specific ideas of the future, time, and rational calculation are largely absent (Bourdieu 1979, 4-8). What these ambiguities illustrate is that this part of the debate virtually calls out to be carried one step further—into fundamental questions of language and interpretation across cultures addressed to the proponents of the economic approach. Anthropologists, drawing on (or paralleling) Wittgenstein and Winch among others, have already traveled down much of this road. In thinking about the interpretation of the economy, about rational choice and related theories, we might well profit from their reflections—not necessarily from their conclusions, but at least in their questioning and challenges.

How does the idea of the premodern embedded economy provide a critical fulcrum against market society? As important as the normative valuation of nonmarket societies is to the moral economic argument, it is very difficult to get a purchase on it, because, as I have said, it is even more thinly developed in this literature than is the basis of their re-

pudiation of market society. A promising starting point, however, is the idea of embeddedness. This notion contains a normative clause grounded in the claim that in nonmarket societies it is the community that absorbs the economy—directs it or infuses it with its values—rather than, as in modernity, being ruled by it. When we unfold the notion of the economy embedded in or under the governance of the community, what we find is the "domination moment" as central to the community and the encroachment of status and other forms of hierarchical gradation upon society and economy (Brunner 1956, 43-44; Finley 1974, 49). These aspects are not mere scarifications on an otherwise noble edifice but are crucial to the workings of many of those communities and to the shape of the economy within them. While similar properties have been observed in some peasant communities (see Bates and Curry 1992; Popkin 1979, 28, 61), I shall focus here on the concept of embeddedness in ancient Greek society.

We might begin by observing that the oikos, or household economy, literature was, at its birth in classical Greece, bound up with a critique of both markets and democracy for their indifference to the rank ordering of persons and of the goods that ought to be pursued (Humphreys 1983, 12). By contrast, the embedded economy was located in the koinonia—in its affective bonds, in its proportional equality (hierarchy)—and was subject to its ruling ends. In this world, the economy did not disrupt the rank order of human beings but rewarded each according to his worth. Persons there are not the ciphers they will become in markets but individuals whose different worth shapes exchanges between them (Finley 1974, 34; Godelier 1986, 231). Embedded also means, in this context, that each does what is proper to his or her status-male and female, freeman and slave, adult and child. And it signified the tight solidarity of the community against the universalism of commerce (see, e.g., Aristotle, Politics 1258a39-1258b8; Demosthenes, Private Orations 37.52; Lysias, Orations 22.14, 31.6-7). Finally, embedded conveyed the sense of "subordinate to the proper ends or purposes"-not the Midas or animallike passions of the many, not the "cloak of many colors" (as Plato in Republic 557c mockingly calls the diversity of goods under democratic regimes) but the pursuit of ta kala, the beautiful life made possible by detachment from provisioning activity, a detachment secured in its turn by domination over others. Both of the two defining elements of the ancient idea of the embedded economy, as located within the framework of the community and made instrumental to the pursuit of the good life, are suffused with hierarchy—the good rank-ordering of ends and persons. The location of the economy—the form of its embeddedness-would have been incomprehensible to them without that cosmos. What the moral economist calls the disembedded economy of the market was condemned (as was democracy) from the standpoint of the oikos (and the best regime) for its egalitarianism and indifference to rank order, its neglect of the best life, and its openness to something

outside of the tight bonds of the community's hierarchical solidarity.

We may suspect, then, that the moral economic reading of the embedded economy is colored by a certain romanticism (Godelier 1986, 181; on a similar temptation in communitarian political philosophy, see Kymlicka 1991, 85-90; Humphreys 1978, 38). Were this simply, in Habermas' wonderful turn of phrase, "the melancholy charm of irretrievable pasts and the radiance of nostalgic remembrance of what had been sacrificed to modernization" (1987, 329), then the moral economists' starry-eyed appreciation of the world before markets would be less troubling. But they mean to use that world as a mirror in which we may more readily see the flaws of our own, and in the course of doing that, they abstract from its particular binding cement, from its hierarchy and domination. In other words, they seem to have put to one side their insistence on sensitivity to the historical and institutional context. Yet embeddedness is hardly intelligible apart from that context, and once it is extracted from its specificity it loses much of its conceptual substance, becoming an ill-defined, if seductive, perception of a "thick" community. And as a countermodel to the market, that world sheds its lustre the moment its mode of embeddedness is unpacked. By leaving unsaid how the economy was subordinated and contained, this approach also causes us to miss something crucial about the theory of market society, namely the extent to which it too is a moral economics, developed in response to the embedded premodern household model.15 I now want to examine critically the notion of the disembedded market and its normative implications.

As we saw, the governing juxtaposition of the moral economic argument—the embedded economy as opposed to the self-regulating juggernaut market—also yields a picture of market society ruled over by its disembedded economic institutions. There is, however, something misleading and false in this account—at least as misleading as its corollary claim that there is no recognizable economic behavior in premarket societies, for surely the market is embedded in both significations of that term: institutional and normative. The institutional context is evident and I shall not dwell on it here. It consists, among other things, of a mesh of property rights made operative in the form of law, of contracts and their enforcement and so on. The market, in other words, flourishes only amidst an array of "external" institutional supports. 16 When it is said that markets are institutionally disembedded, therefore, it is unclear what precisely is meant-except, perhaps, that they are not (wholly) regulated from above, as if by a very visible hand. That last way of speaking of their embeddedness permits us to see their ethical locale, for it suggests that we can think of markets as being one institutional outcome of the normative shift from status to contract. I shall develop that thought in order to suggest how the embedded/disembedded language may easily be seen as misconstruing the normative character of modernity.

The mode of producing and distributing the means for human sustenance embodied in the market is expressive not of a human propensity to truck, trade, and barter or of the desire to acquire ever more things but rather of a moral redrawing of the community and of the place of the economy within it. What that transition yields is a new form of moral embeddedness for the economy. The attributes claimed for it are familiar: an economy whose actors are considered equals and a system indifferent to their noneconomic attributes; a contractarian, voluntaristic institutional context for exchanges; and the view that the public authority should not decide among preferences—that one is entitled to live one's life "from the inside," selecting and ordering one's preferences according to the good as one understands it and seeking to engage the voluntary cooperation of others in one's pursuit of them. At its foundation, this redrawing involves a shift away from the patriarchal household model, with its hierarchal and unfree core. To say that the "world we lost" was characterized by the subordination of the economy to the noneconomic values and norms of the community whereas market society has freed the economy from those moorings is to misrepresent the meaning of modernity, a meaning that has to be located in no small measure in a rejection of the household model in the economic sphere and its close analogues at the political level and in an attempt to fashion a new community and economy. 17

Consider, as an application of this argument, that in market society, labor and land have been made into commodities, a proposition that is intended to show the degree to which the self-regulating market has taken hold of-and economized-society, even down to the level of such "fictitious" commodities as nature and human activity. A more natural construction of this process of "commodification" is that land and labor are removed from their status-determined, nonconsensual location in the social hierarchy. Land is freed from its attachment to patrimonial lineage, with (as Tocqueville observed) the resulting democratization of the agrarian economy. Labor is taken from its servile status (determined by gender and other ascriptive factors) and made a part of the self-ownership of the person, something to be freely disposed of. Viewed in that light, it is once again inaccurate to characterize the "great transformation" as one from a society in which noneconomic, human values ruled to a modern order in which the primary determinants are the all-embracing and purely economic laws of the juggernaut market. There was indeed a great transformation, but it might be better expressed as a move from a community and its economy (heavily encroached upon by hierarchy and status) to a society in which a certain equality and autonomy were accorded primacy.

A similarly contestable reading is at work in the way the embedded/disembedded distinction is used to weave the moral history of modernity as the battle between the self-regulating juggernaut market and a society trying to impose a certain humaneness upon it. A less tendentious reading is that voluntary (con-

tractual) relations among persons, whether in economic or other matters, represent a central good in liberal market society and that therefore any interdiction of what is voluntarily agreed to among persons must be taken, in the first instance, as a prima facie evil. However, this is not to say that voluntary agreements are the only good to be secured, at whatever cost to other values society may wish to advance. It is to assert, however, that the interdiction of freely made agreements must be understood as a normative loss and weighed accordingly against the expected benefits. The many ways in which society now regulates contracts and markets represent not a reembedding of a purely economic sphere but the seeking of a balance among competing goods, one of which is the importance attached to the market as a forum composed of voluntary contracts that can, despite their importance, also on occasion be restricted so as allow for other goods to be realized. We could say then that what the moral economists call the reembedding of the economy is better explained as the workings of a society possessing a plurality of goods. In Michael Walzer's terms, the market is one sphere of justice, and when its operations "abut" on different spheres with other principles of justice, society may decide to limit its effects by regulating it in various ways (Walzer 1983). The embedded/disembedded conceptual framework obscures the character of market society by simultaneously understating the presence of recognizable and distinct economic behavior in archaic societies and insisting on too radical a detachment of the modern economy from its sustaining institutional and normative nexus.18

THE MORAL THEORY OF THE EMBEDDED ECONOMY

Here is roughly where we stand: the moral economists are committed to a theory of the radical exceptionalism of market society, a view that entails (so they maintain) that the mode of explanation of modernity must be similarly novel. Some are also given to the use of premodern (householding) models—the classical Greek especially (half of Polanyi's last major work, The Livelihood of Man, is devoted to ancient Greece)—as part of a critical apparatus employed against the world of the market. For reasons already suggested, I take the latter to be largely unsustainable in the late twentieth century; the former is a valuable invitation to further exploration of the issues surrounding the interpretation of economic forms. While acknowledging the difficulties to which the foundational embedded/disembedded contrast gives rise, what I wish to pursue further is the claim that far from being unserviceable, this approach suggests a powerful (and debatable) way of thinking normatively about the economy.

At the center of this part of the reconstruction of the moral economic argument is the idea of the embedded economy, and while, as I just said, that

idea is not uniquely specified by the particular structure of the classical Greek oikos, the latter is nevertheless a good starting point. Because in linking oikos and oikonomikē, "household" and "economics," it intimates another understanding of the economy as located in the mesh of family (or community) relations and purposes. 19 We can approach this idea by unfolding it into the two parts of the meaning of Aristotle's idea of the economy en tais koinōniais, "in the framework of the community." The first suggests that the provisioning of human beings is embedded or submerged in a nexus of relations—relations of justice, if you will. For example, on certain holidays, members of families give gifts to one another, and those exchanges are embedded in the framework of their community—their religious beliefs, their norms of deference and of reciprocal obligation between parents and children and so forth. By the same token, in a political community of equals who prize freedom above all, relations of distribution and of production will adapt to that overarching pattern. So, likewise, in a society that recognizes deep inequalities among its members, the economy will most probably take on yet another form. The economy, in short, is suffused by the norms of the community of which it is a part, and we would expect that where divergence occurs either economic relations will be refashioned so as to bring them into harmony with the community or the latter's values will, in a process of reflective equilibrium, undergo change. In this sense, and contrary to Polanyi-type arguments, all economies, including the near-topervasive-market economies, are moral economies, embedded in the (ethical) framework of their communities.

A second (and related) way of unpacking the concept of the economy en tais koinoniais is as a part of the view that all communities aim at some good, the perception of which informs the life of that association in its many and varies aspects (Aristotle, Politics 1252a1-5). We have just seen one manner of speaking about that claim, and the language of aiming or "being directed to" suggests another. From that perspective, the economy is understood as instrumental to the good, and that claim marks the beginning of a teleological inquiry about the good and about the place of the economy in relation to it. Here is a schematic version of this aspect of the classical theory as it proceeds from the starting point of the household. Wealth is an instrument or tool: Aristotle speaks of "wealth . . . and instruments generally" and again of property as "tools for the purpose of life," and Xenophon has Socrates discuss "the instrument wealth" (organa chrēmata; see Xenophon, Oeconomicus 2.13; Aristotle, Nicomachean Ethics 1987a28; idem, Politics 1253b31-32). Like all instruments, it has an end or purpose external to itself and in light of which its worth is judged. Wealth not governed by such ends does not constitute a good for human beings, because it has lost its telos-directed nature. Instead, it becomes a dead weight, a thing to be borne but that does not serve its proper end; or worse, it causes that peculiar corruption of the soul, pleonexia, the unlimited desire for things, a Midas-like passion that (as was the case with Midas himself) deprives those whom it afflicts of the ability to use properly the things they have.

So knowledge of the purpose that wealth ought to serve is essential to its character as an instrument in the service of the good—essential, in other words, to its being wealth at all. We do not recognize wealth as wealth (much less know how to organize and deploy it) before we have answered the query what end it serves-and not what derivative ends but, rather, what highest end for the person or the community. A plow is an instrument, and the slave is a "living tool" in the service of the oikos master, as also are his wife and overseers (Aristotle, Nicomachean ethics 1161b3-5; idem Politics 1253b24-54a8, 1255b30-40; Xenophon, Oeconomicus vii. 10-17, xii.12-20). They create material things for him (food, clothes, etc.) and by carrying the burden of direct production and supervision allow him time for higher activities. Yet none of these are wealth (at least not yet), for they await an answer to the question of how they are to be used, of the final good in relation to which they are but so many instruments. The person who is truly a good economist, who possesses oikonomikē, is the one who can answer that question. It surprises us perhaps, but not them, that Socrates, poor, pale, and shoeless, should lead Xenophon's discussion of economics; for he was, after all, "the chief of helpers in the quest of virtue" (Xenophon, Memorabilia 4.8.11).

Any human community—the household or the city-has thus to ask itself this primordial question concerning the location of the economy in its hierarchy of goods. Not to subordinate the economy to those rank-ordered goods is, in a basic sense, to render the community incoherent from the point of view of justice—to cause perverse distributions of things and to allow wealth itself to become the chief good. Classical Greek philosophers saw this subordination as directed to the master's leisure and praxis and those in turn to the possibility of philosophic or citizen activity. The classical city, for its part, intervened in the economy for the sake of securing food supplies but also to allow for the participation of poor citizens in democratic institutions.21 In both cases, the embedded economy could be seen as the practical conclusion to an inquiry concerning the end or purpose of economic activity. Economics as the science of wealth-its acquisition and employment-must be guided by the question of wealth's purpose or render itself radically incomplete. It presupposes another science, a moral or political one, just as the object of its study, the specific institutional form of the economy, presupposes our common answer to the question, "To what end?"

The answers ventured by the ancients to this question, a rentier life for the *oikos* master and for the city, are now museum pieces, valuable primarily as a warning against romanticism. However, as I have just suggested, it is possible to tease out of those arguments the materials for an alternative framework

within which to think about the economy. Because that framework makes economics—unavoidably and at the outset—an ethical inquiry, it is a worthwhile antidote to what Amartya Sen has called the "engineering" approach to economics, an approach that seems at times almost to revel in its nonethical character (Sen 1987, 2). For this framework points to two related paths, reflecting the dual types of inquiry that emerged from our analysis of en tais koinoniais. In the first place, it says that economic science is incomplete if it does not embed its studies in an overarching account of the location of the economy in the wider architecture of the community, of which the economy is simply one moment. Second, it tells us that this overarching account must be centrally informed by the question of the good to which the economy and its sustaining institutional nexus are, or ought to be, subordinate. In so doing, it directs us beyond the language of rights, self-ownership, and in general the liberal way of framing the normative study of the economy, for the question "To what end?" leads us to see the limits of a theory grounded in exclusionary rights and neutrality, a theory that (from the standpoint of the moral economy) neglects the most basic of queries as to what good is served by the economy. Taken in its entirety, this theory insists on the study of the community and its institutions grounded in the ethos that invests them with their life and form as central to an adequate and complete account of the economy. It insists, in other words, on the economy as something that we do, that involves us in certain patterns of relations with our fellow human beings and with nature, and for a purpose or end. The question "What good is thereby served?" is essential to understanding what we do when we do the things called economic—much more essential than grasping the most efficient way of doing them.

I said that the institutional and teleological senses of en tais koinōniais are related, and that relationship is evident in what we have just discussed. The end the good aimed at-informs the institutional structure of the community, including the character and location of the economy within it. But the householdmodel teleological theory that we have been considering does not simply want to map out a (normative) form of explanation. Its interrogative path defined by the question "For what purpose?" and emphasizing the instrumentality of wealth in the light of that question was meant precisely to call into doubt the market and democracy as forms for the economic and political integration of the community and to challenge them as ways of life (see Booth 1994; Humphreys 1983, 12). We have already spoken of one dimension of this. The good oikos represented a regulated economy, hierarchically organized according to the respective worth (proportional equality) of its members, husband, wife, children, free servants, and slaves and subordinated to a sovereign good. The world of the emporium by contrast was anarchical, indifferent to the community and its order, and bound together by the pursuit of money alone. Democracy, like the market, is blind to the necessary

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closedness of community (Plato, Republic 563a) and to all but a narrow and partial facet of the good.

We can generalize this still further by observing again that that type of inquiry was intended to display the (related) limits of the market and democracy. This it does by driving the question of the good towards a telos, a ruling good (or goods).²² Whether by the dramatic and ascending path of Socratic dialogue or in the more mundane Aristotelian variant, conceptions of the good are challenged and undermined until that terminus is attained. Their thrust is to show that not all goods (or lives) are equal and that a failure to perceive and to be governed by the highest possible good produces a radical deficiency. Neutrality and an egalitarian pluralism are the hallmarks of that deficiency, yet they are institutionally enshrined as the virtues of the market and democracy. This, in brief, is how the critique of them in its classical form is worked through. Democracy, like money, tends to make all things alike, for both are in their different ways egalitarian and thus oblivious of ranked distinction-the one to the worth of the person, the other to the use value of the thing for the attaining of the human telos. One result is that unequal things and persons are rendered (conventionally) equal by democratic citizenship and money respectively (Aristotle, Nicomachean Ethics 1131a25-33, 1133a20-23; idem, Politics 1280a8-17, 1281a-8, 1317b4-9; Plato, Republic 558c). Mirrored in the lives we lead as members of a democracy (and presumably of the emporium), our characters become, Aristotle writes, like masterless (adespotois) households in which one can do whatever one wants (Nicomachean Ethics 1161a8–9; Politics 1317b10; see also Plato, Republic 557b). One does as he wishes because the idea of a sovereign good (the master) is absent and in its absence all desires and pleasures, all life plans, are treated and honored equally. In other words, this equality and indifference as among persons and ways of life is possible where, in Plato's words, "the acropolis [of the soul is] empty" (Republic 560b, 561bc). For Aristotle as for Plato, that is an empty soul and city, a failed life and an inferior community, and its root cause is that the question of the telos, of the good life for citizen and human being, has been neglected. The "cloak of many colors," the celebration of the multiplicity and diversity of lives and ends, is testimony to the incompleteness of these institutions (and souls), evidence that they have not only not come to the end of the interrogative path "For what purpose?" but that they have lost sight of it altogether.

Here then is the nub of the dilemma for contemporary moral economists who wish to travel down that path. There is an answer to the question, "For what end is the economy in this specific, market, form?" The answer might be expressed, "So that within this sphere I may do with my things, my labor, and so on as I wish, consistent with a recognition of the right of others to do likewise; in short, so that as far as is possible under conditions of interdependency, I may lead my life 'from the inside'." Such an answer, when completely developed, is very likely

to issue in some variant of a theory of institutional neutrality among those life plans rooted in, for example, liberal conversational constraints that rule out attempts to legitimate power through claims to a knowledge of the good or to a personal superiority, in the idea of a modus vivendi in the face of moral heterogeneity or in the notion of an overlapping consensus (see, e.g., Ackerman 1980, 8–12; Larmore 1987, 129–30; Rawls 1993). If the moral economist allows the interrogative path to end there, what she has accepted is (arguably) a more reflective form of liberalism—guided by the question of the good rather than by a theory of rights alone, but a liberalism nonetheless.

If, however, she follows that road as far it was meant to be followed and as far as its internal logic suggests it ought to be, pluralism (the "cloak of many colors") and neutrality or an overlapping consensus in place of a comprehensive and sovereign good (the "empty acropolis") will not be satisfactory. What then presses onto the philosophical stage is (something very much like) a theory of the good as over against the shadow world of the market and democracy, the world structured so as to allow for a plurality of life-plans lived from the inside (see Rawls 1993, 134-135, 146n). In other words, because neutrality among competing life plans is (from this standpoint) at best a chimerical answer to the question of the good and of wealth and the economy as instruments in its service, the moral economist will be led beyond the threshold of liberal conversation. In brief, it is difficult to see a via media by which this part of the project can be pursued and not end up in a challenge to a social pluralism of ends and the norm of choosing one's own life plans. Yet even if that is so, there is still much to be learned from an engagement with the moral economic argument. For their writings draw us into a way of thinking about the economy that is radically at odds with both the economic approach to human behavior and institutions and with the rightsbased theorizing that has long commanded the conceptual heights of normative economics. They invite us-political scientists and philosophers-to place the economy in a larger cosmos of human goods, to see it as permeated by the norms of the enveloping society and thus to look at it in a light different from the one that we are accustomed to. That, I would suggest, makes the encounter with the moral economists worthwhile, however far we may choose to follow them along the road they marked out.

Notes

1. For recent social-scientific discussions, see Bates 1983; Bates and Curry 1992; Hechter 1983; North 1977; Popkin 1979; Scott 1976; Taylor 1993.

^{2.} The German terms Gemeinschaft and Gesellschaft, best known from Tönnies's (1979) study, resist translation into English, because our nearest equivalents, "community" and "society," are so close in meaning as to smudge just those distinctions that the German usage was meant to capture. The difficulty is not merely terminological, however, for the no-

tion, central to the moral economic argument, of a radical separation between two great epochs, their corresponding normative structures, and the theories needed to explain them is (as we shall see) questionable. Among contemporary communitarians, Alasdair MacIntyre recognizes the affinity of his own work with the moral economic school but does not develop it (1988, 211; see also MacIntyre 1984, 227-28)

- 3. Finley (1975) is critical of parts of Polanyi's project. Some of the classical historians cited in this essay (e.g., Vidal-Naquet and Will) develop arguments similar to those of the moral economists without, however, directly employing their writings; others such as Humphreys and Finley address them explicitly. Above all, many of them share with the moral economists a suspicion of the economic approach to the analysis of these societies. To the best of my knowledge, they have not engaged in the normative/critical dimensions of the moral economic project. Other classical historians are sharply critical of the moral economic school (Ste. Croix 1959-60; Veyne 1974, 1976). I shall draw on their writings to fill out points only thinly developed in the canonical "moral economy" literature, and these references should not therefore be taken as including them among the members of this school (if that designation is even appropriate for such a heterogeneous cluster of theories).
- 4. On the complexities of understanding investment in the classical Greek world, see Millet 1983.
- 5. For a canonical statement of the debate over the limits of scarcity, cf. Aristotle, Politics 1257b28 and 1323b7-10 and
- Menger 1976, 94–96, 98–99, 100–101, 289–90.
 6. To see the quite different interpretations given to the phenomenon of premodern trade, compare Mancur Olson's commentary on Herodotus' The History 4.196 describing trade between Carthage and native peoples (1992, 60-61) and Polanyi's gloss on the same passage (1968, 243; cf. 238-39, 241, 258). In general Polanyi emphasizes the administered character of premodern trade, the absence of an economic rationality, and the distinction between markets and trade. Olson's reading underscores the view that the "gains from trade are often huge" even in "the most difficult and primitive circumstances," rejects value-based explanations of the presence of markets, and suggests that the absence of a vital institutional nexus (e.g., courts and a legal system) prevents further such gains.
- 7. For a critique of Becker on the family, see Sen 1984, 372-73, 375; see also Sen 1990.
- 8. On subsistence, see Evans-Pritchard 1940, 84-85; Scott 1976; Thompson 1971. On labor, see Polanyi 1957c, 82-83; Thompson 1967. On the environment, see Polanyi 1957c, 73, 130; Wolf 1971, 277, 279–80.
- 9. Tönnies was wary, however, of what he took to be a romantic misappropriation of his book, one emphasizing a
- "social pessimism" about modernity.

 10. On this and related issues, see Coase 1988, 3; North 1977, 708–11, 713; idem 1981, 20–33, 42–43, 106, 181; Posner 1983, 146-173.
- 11. I develop a critique of this aspect of the moral economic argument in Booth 1993a, 76-92.

 12. On kinship groups as "insurance companies" and on
- open fields in peasant agriculture, see, e.g., Bates 1983, 7-20; Becker 1981, 238; Popkin 1979; Posner 1983, 146-173
- 13. On Scott's mode of explanation, see Booth 1993b; Taylor 1993, 109-10.
- 14. For nonmoral economic readings of the ancient economy, see Lowry 1979; Gunderson 1982; Veyne 1976, esp. 69-74 (critique of Polanyi).
- 15. For a parallel critique of communitarian political philos-
- ophy, see Homes 1993, 190–97.

 16. See Desai 1986, 8, On Marx though his comments are equally applicable to the moral economy argument. The difficulties encountered by the emerging market economies of the former Soviet bloc illustrate just how important this sustaining institutional framework is; see Clague and Rausser
- 17. A comparison of the Gemeinschaft to Gesellschaft estimation of this watershed with Maine's (1864, xl, 163) positive

valuation of the shift from status to contract is instructive in this regard.

- 18. For an explanation of the firm as an embedded institution, see Granovetter 1985, 487-504. This article is a powerful sociological critique of both the economic and embedded approaches.
 - 19. On the philological background to this, see Singer 1958.
- 20. I follow Will's suggestion and translate Aristotle's en ... tais koinōniais (Nicomachean Ethics 1132b32) by "framework of the community" (1954a, 215). Aristotle is here discussing reciprocity, but the idea clearly informs the entire economic discussion of book 5, as well as the analysis of the household in the Politics, book 1.
- 21. On food policy, see Garnsey 1988; on civic salaries, see Will 1975.
- 22. For analyses suggesting a range of goods in Aristotle, rather than a single "grand end," see Broadie 1991, 198–202; Nussbaum 1986, 352–53, 498, n. 20; Salkever 1990; Saxonhouse 1992, 185–232. Relatedly, see Yack 1985, 94, 99, 109; 1002, 207 (constitutions) idem 1993, 25–87 (community, esp. 43–50), 106 (democracy), 170–71 (political dialogue and the common good). Rawls 1993, 134-35 (and even more so MacIntyre 1984, 146-64) stress the more restrictive perfectionist reading, though needless to say with very different conclusions.

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THE PIETY OF THOUGHT IN PLATO'S REPUBLIC, BOOK 1

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In the opening sentence of the Republic, Socrates recounts his intention to combine the operations of piety and theoretical speculation. Nevertheless, many readers regard Cephalus' subsequent abandonment of rational inquiry to perform certain sacred rites as a definitive indication of Plato's opinion that piety and philosophy are fundamentally incompatible. I find this interpretation untenable inasmuch as it depends upon the misidentification of Cephalus as the dialogue's representative of piety. I suggest that the true nature and philosophical significance of piety are indicated instead in Socrates' conversation with Cephalus' son, Polemarchus. As this conversation unfolds, Polemarchus' pious inclinations culminate in a perception of the dearness of the unknown good. Inspired by this piety, Socrates and Polemarchus defend the conventional paragon Simonides and, at the same time, launch a truly philosophical inquiry into justice.

lato's Republic is reputed to be one among a handful of the greatest works ever written on the theme of social justice and personal happiness. Yet if we should wish to verify this judgment, and to derive the benefit of its insight for ourselves, we would confront an extraordinary challenge in the complexity and sheer size of the work. This challenge might seem to outrun every endeavor, for the better acquainted one becomes with the Republic, the more time-consuming becomes the consideration of any given section of it. The pace of successive readings tends to slow. Eventually one opens the text only to ponder at length the significance of a single phrase or dramatic event. Such a meditation can be exquisitely pleasant. But what is there to prevent so closely focused a study from losing its bearings? How might the student keep from losing sight of the forest for the trees?

It would be incongruous, to say the least, were a professional academic to deny the relevance of this dilemma to the study of Plato's Republic. For it is the achievement of synopsis (the capacity to see simultaneously both the forest and the trees) that the Republic submits as evidence of a dialectical nature and thus as testimony of one's being qualified to undertake advanced studies in the first place (537b8-c7). So it is incumbent upon every reader, when approaching this dialogue, to consider how the forester's dilemma might be resolved. Now, Socrates himself offers a key to the resolution of this dilemma in his remarks on the art of writing (Phaedrus 264c2-266c1). In a properly constructed composition, he maintains, one finds something like the integrity of a living body. Where such integrity obtains, the natural articulations or joints of a composition mark off segments that are intelligible precisely as parts of a whole. We may say, then, that the very divisions that call for detailed study also summon, at the same time, a recollection of the whole. By concentrating attention on such divisions, the student is able to think and to talk about the most minute textual details in a wholly nonreductive manner. Plato's Eleatic Stranger, expert in the art of dialectic, invites us to call such divisions *eidetic* or *ponderable* parts in order to distinguish them from the mere pieces into which any whole may be thoughtlessly—or even methodically—broken (*Statesman* 262c8–263b11).

In such passages Plato encourages the practice of dialectical analysis upon his own writings. But on what basis may we undertake the dialectical task of dividing Plato's Republic into ponderable parts? Clearly, some grasp of the dialogue as a whole is required before one can reasonably venture upon such a division. But whence comes this division-initiating understanding of the whole? Surely, it cannot originate in a concentrated study of ponderable parts, for this begs the question of which parts are correctly identified as ponderable. Neither can it derive from blind trust in the judgment of more experienced students, for this begs the question of the basis of the more experienced student's understanding. The only escape from this impasse lies in the possibility that the understanding needed to guide the division of the dialogue into ponderable parts is somehow accessible even to the novice upon an initial reading of the entire work.2 The contours of the very surface of the work might then be said to reveal its natural articulations. Leo Strauss put it well when he declared in the case of such a work that "the problem inherent in the surface of things, and only in the surface of things, is the heart of things" (1958, 13).

Plato employs his luminous poetic talent accordingly, to epitomize the profoundest themes of his philosophical compositions. One of his customary devices in this undertaking is the ethological mime, the representation of an action in which all pretense is stripped away and an interlocutor reveals himself both in character and in thought (see Klein 1965, 18). The first example of such a *mimēsis* in the *Republic* reaches a climax early in book 1, with the departure of Cephalus. The importance of Cephalus' departure is further highlighted by its conspicuous reversal of the dialogue's dramatic momentum. In the beginning, Socrates and Glaucon are together. These two are

soon joined by Polemarchus, Adeimantus, Niceratus, and some others. This ensemble then makes its way to Polemarchus' house, where await Lysias, Euthydemus, Thrasymachus, Charmantides, Cleitophon, and Cephalus, the father of Polemarchus. A conversation is presently launched concerning the nature of justice. Although this conversation continues into the early hours of the morning, not a single member of the congregation is said to depart, except for Cephalus. Cephalus abandons the inquiry in its infancy. The old man departs because, as he says, "the time is come when I must tend to the sacred things" (331d7).

Cephalus' departure surely stands as one of the most striking dramatic events in the dialogue. As such, it invites comparison with the violent eruption of Thrasymachus, who hurls himself at Socrates and Polemarchus "like a wild beast" (336b5-6). But the antagonism represented by Thrasymachus' outburst is composed in the course of the conversation. Socrates later states that he and Thrasymachus have become friends, though they were not even enemies before (498c6-d1). Cephalus' departure, on the other hand, is final. He never returns. So if even Thrasymachus (the sometime champion of might over right) can be reconciled to Socrates, it would seem that Cephalus' complete break with Socrates and the community of inquiry must portend an issue of considerable importance. Following the clue of Cephalus' parting words, we may infer that this issue somehow concerns the divergence of philosophical inquiry and attendance to "the sacred things." Cephalus' departure somehow manifests Plato's understanding of the tension that exists between the interrogatory motion characteristic of the philosophical life and the magisterial stability characteristic of revelation and fundamental law. Strauss characterizes this tension—the philosopher's "theologicopolitical predicament"—as nothing less than "the secret of the vitality of Western civilization" (1989, 270).3 Of course, one need not agree with Strauss's specific interpretation of the character of this tension to agree with his estimate of its vital importance. But any attempt to propose a different interpretation must account for precisely such testimony as is presented by Plato's rendition of the departure of Cephalus. We may begin to offer such an account by noting that Cephalus' schism constitutes the seismic epicenter of a passage bounded by the opening of the dialogue on one end and by the closing of Socrates' conversation with Polemarchus on the other. As we shall see, this passage must be considered in its entirety if we are to arrive at an accurate assessment of Plato's conception of the relationship between piety and philosophy.

THE DIVERGENCE OF PRAYERFUL REVERENCE AND SPECULATIVE SCRUTINY

Let us begin then at the beginning, for Cephalus' parting words are not the first indication in the

Republic of a concern for the relationship of piety and inquiry. In the dialogue's opening sentence, Socrates reports his own intention of combining these divergent operations. To appreciate the full significance of this remark, however, we must keep in mind that the Republic is, in form, a narrated dialogue-and a particular kind of narrated dialogue at that. Plato assigns the role of narrator in the Republic to Socrates. In this role, Socrates reports not only what he and others said and did on the preceding day but also what he thought then and what he thinks now, with the benefit of hindsight, of those words and deeds. So Plato puts Socrates forward as a uniquely authoritative commentator on the conversation reported in the Republic. Nevertheless, even in his narration Socrates leaves many things unexplained. For example, the occasion of his recapitulation of the previous day's doings is not itself described. Nor is there any indication of the existence (much less the identity) of any auditor or auditors. Socrates, to all appearances, is talking to himself. Of course, we readers are the intended audience—ultimately. But within the frame of the dialogue, Socrates addresses his rehearsal to no one but himself. The reader evidently is witness to an exercise in personal recollection, a soliloquy. 5 Now a soliloquy is a speech spoken with a level of candor not to be expected in the public discourse of a prudent man. So Socrates' narrative remarks will be more directly indicative of his own views than much of what he reports having said in direct discourse with his interlocutors, whose dispositions might on occasion be expected to skew, to some extent, his choice of words.6 We are well advised, then, to consider carefully Socrates' opening statement of purpose: "Down I went yesterday into Piraeus with Glaucon, Ariston's son, both in order to pray to the goddess and at the same time because I wanted to see in what manner they would conduct the festival, inasmuch as this was the first time they were holding it" (327a1-3).

In this narrative opening, Socrates cites a thoughtprovoking linkage between the two reasons that draw him toward Piraeus. He descends "both in order to pray . . . and at the same time because [he] wanted to see" (te . . . kai hama). It is important that we understand the significance of this linkage correctly. For some might suppose that Socrates is interested in nothing more than "killing two birds with one stone," in the sense of indulging his personal intellectual curiosity (theasasthai 327a3, theōrēsantes 327b1) while merely appearing to satisfy certain social expectations concerning his acknowledgment of the goddess. But Socrates does not say that he intended to be seen praying; what he says is that he intended to pray (proseuxomenos 327a2). Nevertheless, many readers will be inclined to maintain that any show of devotion on Socrates' part must be construed as nothing more than the window dressing required if the philosopher is to escape persecution by an intolerant, closed society.8 On this interpretation, Socrates bows not to the goddess but merely to his own theologicopolitical predicament.

This interpretation deserves to be taken seriously, for the Republic certainly presents convincing evidence that Socrates rejects many points of theology popularized by Homer and Hesiod. There is also the matter of his claim to possess a personal daimonion, which undoubtedly overshot the boundaries of Greek religious convention (albeit in the manner of evidence of Socrates' acknowledgment of the existence and the providence of the divine). But these considerations by no means preclude the possibility that Socrates finds himself obliged to the graciousness of certain other divinities, not all of which were utterly unrecognized by the customary religion (see, e.g., Phaedrus 279b8-c3, Republic 451a4-5). Indeed, other factors besides the candor of his narrative remarks corroborate the sincerity of Socrates' avowed intention to pray to "the goddess." For the goddess in question is Bendis, as seems clear from the sequel (354a10-11). Bendis is the Thracian persona of Hekate (a.k.a. Artemis), leader of souls in the underworld and goddess of crossroads (Pauly-Wissowa 1937, 3:269; Brann 1967, 111, n.4; Burkert 1985, 171; Morgan 1990, 21). Such a goddess might well be reckoned a patroness of Socratic political philosophy, for in one of the most celebrated metaphors in all the works of Plato, Socrates likens the political realm to an underground abode, a cave, which he in turn compares to Hades (514a-15a5, 516c8-e7). Socrates "goes down" into Piraeus, just as Heracles went down into Hades to perform certain labors (Brann 1967, 3-6) and as the philosopher king is later said to go down into politics (540b5-6; see Dobbs 1985, 812-21). Socrates makes this descent with Glaucon, a young man who stands at a moral crossroads. Glaucon faces a choice between justice and injustice, a choice that will determine the course of his life and the salvation of his soul (347e2-7, 352d4-6, 621b8-c2; see Dobbs 1994).

Moreover, if Bendis does represent some aspect of the true divinity (the one necessary and eternal being, recognized by philosophers of every era), Socrates' prayers to the goddess would naturally express and comport with his own acknowledgment of mankind's finitude and contingency. An appreciation of the limits of the human condition is crucial to the understanding of Socrates. Socratic wisdom is not divine wisdom. Yet Socrates also holds that human beings are capable of partaking in the heavenly spark of divine intelligence (e.g., 590c8-d6). For Socrates, then, the human condition is twofold in nature. Consequently, a pair of prerequisites must be satisfied for a human being truly to know himself: each of us must acknowledge the gulf separating himself from the divine and, at the same time, honor his own capacity for divine intelligence by developing it to its highest peak of excellence. Such self-knowledge would expose the mortification of ascetics and the pride of intellectuals as inadequate and merely piecemeal responses to the human condition. For what the human condition calls for is both reverential and intellectual endeavor, not merely severally or alternately but at the same time. So Socrates' twofold intention—both to pray and at the same time to

see—is indicative of the character of an ordinate and (I hazard to say) just response to the essential human calling. Of course, it remains to be seen whether and how far such a response is practicable.

What, then, are the prospects for the satisfactory realization of Socrates' intention? It seems evident that the operations of reason and reverence may collide. Piety is as demanding in its call for commitment as critical judgment is in its requirement for detachment. Each would appear ultimately to exclude the other. If we take into consideration the conversation occasioned by Socrates' descent into Piraeus, we find that Cephalus' abandonment of Socrates' inquiry into justice—at the very moment that inquiry appears to intrude upon his service to the divine—dramatizes this conflict and suggests the possibility that piety and rational inquiry are fundamentally incompatible. This possibility shades increasingly into a certainty as one looks upon Cephalus as the embodiment of genuine piety. If Cephalus does stand for piety, then one might well conclude that it is simply impossible for the same person to do justice to both aspects of the human condition (Strauss 1989, 269-70).

But it is by no means clear that this is Socrates' view, or Plato's. Certainly, one cannot leave unexamined the possibility that it is Socrates, rather than Cephalus, who practices genuine piety. In fact, Socrates' disclosure of intent in the narrative opening of the Republic corroborates the public testimony he offers later at his trial on the charge of impiety. In this testimony, Socrates answers the Athenians' notorious indictment by portraying his customary crossexamination of fellow citizens as a service to god. He explains, however, that this pious service involves, at the same time, a scrutinization (elexon) of the divine oracle itself (Apology 21b1-23c1). In other words, Socrates interprets his divinely sanctioned vocation as one that requires a combination of piety and rational scrutiny. 10 Plato's illumination in the Republic of the quality by virtue of which Socrates can achieve this combination will succeed, however, only if certain pretenders to the status of reverence and reasonableness are first unmasked. This, to state the matter concisely, is the guiding purpose informing his composition of Socrates' conversation with Cephalus.

THE EXPOSURE OF RATIONALISM IN CEPHALUS' FALSE PIETY

As Socrates and Glaucon head homeward from the sacred processions, they are intercepted by Polemarchus' servant. Polemarchus and Adeimantus arrive presently and prevail upon Glaucon and Socrates to go to Polemarchus' house. There Socrates meets with Polemarchus' father, Cephalus, who has just come from performing some sacred rite himself. Cephalus greets Socrates warmly. Pleading his own inability to make the journey up to the city, Cephalus urges Socrates to come to the Piraeus more frequently.

Socrates says he delights in conversing with his elders, for it permits him to inquire about the path that he, too, may have to travel one day. So he asks Cephalus, "Is it a hard time of life or what?" Cephalus' answer is heartening. This old man discounts the many complaints one ordinarily hears from old men. Their nostalgia for the faculties and pleasures of youth is especially mistaken, according to Cephalus. As against such complaints, Cephalus notes that even the tragedian Sophocles, high priest of the Dionysian celebration of eros, regards the dwindling of sexual appetite as a liberation from a raging tyrant. To mourn such a liberation is absurd. The complaints and the troubles of the aged must be blamed, then, not on old age but on bad ways (329d2-6). After all, if it were old age itself that caused such trouble, Cephalus reasons, he would suffer from such difficulties as much as the next man.

Socrates marvels at Cephalus' saying this and desires to hear more. He notes provocatively that many would be likely to dismiss Cephalus' testimony altogether; they would credit his brave words to the possession of wealth. Cephalus concedes that wealth is important, though not so important as the multitude may suppose. By way of explanation, he recounts a saying of the illustrious Themistocles. It seems that a certain "Seriphian detractor"-a man embittered by his own obscurity, which he blames on the political insignificance of his native city-once insisted that it was only thanks to Themistocles' being an Athenian that he had achieved glory. In reply, Themistocles acknowledged that if he too had been a Seriphian, he would never have become renowned; but neither would this detractor ever have become renowned, even if he had been an Athenian! Cephalus thus suggests an analogy between his own conception of great wealth and Themistocles' conception of great politics. Cephalus no more regards wealth as an assurance that one will possess the greatest good than Themistocles so regarded citizenship in a great polity. Socrates surmises that Cephalus has inherited his wealth, because he does not seem in what he is saying to be overly fond of money (330b8-c1). Those who earn their own wealth, in contrast to those who are given it, resemble parents or poets. The latter cherish their children or poems not only insofar as these are good or serviceable (kata tēn chreian) but also because they are their own (330c3-6). Socrates thus remarks on the absence in Cephalus of a vehement (sphodra) love of his own. It seems then that the Themistocles analogy may run deeper than Cephalus had intended, for, as we know, Themistocles betrayed his own country, showing himself in the end to be indifferent as between the equally serviceable political communities of Athens and Persia. Cephalus, we may say, is similarly indifferent as to the source of his wealth, provided only that it proves to be of service to him. The precise character of Cephalus' understanding of the serviceability of wealth remains, then, to be considered. So far, Cephalus has suggested that wealth is some sort of prerequisite to the enjoyment of happiness.

One wonders in what sense wealth is prerequisite to happiness. In Cephalus' case, the relationship between wealth and citizenship exceeds the analogical and approaches the literal, for as a resident alien, or metic, Cephalus is not eligible to participate in the politics of Athens. For all practical purposes, Cephalus' wealth is his country. 11 But to Socrates, who proclaims that he never left Athens except to fight in her defense and who suggestively adverts to the riches of his own ten-thousand-fold poverty, Cephalus' likening of wealth to citizenship must appear questionable. So Socrates invites Cephalus to name what it is that he takes to be the "greatest good he has enjoyed from his abundant wealth" (330d2-3). Cephalus declares that thanks to his wealth he has not been forced "to cheat or to deceive anyone, or to depart for that next place in fear, on account of owing some sacrifices to god or money to a man" (331b1-4). Of course, Cephalus stipulates that wealth does not provide this benefit for just any man but only for one who is decent and orderly (tōi epieikei kai kosmiōi). Couched in this qualification, Cephalus' evaluation of wealth might seem innocuous enough. One is almost tempted to equate it with Aristotle's appraisal of wealth as the "necessary equipment" of a happy life (Nicomachean Ethics 1101a14-16). But we must tread carefully here, for Cephalus values great wealth not as an instrument employed in the exercise of an already confirmed disposition to virtue but rather as a necessary means to confirm such a disposition in the first place. Its possession provides insulation from circumstances that Cephalus assumes will drive any man to crime. But for Aristotle it is precisely character that keeps a decent man "in character." As it happens the decency stipulated by Cephalus is not a matter of character (ēthos) in the strict sense at all. Instead, it is in his own words merely a tropos, that is, an inclination or tendency (see 329d3-4). Wealth, according to Cephalus, is the functional equivalent of

Once this has come to light, Socrates admiration for Cephalus gives way to thinly veiled irony. 12 Nevertheless, Cephalus' words and deeds continue to demonstrate that he is a man very much concerned (one might even say preoccupied) with ultimate or eschatological matters. This does not, of course, establish his piety or righteousness. On the contrary, the aged Cephalus experiences what may be described as a servile, rather than a reverential, fear of the gods. It is only due to anxiety at the prospect of retribution in the afterlife for his crimes in his present life that the anerotic Cephalus even takes notice of the gods: "Know well, Socrates, that when one is gripped by the thought that his end is near, he is possessed by fear and concern for things that never occupied him before. The stories, which he used to ridicule, about what goes on in Hades—that the man committing injustice here must pay the penalty there—now torment his soul for fear they might be (330d4-e2). (A truly erotic soul, however, would not require the prick of such anxiety to seek the divine [see, e.g., Phaedrus 248d2-249e4].) Of

course, Cephalus is uncertain whether his newfound interest in sacred matters is a function of some special perspective vouchsafed the aged by virtue of their proximity to death or is merely a function of the infirmities of old age (330d7-e5). But he does acknowledge that he finds security in assuming the worst as regards the eschatological facts of life. Cephalus compares what he has to lose if he mistakenly disregards the warnings of traditional theology with the value of the money required to carry out the customary, propitiatory oblations. He decides to sacrifice his superfluous wealth, which he takes to be all except enough to leave his heirs a little more than what his own spendthrift father left him after wasting a good portion of the family inheritance (330b6-7). In other words, Cephalus makes his gamble on the same terms as the famous wager, which, we may be assured, Pascal contrived to demonstrate the untenability of the rationalist criticism of piety, rather than to induce genuine faith (Pascal 1986, §418 and §110). Hence, Cephalus' turn to religious ceremony is hardly pious in itself; it is merely indicative of a certain talent for calculation that is pressed into the service of anxiety concerning the afterlife. It follows that Cephalus is better understood as a representative of rationalism—which holds that rational calculation suffices for the resolution of all questions of human affairs—than as the spokesman for piety. 13

Of course, there can be no question but that Cephalus embodies the mercenary tendency that the vulgarization of Orphic writings and religious practice had taken in his time (see, e.g., 363c3-365a3, 365d7e6). In this sense, one may speak with some propriety of Cephalus as representative of a decadent piety. But traditional piety in its decadence is by no means to be confused with traditional piety in its prime (see, e.g., Morgan 1990, 108-14). Hence, I suggest that Cephalus' divine service bears roughly the same relation to traditional Greek piety that Macy's Christmas parade bears to the pilgrimage of the Three Wise Men. To be sure, Cephalus' departure from the conversation indicates the incompatibility of a decadent piety with Socratic philosophy, but it in no way settles the question whether or not Socratic philosophy is at odds with uncorrupted piety. Therefore, if, as Bloom says, "Socrates must induce Cephalus to leave the scene because Cephalus is beyond reason" (1991, 312), we must add that it is not due to piety that Cephalus is beyond reason. We may grant that Cephalus leaves because he is beyond reason, but he is beyond reason only because there is something unreasonable in being a rationalist.

In the course of answering Socrates' question concerning the greatest good that derives from his possession of great wealth, Cephalus had occasion to imply that it is unjust to cheat or lie to any man or to fail to render to gods or men the sacrifices or things they are owed (331b2–3). Picking up on this suggestion, Socrates now asks whether we should pronounce "the opposite of these things, namely the truth and the rendering back of what one has received from another, to be justice or righteousness

itself; or should we say that it is just in some cases and unjust in others to do these very same things? (331c1-5). Cephalus does not respond. He has not taken the trouble to formulate definitively his understanding of righteousness, whether in relation to other human beings or to the gods. What he has is a rule of thumb. But Socrates pursues the inquiry. He proposes a thought experiment that may help Cephalus to sort out the essential from the merely accidental characteristics of acts of justice. Socrates supposes that everyone (pas 331c5) would agree that it would not be just or right to return a borrowed weapon to its owner, if the owner were to demand it in a fit of madness. Cephalus acknowledges, in response, that Socrates speaks correctly (331d1). Hence, there can be no question but that Cephalus correctly judges the force of Socrates' counterexample. The most we can say is that Cephalus fails to follow up this correct judgment with an attempt to formulate more adequately his understanding of justice itself.

Cephalus, in other words, is complacent in the face of the manifest inadequacy of the definition that Socrates has cobbled out of his rule of thumb. But this is neither irrational nor contrary to expectation. The fact of the matter is that Cephalus correctly judges the test case. (Of course, this is no great claim, since Socrates is willing to assume that it is true of everyone. We shall consider Polemarchus' taking exception with this momentarily.) In other words, Cephalus rightly perceives that the situation described by Socrates does not properly fall under the jurisdiction of his rule of thumb. But a genuine refutation would require that Socrates devise a case in which Cephalus errs either in applying or in failing to apply his rule of thumb. Socrates does not do this. Logically (but only logically), then, Socrates and Cephalus are quits. Polemarchus steps in at this point, oddly enough, to defend his father's rule of thumb as if it were a definition (horos 331d2–5). Polemarchus' initiative provides Cephalus with an opening to depart. The old man speaks of handing the argument down to the assembled company (hymin, 331d6). But Polemarchus persists in taking matters into his own hands: "Then am I not the heir of what is yours?" (ego, cf. toutoisin 330b6). Cephalus laughs at his son's enthusiasm, uttering something like, "It's all yours, such as it is" (Pany ge 331d9), as he departs to attend to the sacrifices.

The palpable contrast between Cephalus' appreciation of the force of Socrates' counterexample and his son's zeal to defend the indefensible suggests that the shortcoming of Cephalus, anyway, is hardly one of rationality. Moreover, as we have already noted, Socrates does not refute Cephalus. What Socrates does accomplish in their exchange is to reveal with perfect clarity the nature of Cephalus' temperament. This revelation is a critical step in the disclosure of Plato's understanding of the relationship of piety and philosophy, for Cephalus represents a common vulgarization and misunderstanding of piety. As long as Cephalus stands as a representative of piety, the relationship of piety and philosophy will be miscon-

strued. Plato corrects this misunderstanding through his ethological mime, in which he strips away Cephalus' pretense to piety and reveals that Cephalus' turn to religious ceremony is thoroughly mercenary, not reverent. Hence, we may conclude that Cephalus' departure from the dialogue in no way dramatizes the incompatibility of reason and piety. If anything, his departure is indicative of the divergence of rationalism from the virtue of reasonableness. Rationalism, thus understood, is an obstruction to the mutual opening of reason and reverence. With the departure of rationalism, this obstacle is removed.

THE PLACE OF REVERENCE IN POLEMARCHUS' TURN TO PHILOSOPHY

Cephalus, as already noted, does not attempt to universalize his rule of thumb. But Polemarchus rushes in where his father is too shrewd to tread. Polemarchus holds that his father's formulation does provide an adequate definition of justice, "at least if we are to be persuaded by [or to obey] Simonides" (331d5). Polemarchus thus acknowledges a requirement (chrē) to defer to Simonides. Socrates by no means disputes this requirement; he grants that Simonides is a man of wisdom and godliness (331e6). So he invites Polemarchus to identify "what it is you suppose that Simonides said correctly in speaking about justice" (331e1-2). Polemarchus thinks Simonides speaks not just correctly but nobly (kalōs) when he states that justice is rendering to each what is owed (331e3-4). We must note, however, that what Polemarchus espouses here is an unwarrantedly literal-minded interpretation of the poet. By the term "what is owed," Polemarchus evidently assumes that Simonides means exactly what another has lent him. Hence, Polemarchus deems superfluous the question of the mental capacity of the man in Socrates' thought experiment, the relevance of which even his father had appreciated. On this basis, he holds that his father's rule of thumb may be enforced over the entire range of just actions. Polemarchus maintains that it is just and right, regardless of circumstances, to return a borrowed weapon to its original owner, because such a weapon is plainly owed to its owner and because justice consists in returning what is

But before we go too far in disparaging Polemarchus' literal-minded veneration of Simonides, we must pause to ponder the significance of the manner of Socrates' questioning. Socrates asks no more of the young man than to declare what it is that he supposes Simonides says correctly. In other words, Socrates does not impose upon his interlocutor the Cartesian burden of proving beyond all possible doubt the truth of what Simonides says. Socrates thus avoids calling the wisdom of Simonides into question (see Adam 1902, 1:13; Bloom 1991, 316). But the significance of this insulation of Simonides from the rigors of

Socratic elenchus is commonly mistaken. Many readers see in it no more than the tact (and perhaps the tactic) of a philosopher who prudently camouflages his own awareness of the shortcomings of a conventional paragon, which an intellectually inferior interlocutor is supposedly incapable of appreciating. Such a view, however, assumes more than an inequality between the interlocutors; it posits a discontinuity between what Socrates knows in this matter and what lies within the capacity of Polemarchus to understand. It presumes that Polemarchus is unable to learn what Socrates can already discern. (The reader may wish to consider the extent to which this view implies a certain dogmatization of the essentially negative character of Socratic wisdom.) As an alternative to this view, I would simply suggest that Socrates' respectful treatment of Simonides is informed by his appreciation of the possibility that Simonides might well be right and by his collateral desire to help Polemarchus to acquire a similarly thoughtful appreciation. In other words, I am suggesting that Socrates reads Simonides in the way we have undertaken to read Plato. Socrates' mode of questioning Polemarchus encourages the lad's confidence in the poet's wisdom and, at the same time, calls upon him to exercise his critical abilities in identifying the formulation that best expresses this wisdom. We should note that Socrates endorses such a combination of conviction and discrimination throughout this dialogue on justice. He holds that this combination will ensure that inquirers will be, as he puts it, "both judges and advocates at the same time" (348b3-4). Socrates' locution, "both judges and advocates at the same time," recapitulates the linkage of the divergent operations of piety and reason, advocacy and criticism, which was suggested in the dialogue's opening sentence. But it is Socrates' own conduct in the inquiry that most strikingly dramatizes the correlation of these divergent operations, for he steadfastly advocates the cause of justice as sacred (368b7-c2) even as he conducts a searching investigation of its nature and goodness.14 It is no surprise, then, that Socrates proceeds as he does in his questioning of Polemarchus. He summons Polemarchus to be both an advocate and a discriminating judge of Simonides. By responding as Socrates directs, Polemarchus becomes a full participant in Socratic in-

So rather than ridiculing Polemarchus' literal-minded veneration of Simonides, Socrates attempts to encourage its development. He indicates that the phrase "to each what is owed" cannot be meant literally, for no one would say that a borrowed item is to be returned to anyone whatsoever, should its owner make such a demand when out of his mind (331e3-4). Socrates' nudge opens Polemarchus' eyes. It must mean "something else, by Zeus," Polemarchus now realizes, "for [Simonides] thinks that it is owed to friends to accomplish good and nothing bad" (332a9-10). Polemarchus reasons that if something bad results from returning a weapon to a friend who is out of his mind, it cannot be right to have returned what

one has borrowed. Polemarchus' devotion to friends works as a lever to dislodge him from the grip of a narrowly literal and unwarrantedly dogmatic interpretation of Simonides. Interestingly, his recollection of friendly solidarity has made it possible for him to exercise critical thought. Polemarchus now judges that it is the poet's statement on friendship—that it is owed to friends to accomplish good and nothing bad-that is his most important dictum. Consequently, the lad abandons his literal interpretation of "what is owed" without in the least abandoning Simonides. Polemarchus merely exercises the prerogative of interpreting Simonides out of Simonides, just as the classical formula for sound exegesis recommends. If attention to the whole of an authoritative body of writings demonstrates more respect for an author than does mere proof-texting, we may say that Socrates has already succeeded in bringing about an improvement in the quality of reverence Polemarchus is prepared to show Simonides.

As we have noted, this refinement in Polemarchus' reverence follows upon his own recollection of friendly solidarity. A hearty concern for that which is dear (to philon) is characteristic of the psychic capacity that Socrates terms spiritedness (thymos). 15 Polemarchus, whose name evokes the fiery spirit of a warlord, momentarily overlooked the special claims of friendship when he formulated his argument for the justice of returning deposits always and everywhere. He quite naturally swears an oath as he recovers himself and corrects this oversight. Polemarchus' vehemence combines anger for the past affront to friendship and zeal for its present correction. Socrates invites Polemarchus to complete the formulation of his understanding of Simonides' saying concerning justice, now that he has the special claims of friend-ship clearly in mind. "What about enemies," Socrates asks, "are we to render whatever it is that happens to be owed to them?" (332b5) "By all means," Polemarchus replies, "we are to render to them exactly what is owed, which I take to be the very thing that suits [prosēkei] an enemy from an enemy, namely something bad [kakon]" (332b6–8). So, according to Polemarchus' revised interpretation: (1) Simonides' phrase "what is owed" is best understood not literally but rather analogically, as the suitable or the fitting and (2) the suitable or fitting in the case of enemies is something bad. Polemarchus thus proposes that what Simonides means by justice is rendering something good to friends and something bad to enemies.

It may be inferred from Polemarchus' oath ("by Zeus!") that he has his heart—that is, his thymos—set on this definition of justice. It appears that Polemarchus' most heartfelt desire is to uphold and to do right by the distinctive status of friends. Socrates must nevertheless subject this understanding of justice to a searching examination if there is to be any further refinement of Polemarchus' piety. Such an examination calls for great delicacy, to say the least. Socrates conducts this examination in three steps, the outlines of which may be briefly indicated in anticipation of more detailed discussion below. First, be-

cause thymos is invincible (375a11-b1, 440d1-3), one cannot oppose its present orientation directly. Instead, Socrates seeks to disengage the spiritedness with which Polemarchus embraces the notion that justice consists in a sovereign power of distributing boons and blows. Socrates accomplishes this disengagement by showing Polemarchus that such a conception diminishes the excellence of justice and calls into question the goodness of loyalty to friends (332c5-334b7). Second, Socrates elicits a new recognition on Polemarchus' part of the dearness of the unknown good. 16 This recognition results in a remarkable reorientation of Polemarchus' spiritedness. Socrates achieves this reorientation by showing Polemarchus that the true friend is the good, even when this good is not yet apparent as such (334b7-335a5). The acknowledgment of the dearness of the unknown good, I shall suggest, constitutes genuine piety or reverence. Because this acknowledgment is informed by a recognition of the dearness (as distinguished from a putative knowledge) of the good, it establishes in the soul a robust loyalty to the authority of the good, which is resistant to the venal or hubristic tendency to declare oneself instead as autonomous. 17 Third, and finally, Socrates argues that the good is never a cause of corruption or harm. It follows from this that the determination to do damage or harm to anyone must involve turning one's back on the good—a manifest act of disloyalty toward the true friend! Socrates thus demonstrates, in terms that thymos itself will find persuasive, that justice involves a steady orientation toward the good and so cannot involve the determination to harm anyone.

The Disengagement of Polemarchus' Spiritedness

Socrates now commences in earnest his examination of Polemarchus' conception of Simonides' understanding of justice. "By Zeus!" he demands, with some spiritedness now of his own, "What would Simonides say if someone asked...?" (332c5-8). Judging from the thrust of Socrates' subsequent questioning, we may surmise that this oath represents a didactic expression of vexation at the feebleness of Polemarchus' present appreciation of justice. For Socrates immediately elicits Polemarchus' agreement that arts other than justice, each within its own domain, have the capacity to render what is fitting and due. Consequently, one is left to wonder just what, if anything, is rendered by the art of justiceand to whom (332d2-3). Polemarchus follows the line of questioning perfectly well. He maintains that justice consists in benefiting friends and damaging or harming enemies. But this is an unsatisfactory answer and does nothing to elevate the importance of justice, for the things fittingly assigned by other arts may also be understood, more generally, as goods and bads. When Socrates asks, "Who, then, has the most power to accomplish good for ailing friends and bad for enemies as regards disease and health?" Polemarchus sees the point. He accordingly ignores the claim of the just man and names the physician

instead. Similarly, when the question is repeated, now concerning voyagers facing danger at sea, Polemarchus again ignores the righteous and instead nominates the helmsman as the one most able to render good and bad.

Searching, then, for the arena in which justice might come into its own, Socrates challenges Polemarchus to say where it is that the righteous will prove to have "the greatest power to benefit friends and to damage enemies" (332e3-4). Polemarchus answers that it is "in the conduct of war and in alliances" (332e5). But evidently not (as Socrates recalls on the basis of Polemarchus' previous admissions) when we are sick or at sea (332e6-10).18 Polemarchus admits Socrates' point. Socrates then moves to broaden the perspective of the young "warlord," for Polemarchus so far has failed to mention the usefulness of justice in peacetime. Socrates alludes to this possibility and Polemarchus quickly responds that justice is indeed useful in peacetime, particularly in partnerships (332e11-333a14). But again, when Socrates invites Polemarchus to choose between the expert and the just man as the more useful and better partner in each of several transactions, his young interlocutor chooses the expert in every case. Polemarchus would prefer the collaboration of an expert draughtsplayer when positioning draughts, a mason when laying bricks, and a harpist when playing the harp. In all these cases we may say that Polemarchus rates virtuosity above virtue.

Yet there is one situation in which it is the just man who seems to Polemarchus to be the better partner—in partnerships where there is money dealing. But this turns out to be too broad an endorsement even for Polemarchus, for he admits, under pressure from Socrates, that if the partners are to use the money (say, to buy or to sell a horse in common), then someone who has technical knowledge (in this case, about horses) would be a better partner than the just man (333b11-c2). It is only when money is not put to use but merely needs to be kept safe that the just man is the better partner. Socrates summarizes the discussion, employing images that together represent the comprehensive human experience of war and peace: justice comes into its own "when there is need to guard shield and lyre and no need to use them; but when there is need to use them, it is the art of the infantryman and the musician that are of use" (333d6–9). Socrates thus presents the work of guarding or preservation as the least glorious, the least worthwhile, of human activities. Polemarchus is moved to agree that justice is useless for useful things and useful only for useless things. Socrates turns to his young interlocutor and remarks "Justice, then, my friend, would not be worth taking seriously at all" (333e1-2).

Socrates addresses Polemarchus as his friend (\bar{o} phile), while examining the notion that justice consists in benefiting friends and harming enemies. He thus invites us to ponder what good thing it is that he is accomplishing for this dear young man. Of course, many readers have doubted that Socrates is convey-

ing any benefit at all. After all, Socrates has just led Polemarchus to a fallacious conclusion; his denigration of justice clearly exceeds anything required by the logic of the argument. We agree. Socrates' disparagement of justice is, in every sense of the word, gratuitous. Certainly, nothing in the work of guarding or preservation requires such a disparagement. If anything, the notion of justice as safeguard or preservative implies an enormously attractive role for this virtue, as subsequent discussions in the Republic will elaborate. Justice Saves is by no means a slogan incapable of stirring the blood. But Socrates plays this down for the present, precisely because it is his concern to settle, rather than to stir, Polemarchus' blood. In so doing, Socrates accomplishes something very much of benefit to his friend. And the benefit is indeed gratuitous, that is, a true gift. For, as Socrates notes, Polemarchus' definition of justice-benefiting friends and harming enemies—is typical of the frame of mind of "some rich man who supposes that he has the power to do big things" (336a6-7). (It was Polemarchus, we recall, who at first threatened to use force to compel Socrates to remain in the Piraeus. In that threat, Polemarchus placed himself beyond the influence of reason; he refused even to listen to Socrates' attempts at persuasion. The only thing to which he would give way was even greater power [327c9].) Dazzled by the power to accomplish "big things," a youth such as Polemarchus will grow oblivious to the need to learn the good things. If such a youth is ever to begin to consider what is good, he must first be disengaged from his attachment to what he currently regards as big or great. So we may say that in Polemarchus' case, at least, Socrates' familiar use of the analogy between virtue and the arts does not merely serve to emphasize the role of intellect in what is ordinarily called moral virtue. The prosaic implications of this analogy serve, even more importantly, as an antidote to the charm that power holds over our understanding of human excellence.

Socrates invites Polemarchus to consider further the expert craftsmen in whose likeness he has been modeling the just man. It is noteworthy that in the course of this consideration Socrates never speaks of this expert in the professional form of address. For example, instead of referring to the physician (*iatros*) as such (as Polemarchus had done, 332d12), Socrates speaks of "the one who is cleverest at guarding against illness as well as engendering it unnoticed" (noson . . . phylaxasthai kai lathein houtos deinotatos empoiēsas, 333e6-7). The circumlocution draws attention to the possibility that true professionalism, embodied in the physician properly so-called, is not to be identified with moral neutrality or mere dynamic bivalence. It must be admitted, however, that even if it is not morally neutral in its essential act, technē does remain peculiarly liable to exploitation for alarmingly divergent purposes. It is with a view to this liability, I would suggest, that Socrates introduces to the discussion the term deinos, which means "terribly (or, perhaps, terrifyingly) clever," in order to characterize the disposition of one who exploits the equivocal or

bivalent potentialities of technical expertise. For the professional, like the clever man, plies a craft; but unlike the clever man, he never permits his craft to degenerate into mere craftiness. Socrates' strategy is to show Polemarchus that this attribute of cleverness or *deinotēs*, with which the lad's current conception of justice is contaminated, is not merely useless but is vicious in itself and repugnant to genuine friendship.

Socrates poses several leading questions and suggests a literary parallel, which together move Polemarchus to reject anything tainted by cleverness. Socrates induces Polemarchus to agree that (1) the most clever at striking a blow in battle is also the one to guard against such blows, (2) whoever is clever at guarding against illness is also most clever at causing illness unnoticed, and (3) the good camp guard is also the very man to steal what belongs to the enemy (333e3-334a4). Polemarchus is compelled by these admissions to conclude that the deinos guard—and so the just man, in accordance with his current conception of virtue—is essentially a clever thief (334a5–10)! Socrates then blames Homer for teaching Polemarchus this lesson. Homer, Socrates notes, is especially fond of Odysseus' grandfather, a man named Autolykos (roughly, "Lone Wolf"). In the words of the poet, this Lone Wolf "surpassed all mankind in thievery and in oath-swearing" (334b2-3; cf. Odyssey 19.394-95). What it means to surpass all mankind in thievery is plain enough, but how one wins such distinction "in oath-swearing" calls for an explanation. Stanford surmises that Autolykos excelled all mankind "by cleverly framing his oaths so as to leave loopholes for advantageous evasions later" (1958, 2:332). As a devotee of Hermes (from whom our word hermeneutics derives), Autolykos was able to exploit verbal equivocation and so was able always to get the better end of promises and bargains (Odyssey 19.396-98). Thus, in view of Polemarchus' preceding admissions concerning the bivalent potentialities of technical expertise and so of the righteous man himself, Socrates quite reasonably offers Homer's praise of Autolykos as a fitting epigram of Polemarchus' appreciation of justice.

But if the elevation of Autolykean cleverness to the status of righteousness is what his argument comes to, Polemarchus wants no part of it. "No, by Zeus," he swears, "though I, at least, no longer know what it is I meant" (334b7). This vehement rejection of Socrates' imputation indicates that Polemarchus would prefer to renounce his own claim to know what justice is rather than to endorse as just the cleverness characteristic of Autolykos. Surprisingly, Polemarchus is unmoved even by Socrates' stipulation that such cleverness be employed "to benefit friends and to harm enemies" (334b3-5). One might have expected this stipulation to have made all the difference to Polemarchus. There can be only one reason why it does not. The practice of Autolykean cleverness must itself be at odds with the good of friendly solidarity. Polemarchus evidently notices that Autolykos, the Lone Wolf, remains essentially a loner regardless of who happens to benefit from his

cleverness. Socrates' delicacy of expression has paid off; Polemarchus has come to see the terror signified by the term *deinos*. Indifference to the good, wherever it surfaces, makes us strangers even to those who are otherwise dearest to us.

Of course, in defense of cleverness, someone might assert that the versatility of technē implies the absence of a telos or goal proper to the art as art and so authorizes the clever man's willingness to ply his craft for evil as well as good ends. But this is mistaken. The versatility of the arts-which indeed renders them liable to such exploitation—simply reflects the importance of what the Greeks called kairos (the due measure, appropriate instrument, opportune time, etc.) in human affairs. The possession of technē entails an ability to address the exigencies of any occasion. Owing to the diversity of circumstances, an action that at one time promotes a particular purpose will at another time thwart it. There is, therefore, a necessary versatility in the capacity that the technical expert commands. For example, a physician will be equipped both to feed (a cold) and to starve (a fever); this versatility is properly exercised in the interest of promoting the intrinsic end of medicine, namely, health. The clever man exploits this versatility, however, not in pursuit of health but rather to further some extrinsic end. For example, "the one who is cleverest at guarding against illness as well as engendering it unnoticed" might take steps to starve his political opponent and to feed his favored candidate. Such an outcome might be thought to be attractive to a man like Polemarchus, who takes devotion to friends so seriously. But there is no reason to suppose that someone who is indifferent to the good of his own art is going to be any more attentive to the good upon which his "friendships" can be distinguished from merely arbitrary or chance associations. Polemarchus rejects cleverness, then, because he senses that it is at odds with the foundation of genuine friendship even though Socrates has stipulated that such cleverness is to be employed for the benefit of friends and the harm of enemies.

Moreover, at the same time that he heartily repudiates cleverness, Polemarchus reveals his new openness to self-examination. "No, by Zeus," he declares, "though I, at least, no longer know what it is I meant; yet it still seems to me that justice consists of benefiting friends and damaging enemies" (334b7–9). We may say that this remarkable statement, taken as a whole, indicates (1) that Polemarchus now recognizes that his opinion is an opinion, not knowledge; (2) that he is astonished at this recognition and so has genuinely learned something in coming to it; and (3) that his *thymos* is now oriented toward upholding or enforcing this recognition.

Polemarchus Acknowledges the Dearness of an Unknown Good

Polemarchus knows now that he does not know what justice is. He admits, nevertheless, that justice still

seems (dokei) to him to consist in benefiting friends and harming enemies. So Polemarchus has changed, even if what seems or appears to him has remained the same; he is now in a position to examine rather than merely to insist upon this opinion. Measured by Socratic standards, this is a most significant development. Socrates' response is to launch Polemarchus upon the examination of this opinion. He asks whether Polemarchus means by friends "those who seem to each to be good or those who really are good, whether they seem to be or not" (334c1-3). Polemarchus conjectures that one "befriends those whom he believes [hēgētai] to be good" (334c4). Polemarchus' word hēgētai implies rather more than a mere perception, which could be derivative from preexisting affinities: hēgētai designates instead a leading belief, thus compactly conveying Polemarchus' conjecture that it is one's belief concerning the good that is the hegemonic factor in determining his affinities, rather than vice versa. Such a hegemony is in fact essential to the independence of thymos, to its very existence as something more than the love of one's own. For if our belief concerning the good were exclusively beholden to what is nearest and dearest, our capacity for friendship could never transcend our cultural circumstances. Were this true, a social science informed by the doctrine of cultural relativism could eradicate any need for philosophy. But a truly hegemonic belief concerning the good, the only kind of belief worthy of thymos, cannot take its marching orders from what is currently familiar. Instead, as we shall see, it must be informed by an acknowledgment of the dearness of an unknown good.

Socrates next asks Polemarchus whether human beings err in their beliefs concerning the good, such that in their view many seem good who really are not good and many others do not seem good who really are (334c6-8). Polemarchus acknowledges that human beings do err in this way (hamartanousin 334c9). Socrates notes that it would then be just, on Polemarchus' account of justice, to harm the innocent. But there is no way (mēdamōs 334d7), Polemarchus declares, that he will brook such an enormity. Rather than affirm so transparent an injustice, Polemarchus vehemently denounces as burdensome and wicked (ponēros 334d7-8) any account of friendship, including his own, that is open to such an implication. In the case of a conflict, Polemarchus will side with what is right, rather than with what is his own. Granting that it is not right to harm the innocent, Socrates still wonders what is right. So he inquires, "Is it right, then, to harm the unjust and to benefit the just?" (334d9–10). Polemarchus answers that this appears to be nobler (kallion) than the converse. But, as Socrates proceeds to point out, this new formulation renders friendship completely irrelevant to the consideration of justice, contrary to what Simonides had said (334d12-e4). In other words, Socrates appeals to the authority of Simonides here in order to remind Polemarchus that one cannot utterly disregard friendship in the course of doing right. Polemarchus' task is to uphold the relevance of friendly solidarity in the

consideration of justice without permitting it to become a source of corruption. Polemarchus sees perfectly well, now, what must be done; he must qualify the role of perception in the determination of what is dear. For the sake of friendship, Polemarchus is prepared to move beyond the limits of perception. He urges Socrates to join him in changing (metathōmetha 334e5) what they had posited as the friend.

Socrates and Polemarchus had been supposing that it is the seeming good that is dear. Polemarchus proposes that they now define as philon that which both seems and really is good (Ton dokounta te . . . kai ton onta chreston, 334e10). He infers that the apparent good—if it is not also really good—is only apparently, though not really, dear (334e10-335a2). By the same token, we may say that the really good-f it is not also apparently good-is really, though rot apparently, dear. Socrates seizes the latter inference, noting that "on this account the good will be dear, as is fitting" (335a3). Socrates thus indicates that Polemarchus' new position brings to light the immediacy of the connection between what is really good and what is really dear. What is fitting has, on this basis, also become apparent: namely, that one's acknowledgment of what is dear is not exclusively a function of current perception. Following Socrates, we may suggest that the most important consequence of Polemarchus' reformulation of the definition of the pailon is not its obvious indication that the good must be perceived for it to appear to be dear but rather its subtle implication of the real dearness of a good that is not yet apparent as such. It is on this basis, we may surmise, that Socrates now speaks of a good that is not simply chrēston but agathos (335a3, cf. 334c4, 334e10). The verbal distinction—the contrast between these terms—indicates that the true friend is a good distinctively beyond what we presently recognize as useful (chreix) to us.

This insight permits Polemarchus to unhold the relevance of friendly solidarity to justice and, at the same time to resist its decomposition into nepotism. Polemarchus thus succeeds because he has found a true friend in the good. Put somewhat differently, Polemarchus finds that if he is to honor friendship as he deems fit, he must attend more to the existence of a sovereign good than to his own possession of a sovereign power. This finding, I suggest, marks the dawn of genuine piety or reverence in the soul of Polemarchus. Polemarchus' devotion to the cause of friendship does not require that he know the good, only that he acknowledge the dearness of the good he does not know. Now, this discovery by no means implies that Polemarchus does not have more to learn. In fact, he does, for in his first attempt to demonstrate his friendly solidarity with what is really 300d, Polemarchus agrees with the proposition that "it is just to benefit the friend precisely because he really is good [agathon onta], and to harm the enemy precisely because he really is bad [kakon onta]" (335a9-10). Thus it still remains for Polemarchus to learn that the determination to harm anyone will tend to alienate him from the true friend, rather than drawing him closer to it.

Polemarchus Learns That the Righteous Do No Harm

As matters stand, Polemarchus supports what may be called the homeopathic conception of justice—that it is right to do good things for the friend (who is good) and to do bad things to the enemy (who is bad). His recollection of the dearness of the true good has rekindled his commitment to uphold and to honor above all the distinction between friend and foe. Because it parallels this distinction by rendering good to the good and bad to the bad, the homeopathic conception of justice appears to Polemarchus to be elegantly or beautifully formulated (kalös legesthai 335b1). But Socrates wonders if it is the part of a just man to harm any human being whatsoever (335b2-3). Polemarchus replies, "Well, of course. One is obliged [dei] to harm those, anyway, who are both wicked and enemies" (335b4-5). Polemarchus believes that the righteous are bound to harm the truly wicked, for if they were not to do so they would fail to uphold the distinctiveness of the good. Polemarchus supposes that we demonstrate our allegiance to the good by treating those who are good in as different a way as possible from those who are bad. To shrink from doing bad things to bad men seems a failure of nerve in distinguishing the good from the bad.

It is reasonable of Polemarchus to insist upon the importance of maintaining distinctions. There is indeed something disorderly, shameful, and even ugly in the failure to make proper distinctions, especially distinctions between good and evil. Moreover, we may grant that good and evil are opposites, as are benefit and harm. But it is at this point that we must reconsider the nobility and the reasonableness of moral homeopathy. Is moral homeopathy truly kalon -is it even successful-in upholding, and illuminating, the fundamental opposition of good and evil? Or is it the part of a just man never to harm any human being whatsoever? Now Socrates may well agree with Polemarchus that the noble or beautiful somehow glorifies and illuminates the distinctiveness of the good. But he clearly departs from Polemarchus' view by denying that the kalon consists in anything like a homeopathic operation. Socrates argues, for example, that the result will not be kalon if a statue painter assigns the finest color, purple, to the finest part of the body, the eyes (420c4-d5). If the statue painter were to guide his actions by this homeopathic pattern, it would turn out that the statue's eyes "do not show themselves as eyes at all." Though we might commend the sincerity of this statue painter, who simply wishes to honor what is fine by anointing the finest part of the body with the finest color, we must make no mistake about the seriousness of his shortcoming, for his homeopathic aesthetic obscures rather than illuminates the eye—the very organ in which beauty and nobility in the body is most evident.

Polemarchus, along with the statue painter of Socrates' example, may be faulted for committing the error of *moralism*. Moralism asserts itself in a certain

preoccupation with rewarding good and punishing evil, which causes one to pay insufficient regard to the ontological significance of the thing that is subject to these judgments. So when Socrates asks whether it is the part of a just man to harm or to do damage to any human being whatsoever (hontinoun anthropon), it happens that Polemarchus omits any mention of anthropos-of human being-in his affirmative response (335b2-5). Polemarchus, of course, believes that a homeopathic assignment of benefit and harm is the just and ordinate response to the presence of good and bad. But the grammatical ellipsis indicates that Polemarchus does not properly appreciate the ontological implications of such an undertaking. Polemarchus has not yet faced up to the fact that it is a human being, and not evil itself, that is damaged by homeopathic justice.

As Socrates hastens to point out, however, it is a real being who is made worse when real harm is done; this worsening of a real being involves a diminution in virtue or excellence (areten 335b6-c3). Polemarchus proves to be receptive to Socrates' reminder of the general ontological significance of doing harm: "That must be," he replies (335b12). Socrates now addresses Polemarchus as his comrade or collaborator (ō hetaire 335c1). Together they maintain that when human beings are harmed it is as human beings (i.e., in respect of human excellence) that they are diminished. The comrades dismiss other notions of harm, which are, of course, conceivable but are nevertheless not telling in the present instance, because such notions fail to reach the ontological underpinnings of right and wrong. In commencing a search for these underpinnings, we must remember that the deprivation of contingent goods may not constitute a harm at all. Socrates later notes, for example, that sickliness actually proved to be a boon to another of his comrades, Theages (496b6-c3). Owing to their circumstantial nature, the presence or absence of such contingent goods in any given pattern of distribution does not truly shed light upon the fundamental opposition between good and bad. But Polemarchus believes that it pertains to justice to uphold this opposition. So justice cannot be a merely contingent human good, it must be essential to what we mean by human excellence (335c4-5).19 Once Polemarchus has acknowledged this, he leaves the distribution of boons and blows to others and joins Socrates in the search for this essential, and not merely contingent, human excellence.

Polemarchus' rejection of cleverness has by now radically altered his conception of art. Thus, we find in the present passage that he understands musicianship and horsemanship as professional qualities rather than as exploitable crafts or powers (335c9–13). Polemarchus has learned to concentrate on the essential act of a *technē* as distinguished from its divergent potentialities. He no longer desires to acquire such an equivocal *dynamis*, for he has come to understand that friendly solidarity is in no way enhanced by the exercise of power in this sense. To be loyal to the true friend, which is the good, one must uphold its

fundamental opposition to what is bad. But one can uphold this opposition only by admitting that it is impossible for justice to cause harm or damage to any human being. Justice cannot consist in a power whereby one even alternates good and bad. If justice could ever cause harm, then it would follow that the essential human excellence could cause its own diminution. In that case, the human good would cause the human bad. But if the good were *ever* to cause the bad, good and bad would be only conditionally and not fundamentally opposed to one another. In order to guard against this implication, Polemarchus embraces without the slightest reservation the proposition that justice never works harm (335d13).

It follows that if goodness never causes harm, one can do harm only by parting or turning oneself away from the good. Polemarchus' thymotic passion for friendly solidarity (and thus for upholding the opposition between good and bad) can only be satisfied, then, if he is himself turned steadily toward the good. Informed by this insight, Polemarchus' spiritedness will henceforth become the perfect ally of philosophy, permitting nothing to distract him from the good. Spiritedness must be so allied in order to escape distraction by the juvenile desire "to settle a score." Socrates will later allude to the undistracted attention to the good as the greatest study (to megiston mathēma) and the one most appropriate for the philosopher (504e4-505b4). So we may infer that only philosophy will satisfy and correctly order the thymotic longings of Polemarchus' soul. But we must also note that philosophy can only be pursued if one first recants the childish notion that it is ever just to harm anyone.

Socrates finally invites Polemarchus to become his partner in defense of Simonides (and other wise and blessed men) by fighting anyone who holds that such men deem it right to harm anyone (335e7-10). Although the argument indicates that Simonides could not have meant that it is right to harm anyone, it has of course not settled what he did mean. Socrates and Polemarchus thus become partners in defense of a good they do not themselves possess or know. Their acknowledgment of the dearness of this unknown good is not only evidence of Polemarchus' piety, it also comports perfectly with the continuing conduct of philosophical inquiry. Plato will not wait long before giving his reader an example of the Socratic-Polemarchean partnership in combat, in which the harmony of piety and philosophy is made perfectly clear.

POLEMARCHUS' CONTRIBUTION TO THE REMAINDER OF THE DIALOGUE

Considering Polemarchus' ripening into a representative of true piety, it is only appropriate that his important contributions to the subsequent movement of the dialogue are conveyed in a most modest and quiet manner. We should take notice of these contri-

butions by way of completing our discussion of his role in the *Republic*.

Polemarchus appears twice more in the dialogue. We hear from him again very soon, in the course of Socrates' conversation with Thrasymachus. In the midst of the first half of this conversation, Plato introduces a brief interlude in which Polemarchus confronts his dramatic foil, Cleitophon. The point of this confrontation, I suggest, is to show the reader that Polemarchus is not at all like Cleitophon. One might say that the confrontation between Cleitophon and Polemarchus in this passage supplies the best index of Sccrates' success in his pedagogical undertaking with Polemarchus;²¹ for rather than turning out to be like Cleitophon, Polemarchus proves to be diametrically opposed to him. Yet the comparison of the two young men is inevitable. They both represent tendencies characteristic of the broader political community. Polemarchus, we recall, attempts to impose the force of democratic numbers in the dialogue's opening enactment of the arrest of the philosopher (327c7–14). And Cleitophon advocates the purest form of legal positivism proposed in the dialogue. Cleitophon goes further in this respect than even Thrasymachus; for Cleitophon holds that when Thrasymachus says that might makes right, what he means to say is that right is anything the politically dominant class believes to be to its own advantage (340b6–8). But this clever loophole, which could have insulated Thrasymachus' position from Socrates' refutation, would also make philosophical inquiry impossible and unnecessary. Philosophy is possible or necessary only if one first recognizes the importance—nay, the dearness—of what he does not know. But there is clearly no room for such piety in Cleitophon's world. Cleitophon's perceptionism enthrones every man as measure and absolute ruler of his own private cosmos. Cleitophon's anxiety to escape refuration leads him to annihilate the possibility of recognizing his own ignorance and, along with it, the possibility of ever learning anything. But Polemarchus, in the course of his conversation with Socrates, has come to appreciate the dearness of the unknown good. He could only do this, as we have seen, by first recognizing the limitations of perception, the inadequacy of the seeming good. So it is inevitable that Polemarchus will find himself at loggerheads with Cleitophon, for whom the seeming good is a jealous god. Yet it does no harm to Cleitophon-and in fact it positively benefits Thrasymachus-when Polemarchus testifies that Thrasymachus did nct say that justice is whatever seems to be advantageous to the mighty (340a1-b9). By speaking out and revealing his own sympathies, Polemarchus makes it more difficult for the crowd-pleasing rhetorician to adopt Cleitophon's loophole. In this sense, Polemarchus plays an important role in making it possible for Thrasymachus and Socrates eventually to become friends. Had Thrasymachus followed Cleitophon's impious tack, undermining the very possibility of philosophy, he could never have become Socrates' friend and might very well be regarded as

his enemy (498c6–d1). This, then, is how Polemarchus first answers Socrates' call to do battle in partnership with him. He is Socrates' true ally in upholding the possibility of philosophy and in waging a battle from which everyone might benefit.

The next and last time we hear of Polemarchus in the Republic is at the opening of book 5. His contribution to the conversation in this passage is muted but is, nevertheless, enormously significant, for it is with hardly more than a whisper that Polemarchus moves Adeimantus and the entire inquiry out of the theoretical doldrums. Taking his cue from Polemarchus, Adeimantus articulates their common concern (hemeis 449d1, tithete 450a4), speaking at considerable length of Socrates' faintheartedness (aporrhathymein 449c2) and injustice in "stealing a whole eidos of the account" of the city-in-speech. So it is Polemarchus who propels Adeimantus and the others to put a stop to Socrates' fainthearted speculation concerning the forms of bad constitution. As a consequence, Socrates is compelled to return to the more arduous task of completing the articulation of his model city. From this nearly inaudible yet decisive intervention of Polemarchus there develops the "digression" of books 5-7, in which Socrates and his interlocutors renew their pursuit of philosophy and the good with an unprecedented seriousness.

CONCLUSION

Socrates goes down into Piraeus with Glaucon both to pray and at the same time to see. I have suggested that the desire to reconcile reason and piety, which is manifest in this twofold intention, is a just and fitting response to what is perhaps the most vital feature of the human condition. As Socrates maintains, human beings are contingent creatures who nevertheless possess the ability to share somehow in the noetic activity characteristic of the divine. I have acknowledged that the departure of Cephalus, who abandons inquiry in order to attend to certain sacred rites, induces us to consider the tension between piety and reason. But I have also suggested that the textual passage centered on this focal event reveals that it is not Cephalus but Polemarchus who, together with Socrates, deserves to be regarded as the dialogue's exemplar of piety.

The textual evidence suggests that Socrates intends to unmask Cephalus as a representative of rationalism rather than piety. Following this evidence, I maintain that we must reject the widespread notion that Cephalus' departure constitutes Plato's dramatic indication of the fundamental incompatibility of philosophy and piety. Instead, we would do better to conclude that Cephalus' departure dramatizes the incompatibility of Socratic philosophy and rationalism. Because rationalism is also hostile to genuine piety, Cephalus must take his leave before true piety can be introduced and reconciled with Socratic philosophy.

I have argued that it is Polemarchus whose devel-

opment dramatizes the character of genuine piety. Polemarchus enters into the conversation advocating a blind faith in the authority of the wise man, Simonides, as a basis upon which to decide the question of justice. But Socrates ripens and fortifies this piety by bidding Polemarchus to consider "what it is you say that Simonides said correctly in speaking about justice." It might seem that such a question could not be seriously intended to initiate a philosophical inquiry because it takes for granted that in some sense Simonides is right. This assumption, I agree, is by no means beyond all possible doubt. But the analysis I have offered suggests that it is precisely the piety—or "bias," if one insists—of this form of question that underlies the love of wisdom. Piety, thus understood, keeps philosophical inquiry steadily concentrated upon the good, undistracted by other considerations.

I have suggested, by the conduct of this inquiry, that one can best approach the study of Plato in the same way. In order to learn from even the closest study of small passages of Plato, the reader must hold dear what he does not know. The reader must be supremely attentive (we might even say reverential) toward the text. This requires respect for the text's structural articulation and for the dramatic context of every argument. With this reverential approach, one recognizes that logical ambiguity may serve as an impeccable conveyance of significance; that silences may speak eloquently; and that blunders that "would shame an intelligent high school boy" may become for the attentive reader critical pointers to an underlying, coherent teaching. The authority of logic is not thereby rejected. Instead, the reader assumes the responsibility to remain alert enough to supply the suppressed premise of an enthymeme. Piecemeal logical analysis is thus transcended in the direction of the holistic consideration of logographic necessity. Such a transcendence sustains the virtues of reason as against the shortsightedness of rationalism. In this way, moreover, the student of Plato adopts Socrates' recommendation to become both judge and advocate at the same time. On the other hand, I would suggest that the more perfectly neutral inquiry "Is Plato right or wrong?" will inevitably distract one's inquiry from the good merely in order to settle a score with an author. Instead of serving as a portal to the good, the author becomes in this case the principal object (and hence, inevitably, an obstacle) to what was intended as a philosophical investigation.

I have argued that Polemarchus' spiritedness is initially inclined toward such score settling. Polemarchus passionately strives to achieve the most rigorous possible discrimination between friend and foe and thus hits upon the notion that to do right one must benefit friends and harm enemies. This notion in turn tends to provoke a dreadful appetite for the power to dispense benefit and harm. Socrates opposes this appetite, first, by disengaging Polemarchus' enthusiasm for this notion of justice. He does this by showing that its practice implies a status for justice that is inglorious and which, moreover, casts doubt upon

the goodness of friendly solidarity. Socrates then helps Polemarchus to identify the proper object of his heart's desire in the true good, even when the good is not yet apparent as such. I maintain that Polemarchus' acknowledgment of the dearness of the unknown good constitutes the definitive indication of genuine piety in the conversations of book 1. Because only the disposition to hold dear what one does not yet know can keep one mindful of the need to learn, we may say that piety in this sense is indeed a docility of soul, not in the mistaken current sense of intellectual passivity but in the original sense of docilitas, or "learning readiness." Such docility, moreover, is the polar opposite of the hubristic resistance to learning, or amathia, which is the culprit so often obstructing philosophical inquiry in Plato's dialogues. With Polemarchus' recognition of the dearness of the unknown good, his spiritedness is set squarely to the task of upholding the opposition between good and bad. It remains only for Socrates to persuade Polemarchus that this opposition cannot be maintained unless justice is oriented steadily toward the good; as a consequence, Polemarchus learns that the just man cannot seek to harm anyone.

Thanks to Socrates' examination of Polemarchus, we are better able to appreciate the character of piety or reverence. Typically, this quality is mistaken as a merely submissive or passive deference to higher authority. But it is more properly understood as an acknowledgment of the dearness of (and hence as a receptivity to) the unknown good. Such receptiveness is manifest not in blind faith but in a questioning that is searching even as it anticipates the truth or goodness of the thing questioned. Piety in this sense is characteristic of one who is at the same time both judge and advocate, and yet neither skeptic nor dogmatist. I thus conclude that it is Socratic piety (and not its diametric opposite, Cartesian doubt) that is the disposition most congenial to the love of wisdom. Piety, we may say, is not merely necessary to thwart the persecution of philosophers. Piety is essential to the pursuit of philosophy.

Notes

- 1. Citations in this essay to the *Platonis Opera* (Oxford, 1900–1907) supply the standard Stephanus divisions of page, section, and line. Unless indicated otherwise, all references are to the *Republic*. The translations are my own.
- Of course, to say that this understanding is accessible is not to say that any particular student will in fact gain access to it; but it is good pedagogical practice never to lose sight of this possibility.
 - 3. See also Strauss 1965, 1–31 idem 1979.
- 4. As it happens, Stephanus divides this passage into 44 sections (327a–336a), with Cephalus making his departure in the last line (331d9) of the twenty second section. The eidetic standing of this collection of 44 sections may be better appreciated if one notes that it already comprises a sufficiently diverse assemblage of interlocutors to reflect something of the character of the *Republic* as a whole. Cephalus, Polemarchus, and Socrates may be said to correspond to the three classes (money-maker, warrior, and philosopher) of Socrates' model city and thus may be said to anticipate the problem of the

wholeness of its politeia and perhaps of politeiai in general. Euben is right to observe that the triplet of Cephalus, Polemarchus, and Thrasymachus does not correspond to the threefold structure of the model city (1990, 248). Thrasymachus, after all, is not a philosopher. Of course, neither Euben's observation nor my claim concerning the significance of the passage upon which I propose to focus is meant to imply that Thrasymachus is unimportant. On the contrary, I would suggest that his presence in book 1 poses the important problem of philosophy's need to incorporate rhetoric while at the same time separating itself from sophistry. But one cannot talk of everything at once: if we try, we shall find ourselves impaled upon the "forest" horn of the forester's dilemma and reduced to the formulation of cocktail-party generalizations. The challenge is to find a way to think and talk about the details nonreductively, that is, while preserving their relationship to the whole.

- 5. I am thus inclined (with Brann 1967, 88, 113) to view the narration of the Republic as an example of the Pythagoreanism of Plato's Socrates. Pythagoreans sought to cultivate a capacity for just this sort of recollection. According to the testimony of Iamblichus, "a Pythagorean man does not arise from his bed before he has recollected what happened yesterday" (Life of Pythagoras 163.20, cited in Brann 1967, 113). This recollection would be a private exercise. Hence, Socrates' narration of Plato's Republic may be likened to a dramatic soliloquy. Nevertheless, one might wish to maintain that Socrates does have a circle of auditors who listen to his rehearsal of the conversation of the Republic, as seems suggested by a possible (though by no means necessary) interpretation of the opening scene of the Timaeus. There is, however, no evidence internal to the Republic to support such an interpretation.
- 6. An example from the present passage will suffice to illustrate this point. Socrates expresses personal admiration (egō agastheis) for Cephalus in a narrative gloss, when the latter reckons that a good way of life is more important to personal happiness than the capacity to enjoy sexual pleasures (329d7). I take it, then, that we may understand this admiration as sincere, if provisional. On the other hand, it is in direct discourse with Cephalus himself that Socrates describes as pangkalōs (i.e., beautiful in every respect) Cephalus' view that money is chiefly useful in making it unnecessary to commit injustice (331c1). Hence, I am inclined to take this evaluation with the proverbial grain of salt. In fact, Socrates' pretentiously exhaustive word pangkalōs might be understood as insinuating that in some decisive respect Cephalus' notion is quite the opposite of beautiful. The sequel, as we shall see, confirms this insinuation. Such is the character of Socratic irony.
- 7. Socrates employs this te... kai hama formulation throughout the Republic to indicate the pairing of things that might well be regarded as irreconcilable. He remarks that the discovery of such a pairing naturally awakens dialectical thought (524e2-5). Sometimes, however, the "longer path" of dialectic is postponed and a provisional understanding is accepted merely by hypothesis (as indicated, for example, at 436d4-437a9). For a discussion of the pedagogical significance of this postponement of dialectic and its importance for the interpretation of the political institutions of Socrates' model city, see Dobbs 1994.
- 8. The case that Socrates' conventional religious practice is more than window dressing is made by Lloyd-Jones, among others, who notes that in late fifth-century Athens many educated people realized "that the traditional beliefs and observances were by no means incompatible with a more sophisticated kind of theism; belief in a god or gods that had been defined by the doctrines of philosophy could easily go together with the maintenance of the ancient worship" (1983, 134).
- 9. Adam points out that in Attic literature the phrase the goddess regularly denotes Athena (1902, 1:62). Nevertheless, he rightly considers evidence internal to the Republic to be more compelling than regular Attic usage in interpreting this phrase; this internal evidence suggests an identification of Socrates' goddess with Bendis. I agree with this assessment,

but venture further to say that it would be mistaken to substitute terms, reading Bendis instead of the goddess in hope of improving upon Socrates' use of an ambiguous phrase. It is more prudent to keep open the possibility that the ambiguity implicit in Socrates' speaking of "the goddess" is meaningful precisely as such. While the future participle proseuxomenos plainly indicates Socrates' intention to pray, his ambiguous diction in the case of the intended recipient of these prayers (Socrates himself never uses the name Bendis) indicate that he does not mean to pray to Bendis qua Bendis. Such ambiguity may be the most economical means of conveying the complexity of Socrates' recognition of this cult as reflecting an aspect of some unknown, though necessary, god. This would also explain why it appears that "Plato apparently regards the content of prayers as more important than to whom they are addressed (Jackson 1971, 34).

10. Of course, some readers are inclined to dismiss Socrates' testimony as ironic. But a careful review of the text will find no warrant for such a dismissal in this case. In the passage typically cited as evidence of his ironic intent, Socrates is commenting on the difficulty of persuading his fellow citizens of the importance of persisting in his crossexaminations. "If I say that it is impossible for me to keep quiet because this is to disobey the god, you will not be persuaded by me, on grounds that I am being ironic" (hôs eironeuomenoi, 37e5-38a1). It should be noted, however, that the hos-plus-participle construction, which Socrates employs in the final clause of this sentence, is used in Greek to give the reason or purpose of someone other than the speaker without implying that it is also the speaker's understanding of the situation (see Goodwin, Greek Grammar §1574). Hence, there is no reason to construe Socrates' statement as an admission that he is being ironic; his statement merely affirms that others will suppose that he is. Perhaps many in the democratic jury will doubt Socrates' veracity in this matter because they are not themselves inclined to take the oracle at Delphi (or the voice of any other daimon) very seriously but confide instead exclusively in their own autonomous will. So it happens that Meletus, the poet, succeeds in his indictment of impiety against Socrates only with the support of Anytus, the democratic politician. The democracy naturally treats complicity in the rearing of oligarchs as the functional equivalent of impi-

11. Morgan makes an intriguing point, noting that inasmuch as he is a metic, Cephalus resembles the human soul, which also is a sojourner "living temporarily in the world of history, politics and moral conduct, . . . always anticipating its future and the possibility of permanent citizenship among the divine" (1990, 100). I think this suggestion is even more pertinent to Cephalus' son, Polemarchus, who is also a metic but whose anticipation of the destiny of his soul is not marred by his father's anxieties.

12. See n. 6.

13. The term rationalism is of course a modern coinage. Ancient authors described this tendency in a more frankly pejorative manner, as a manifestation of hybris or recklessness. Such value-laden language testifies to the clarity of the divergence, in the view of these authors, between what we call rationalism and the true virtue of reasonableness. Dobbs (1987) and Saxonhouse (1988) may be consulted for further discussion of the ancient appreciation of the discrepancy between the virtue of reasonableness and the recklessness of rationalism. It is my intention here to present Socrates as a positive case in point, for Socrates' examination of Polemarchus, which is remarkably deferential to the authority of Simonides, remains a model of reasonableness, though it certainly would not pass Cartesian muster. As I shall suggest, Socrates' conversation with Polemarchus thus reveals that piety is more congenial to philosophy than is radical doubt.

14. In his attempt to argue that the sacred is not a concern for the philosopher but only for the city, Benardete fails to take account of the significance of this linkage of advocacy and criticism (1989, 82–83). Specifically, he faults Glaucon (who recalls Socrates' acknowledgment of a "sacred task to help justice," 368b7–c2, 427e1–2) for failing to appreciate the

distinction between searching for justice and helping justice. But it is reasonable for Glaucon to combine these concerns; by yoking the tasks of searching and helping, he faithfully executes Socrates' intention that they be "judges and advocates at the same time" (348b3-4). Although there is much to admire in Benardete's book, I maintain that we will never adequately appreciate the philosophical significance of Socrates' undertaking in the *Republic* until we acknowledge the importance of his pairing of piety and reason, advocacy and criticism.

15. Thymos, as Aristotle explains it, is the power of soul whereby we love or hold something dear (philoumen); it is indomitable and prerequisite to a capacity for rule and for independence (see Politics 1327b18-1328a16; cf. Plato, Republic 374a12-b2). Zuckert writes pithily that "Socrates identified thymos, or spiritedness, as the psychic origin of distinctively political action. . . . [In Socrates view,] political order does not arise directly from one's own desires or [even from] the means best calculated to realize those desires but rather in reaction to the excessive desires of others" (1988, 2-4). Socrates treats thymos thematically, though incompletely, in book 4 of the Republic (437a4-10, 439e2-441d4). An adequate consideration of the classic understanding of thymos, the seat of the emotions of love, anger, and indignation (to say nothing of the spirit of independence and self-sacrifice), would require a careful study of these and other passages.

16. Strauss notices that Hamlet's famous soliloquy illustrates the tendency of thymos to shift from "the justified indignation about injustice . . . into the unjustified indignation about unrequited love. This is perhaps the deepest secret of spiritedness and therefore at least one of the deepest secrets of the Republic" (1989, 168). To this remarkably incisive comment I would like merely to append the observation that in some sense the opposite of unrequited love is the discovery of an unknown friend. Such a discovery, which Polemarchus is about to make with Socrates' assistance, will naturally counter the thymotic tendency toward unjustified indignation. Moderation is thus nourished by Socratic piety.

17. Piety in this sense is utterly alien to the conduct of Euthyphro, the pseudospokesman for piety in the Platonic dialogue named after him, for Euthyphro plunders his cultic expertise to claim for himself the very prerogatives of divinity (see Strauss 1989, 197-98). Readers interested in the relationship between the Euthyphro and the Republic should also observe that Euthyphro's final definition (that piety consists in commercial transactions with the gods, 14c4-15b3) would lend credence to Cephalus' decadent piety. Socrates' refutation of this final definition, then, would likewise undermine Cephalus' position. But Socrates' refutation, which annuls Euthyphro's designation of self-serving commerce as the attendance (therapeia) that is due the gods, leaves open the possibility that some other relationship of attendance-noncommercial but as yet unspecified-might well constitute the true character of piety (see Euthyphro 13a1-2). It seems to me that Polemarchus' acknowledgment of the dearness of the unknown good qualifies as this other kind of attendance. Thus, the Euthyphro may be said to culminate in the refutation of Cephalus and the anticipation of Polemarchus. We may expect, then, that the Republic will involve a further elaboration of the consideration of piety initiated by Plato in the

18. Justice may yet regain its prominence in the case that one is both sick and at sea at the same time, for then an appeal would have to be made to some third man beyond the physician and the helmsman to determine which of their possibly conflicting prescriptions to follow. Because of the familiarity with seasickness typical of a seafaring people like the Greeks, we may surmise that this implication is not entirely foreign to Socrates' intention in the choice of his examples.

19. Failure to consider this point has led many readers to the erroneous conclusion that it is foolish of Polemarchus to suppose that justice exhausts what we mean by human excellence. Of course, certain conceptions of justice are less than exhaustive of human excellence; some conceptions are even at odds with it. Justice understood as the imposition of arithmetic equality in external goods, for example, might well subvert the excellent endeavors of some of the more talented members of society. But such conceptions of justice are blunted by the incongruity of their concern for contingent goods and so cannot penetrate to the depth that Socrates' present argument requires. Polemarchus, then, follows the train of Socrates' argument better than many of his critics, for once one is required to consider the issue ontologically, it may be necessary as well (kai tout' anangkë 335c5), as Polemarchus agrees, to accept righteousness as the good essential to human being. Moreover, if it is true that the central books of the Republic vindicate the righteousness of philosophy, then Polemarchus' formal identification of dikaiosynë and human excellence will prove to be robust enough to withstand the rigors of the highest intellectual peaks of the Republic.

20. Failure to perceive this development in Polemarchus has led many readers to complain of the inconsistency between the treatment of arts in the present passage and the Autolykean account offered only a few pages earlier. But Polemarchus has consistently viewed the arts from the perspective of justice, as he understands it. As his understanding of justice undergoes change, so does his view of the arts. There is no glaring oversight here, as some readers imagine, but only a good example of human learning.

21. If one wishes to go outside the Republic for confirmation, there is of course Socrates' remark in the *Phaedrus*, affirming that "Polemarchus has turned toward philosophy" (Phaedrus 257b3-4).

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THOUGHTLESS ASSERTION AND POLITICAL DELIBERATION

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Predominant views of political deliberation represent it as a matter either of "contemplation without interest" or of interest without contemplation. Whether we claim that political thinking can transcend power or that it is simply a vehicle for it, we abet a nihilistic political culture and authorize the thoughtless exercise of power. What each view denies—or insufficiently explores—is thinking's capacity to transfigure "interest" and power without pretending to transcend them. By contrast, political perspectivism incorporates multiple perspectives in an effort thoughtfully to respond to the will-to-power that attends our location in the world. We need political spaces if we are to become responsive agents of power, because we often can neither recognize the effects of our activities without the benefit of others' perspectives nor alter those effects by our efforts alone.

Thought must consider the process that forms it and form itself from these considerations.

-Foucault, Language, Counter-memory, Practice.

Taking the canonical texts of Western political theory as our guide, we could conclude that the activity they represent, political thinking, is at best paradoxical and at worst impossible. With few exceptions, thinking (usually equated with philosophy) and politics (derogatorily associated with power and opinion) have been judged antithetical. This conclusion is moored in contestable beliefs about the relationship of power and thinking, body and mind. These assumptions both impoverish thinking and consign politics to the thoughtless exercise of power. Reading Friedrich Nietzsche and Hannah Arendt together, I challenge these views and offer another perspective on political theorizing.

The body as figure is one of Nietzsche's primary vehicles for disparaging the views of others, as well as a means of enacting his own, alternative way of thinking.1 Nietzsche's critique of objectivism, or "contemplation without interest" is well known (1967c, 118-19). But his critique of subjectivism or interest without contemplation, is often ignored, so that Nietzsche is still often converted into an advocate of some variant of irrationalism or ruthless assertion. To interpret Nietzsche's writings in this way is, however, seriously to misconstrue them. As Nietzsche wrote in *The Gay Science*, "the greatest danger" is "the eruption of arbitrariness in feeling, seeing, and hearing, the enjoyment of the mind's lack of discipline, the joy in human unreason" (1974, 130). In other words, the greatest danger is the repudiation of thinking and its consequences. Both "contemplation without interest" and interest without contemplation are aspects of such thoughtlessness and manifest the unitary point of view that Nietzsche associates with nihilism (1968, 70). By contrast, as a perspectivist, Nietzsche is attentive to will-to-power (evinced in the plurality of bodies and their effects) and its potentially affirmative relationship to thinking's capacities. Perspectivism is located, interested thinking and its goal is the incorporation of multiple

perspectives in the service of controlling "one's Pro and Con" to refashion who one is becoming (1967c, 119). The transfiguration of the *body* proves to be a potent metaphor in pursuit of perspectivism's goal.

Locating thinking, or "reason[ing] the particular," is usually conceived as a matter of judgment, whether aesthetic, moral, or political. Hannah Arendt has furthered our understanding of this reflection attuned to particulars.² While there can be no doubt that Arendt chose Kant as her primary touchstone for understanding thinking and judgment (Arendt 1977, 197–264; idem 1978; idem 1982), the heavy reliance on him obscures a rich, Nietzschean vein in her political theory.³ Contrary to the most common view, Nietzsche may further our understanding of the ways of thinking that make specifically political power and deliberation possible.⁴ He thereby prefaces (though he himself does not pursue) a revaluation of politics.

In Arendt's view, Kant needs her for his political philosophy to appear (Arendt 1982, 7-8). Nietzsche also needs her if we are to conceive the significance of his writings for political thinking. Indeed, if perspectivism is to be actualized or to avoid repugnant forms (Fowler 1990), it must (contra Nietzsche) be political perspectivism, because (as Arendt teaches us) politically constituted commonality is a condition of the appearance and worldly effects of a plurality of perspectives. In short, perspectivism is necessarily a political practice.⁵ But similarly, Arendt needs Nietzsche-and in ways she does not acknowledge. I wonder whether the political spaces that Arendt commends can be created, sustained, and esteemed without keeping before us what Nietzsche teaches about the relationship between thinking and power. Contrary to Arendt, I contend that political thinking must be perspectival, because only by so responding to our bodies, "private" locations, and their effects can we initiate the political spaces in which plurality appears and is, if transfigured, preserved. In short, word and deed are political only when they seek to enact

Both Arendt and Nietzsche challenge predominant views that equate thinking with knowing and thus

with the search for truth, rather than meaning (Arendt 1978, 14-15, 57-65).6 Arendt follows Nietzsche in disclaiming the distinction between "reality" and "appearance" (e.g., Arendt 1978, 10; Nietzsche 1954, 485-86; idem 1968, 305-6) but her characterizations of "mental faculties" sometimes verge on reinstating the distinction. Nietzsche's corporeal thinking registers thinking's effects in the world. Arendt wondered whether thinking, as an activity that disturbs given or thoughtless truths, might have such effects (1971, 418; 1978, 71), but her views about the political relevance of the body unduly circumscribed her efforts to elaborate them. Just as Arendt's acute sense of the relationship between political spaces and the appearance of a plurality of perspectives enables us to see the political-theoretical significance of Nietzsche's writings, his transfiguration of the relationship between body and mind clarifies what worldly, political effects thinking might have. Together, Nietzsche and Arendt help us (re)imagine political theorizing, and the enhanced political vision that emerges from an encounter between their writings attests to the fertility of such political perspectivism as mutual transfiguration among different locations and views.

TRANSFIGURING THE BODY OF THE "THEORETICAL MAN"

An interpretation claiming an affinity between Nietzsche's writings and political theorizing may seem implausible, given Nietzsche's animosity for the "theoretical man" Socrates (e.g., 1967a, 82-86, 94-98, 109-14; 1954, 473-79). Nietzsche claims that Socrates and the dramatist he inspired, Euripides, are responsible for the degeneration of "the old Marathonian stalwart fitness of body and soul" that once distinguished Athens (1967a, 86). The corruption of tragedy marked this decline. The myopic Socrates turned his "one great Cyclops eye" on tragedy, "an eye in which the fair frenzy of artistic enthusiasm had never glowed," and shunned it (p. 89). According to Nietzsche, Socrates found tragedy neither true, nor beautiful, nor (for that matter) useful. The "artistic enthusiasm" that in Nietzsche's view, Socrates did not know and Plato later would not countenance Nietzsche honors because he believes it both reflects and best captures life: "for life is based on semblance, art, deception, points of view, and the necessity of perspectives and error" (p. 23). Already we sense the doctrine of perspectivism, which claims that there are "no facts . . . only interpretations" (1968, 267).

In later works, Nietzsche joins together the artist and theorist, as it were, claiming that criticism always entails affirming and creating: "We can destroy only as creators" (1974, 122). Nietzsche's objection to the "theoretical man" lies in the latter's denial of this creative aspect of thinking—in Nietzsche's terminology, denial of his will-to-power. Both the artist and the theorist are "critical" (1967a, 93–98), but to Nietzsche's way of thinking, the artist is more so because

he understands that living necessarily entails both interpreting and affecting the world and therefore acknowledges and creatively directs his will-to-power. The theorist who emulates the artist, Nietz-sche believes, is less likely to delude others or himself.

The first intonations of Nietzsche's alternative way of theorizing are found alongside this critique of Socrates when he celebrates the dramatist who possesses sufficient power to transform himself and speak out of the bodies and souls of others (1967a, 64). This foreshadows Nietzsche's affirmation of the philosopher "who has traversed many kinds of health, [and] has [thus] passed through an equal number of philosophies" (1967c, 96; 1974, 35). Nietzsche's appreciation for this dramatic talent—"to see oneself transformed before one's eyes and to begin to act as if one had actually entered into another body, another character"—prefigures perspectivism (1967a, 64; 1974, 132–33). The dramatist's talent is the raw material for new thinking habits.

Nietzsche persistently uses corporeal and sensorial metaphors, often unconventional ones, to describe and distinguish various ways of thinking.8 This poses the question of why, when faced with the "need for a new world of symbols," he, like the Dionysian, called "the entire symbolism of the body into play" (1967a, 40). I suggest that thinking through the corporeal registers Nietzsche's alternative way of theorizing. If, as he claims, "perspectivism is only a complex form of specificity" (1968, 340), Nietzsche's use of the body is a potent way of attending to the locality and effects of thinking. He continually transfigures the body, often by telling the story of his own body, taking all the senses and bodily processes together, playing them against-and with-one another, to convey the constitution and transformation of viewing and thinking alike. Perspectivism is thinking incorporated and analogized by embodied, power-responsive vision.

Speaking generally, will-to-power secures the beliefs of, and practices fostering, a way of life (Nehamas 1985, 74–105, 141–69). I use the word *location* for this amalgamation of embodied habits and beliefs. Will-to-power is relational and its effects are engendered by simply living our locations, by our body's ways of being in the world. Consequently, location should be thought of not as a place, but as a space which is continually changing with how it is linked at any given moment to other locations. In enacting these spaces for ourselves, we are inevitably engaged (whether we are aware of it or not) in challenging other locations or in suppressing and destroying them altogether. The views and practices that prevail in a body politic reflect and reinforce patterns of congealed will-to-power. These often invisible patterns are generated by "governing powers" manifest in the norms, practices, and efficacy we bring to bear upon our own and others' conduct (Foucault 1983, 212-26). These powers are not exercised exclusively by government but are borne and extended by subject-citizens and evinced by the traces they leave on

the surface of all to which their *bodies* and activities relate them (Foucault 1977, 165–96).

Even when shared ends bind us to others, differential effects, fashioned by disparate histories, come into view because we carry interpretations attesting to the particularities of our body's manifestations and requirements. Our pursuits resonate with, extend, resist, or are stifled by force fields across a body politic. Conflicting, unequal effects are continually manifest in subject-citizens' mutual relations and engendered by their relation to norms, the will-topower that accrues with them, and the psychological and material consequences that flow from this positionality or location. Our capacities and efficacy are increased or decreased by our degree of conformity to the norms embodied in civil codes even though we do not possess these powers but bear them. Our efficacy is contingent upon our ability and willingness to flow with them. In doing so, we may have harmful effects upon others who are differently aligned in relation to extant patterns of will-to-power. As a result and with our aid, selves and world are made pliant to some persons and projects, recalcitrant toward others. For instance, our body's appearance (e.g., how it carries "race" and "gender") calls forth responses from others and also has (often unintended) effects upon other bodies and the worldeffects that emanate from the activities and the ends that it "needs," demands, or is "empowered" to acquire. All of these are a manifestation of our unavoidable will-to-power, of which Nietzsche says, "This necessary perspectivism by virtue of which every center of force . . . construes all the rest of the world from its own viewpoint, i.e., measures, feels, forms, according to its own force" (1968, 339). When we deny our will-to-power, we ignore the effects attending the perpetuation of our bodies and locations in the world—in which case, politics and culture become matters of thoughtless assertion.

If our thoughts and bodies are so bound, it does not necessarily follow that thinking therefore can only reflect our body and its interests (as so many have concluded it does). Thinking, especially when we attend to location (the continually changing space in which our body appears) can enable us to alter our "interests" and effects, though we cannot altogether transcend them. While we may be beings who are always becoming, we are nonetheless subject to patterns of will-to-power—to specific habits and related limits of perspective—that elude our thinking when we are without the aid of perspectivism and (albeit to a lesser degree) even when we avail ourselves of it. The corporeality of Nietzsche's thinking conveys the weight of the world and our bodies while avoiding determinism (Nietzsche 1967c, 129; see also Blondel 1991, 201-38). The genealogist understands the willto-power that inscribes and incorporates preponderant dispositions and habits, but the genealogist also knows that there is a degree of contingency (and thus human agency) within processes of inscription and incorporation that hold out the promise of alterability (e.g., Foucault 1977, 148-64). Nietzsche thinks corporeally to convey both the possibility of transfiguration and the weight of all that is poised against it.

Philosophers, Nietzsche believes, have traditionally been predisposed to disregard the *body* or to neglect its role in thinking. ¹⁰ He identifies the root of this "peculiar philosophers' irritation at and rancor against sensuality" in the ascetic ideal (1967c, 106). Philosophy's asceticism forecast the conception of thinking that it valorized. The ideal that affirmed "voluntary deprivation, self-mortification, self-flagellation, [and] self-sacrifice" also sought to "contemplate without interest" (pp. 118-19). Nietzsche regards the effort to do away with interestedness and the body as an attack upon thinking, because to conceive of a thinker who contemplates without interest demands thinking of a disembodied eye "turned in no particular direction, in which the active, interpreting forces, through alone which seeing becomes seeing something, are supposed to be lacking" (1967c, 105, 118-19). The objectivist's denial of thinking's creative aspect and interested character ends in the atrophy of thinking, when the "objective man" (the "philosopher," as well as his heir, the "scientist") takes "mirroring" as his ideal and submits to "whatever wants to be known" (1973, 115). Mirroring, the suspension of "interestedness," is actually a refusal that eventually becomes an inability to think. Contemplation without interest, like "all great things [that] bring about their own destruction through an act of self-overcoming," ends in interest without contemplation and, at bottom, in meaninglessness, nothingness (1967c, 157, 161). As we shall see, to think without interest or will-to-power is impossible, in Nietzsche's view, because to think without interest is ultimately to be thoughtless (pp. 103-6; 1973, 131).

Perspectivism represents Nietzsche's revaluation of the relationship between mind and body, thinking and power; it is "interestedness" transformed through thinking. The perspectivist, by bringing other views to bear upon habits and beliefs, heightens attention to the limitations engendered by a particular location. What is unthinkingly accepted or enacted may become unsettled by incorporating alternative views. The relationship between thinking and power is also transfigured, for perspectival thinking is not subject before will-to-power or merely a vehicle for expressing "interest." Rather, will-topower is potentially transformed through thinking, and the latter, in turn, may be ennobled by the power of others' perspectives. Nietzsche reconceives "objectivity" in his penultimate expression of perspectivism: "there is only a perspective seeing, only a perspective 'knowing'; and the more affects we allow to speak about on thing, the more eyes, different eyes, we can use to observe one thing, the more complete will our 'concept' of this thing, our 'objectivity' be" (1967c, 119). The "definition" or "essence" of anything, Nietzsche maintains, is finally an opinion about a thing and an effect of interpretation (1967c, 79-81; 1968, 301-2). Nietzsche's refiguring of "objectivity" resists the epistemological commitments of discourses structured in accordance with a monologic truth. Instead, he favors thinking as the incorporation of multiple and diverse views in pursuit of fuller opinion, the proliferation of meanings, and the sustenance of a plurality of perspectives. Will-to-power thereby becomes less reactive and impositional, more thoughtful.

When Nietzsche narrates the expansion and change of his bodily processes and senses, he is figuratively conveying his effort to alter his views by thinking or incorporating other perspectives. Bringing "many eyes" to bear on any one thing requires "the ability to control one's Pro and Con and to dispose of them, so that one knows how to employ a variety of perspectives and affective interpretations" (1967c, 119). Nietzsche's discussion of brief habits begins to suggest (we will need Arendt's perspective to understand more fully) how we may come to critically evaluate and transform our locations and their effects, if never altogether surpass them. Nietzsche loves brief habits because he considers them "an inestimable means for getting to know many things and states, down to the bottom of their sweetness and bitternesses" (1974, 236-37). To appreciate something, one must experience it with a passionate faith, with the belief that one has found the thing that will provide lasting satisfaction. This is so because "untruth is a condition of life" (1967a, 23; 1973, 17). It is not merely a matter of not being able to subject all of our beliefs and habits to criticism all at once. Even if one could, to do so would be prohibitive. Immersion in the world and (at least temporary) partiality make seeing and thinking possible. Affirming one's historicity is a condition of new thinking and action (e.g., Warren 1988, 192–94).

Either a life of enduring habits or one without any habits at all, according to Nietzsche, would prove inhospitable to thinking. On the one hand, a life of enduring habits is monolithic and without variation; the new, the original, and the many (and thus a plurality of perspectives) are its definitional antitheses (1954, 639). One may become identified with one's location until its perspective is all that one knows; and then, what is habitual is regarded as unalterable. On the other hand, a life without habits precludes the digestion and incorporation of experiences (1983, 62). Regarding one's life as a performance of "perpetual improvisation[s]" ceaselessly seeking to evade the habitual is predicated on an inability or unwillingness to discern the effects that accumulate with one's location in the world (1974, 237). Moreover, to resist or deny one's habits and the "materiality" of one's location is to overlook how will-to-power and its effects not only limit vision but may also enhance it. Perspectivism requires one neither to live one's body and location thoughtlessly nor to believe that they are so plastic as to be transformed at will. Rather, it calls for altering "who" we are or how we bear the effects that accrue with "what" we are (or our embodied location in the world) first by striving to recognize their potential brevity. 11

Learning to love and desire something is to turn it into a habit. Like Socrates, who found eros and thinking inextricably bound, Nietzsche maintains (though with different effects) that "all great problems demand great love," love being something that one must learn by first (often with difficulty) detecting and distinguishing something and then "tolerat-[ing] it in spite of its strangeness" until it finally becomes a part of who one is: by offering hospitality to what initially appears strange, we learn to love and to think (1974, 262). Both require responsiveness to "strangeness"—a willingness to acquire new habits or (at the very least) to have one's own habits questioned. Brief habits are kept from becoming thoughtless truths when we incorporate views that open up the possibility of transfiguring our ways of life.

The love that one feels for one's problems is indicative of a personal relationship to one's thoughts (Nietzsche 1974, 283). Personal thoughts threaten to become cold and impersonal when they take on the appearance of unassailable truths and their temporal specificity and emanation from particular locations and lives is no longer palpable (1973, 201). Such truths simultaneously attest to the power and impotence of thinking; to the destructive yet compelling character of its deracinated forms. Boundless thoughts and the beliefs and habits that appear to command universal assent or consensus are remarkably potent: they rule minds. The individual mind, in its weakness, becomes subject before such thoughts and, forsaking its own, loses a plurality of perspectives. Thinking is at risk if the particulars or habitual character of a thought can no longer be fathomed. There is danger in brief habits becoming "immortal truths" (Nietzsche 1973, 201; see Arendt 1971, 435; idem 1978, 176). When the latter congeal, we approach nihilism.

OUR NIHILISTIC POLITICAL CULTURE

Thoughtlessness is a primary aspect of what Nietzsche calls nihilism. Nihilism exists when there is a predominant sense either that everything is permitted because there is no basis for choosing between things or that all such bases are arbitrary, in turn possibly leading either to resignation and the feeling that all is meaningless or to an aggressive indifference. One of nihilism's chief effects is a crisis in human action (Warren 1988, 13-45), a dissipation of thoughtful efficacy. We are thoughtless when we do not or cannot conceive or recur to the effects attending the perpetuation of our locations in the world. The consequences of doing so are enormous. When the contours of a particular life and location are not perceived because the bounds of one's views are not within the purview of thought (Nietzsche 1973, 114-16), then what is actually bound and limited will appear boundless and rule one's mind. Indeed, if one's way of life is a prevalent one, these views receive confirmation everywhere one looks and will appear commonsensical. A unitary perspective prevails by virtue of our incapacity to rejoin what is strange, challenging, or invisible to us.

Another consequence of such thoughtlessness is a return of the repressed. Will-to-power, once denied, now may appear everywhere and seems to overcome thinking. This situation is often precipitated by those whose views are dismissed or elided when prevailing yet limited views are represented as boundless; "genealogical" cultural criticism documents the will-to-power evident when particular perspectives on the world mask themselves as universal or commonsensical. Such critiques are often regarded as an assault upon the possibility of "objectivity" however conceived. This is the state of affairs characteristically named "nihilist" (and "Nietzschean") in the United States (e.g., Bloom 1987, 141–240).

Thus, the contemporary intellectual, cultural landscape appears to be divided among distinct camps. 12 On one side, are "traditional" philosophers and scientists, who maintain that one who thinks can surpass interest, that is, break the ties that bind thinking to power. (The latter is sometimes equated with domination and coercion, more often with the "subjective" and "irrational.") Indeed, to transcend will-to-power and to dissipate or dispel interest are thought to be among the chief characteristics and virtues of thinking (which, in this case, is usually equated with knowledge). The political outlook corresponding to the perspective of this first camp holds that faction and differential effects can be exceeded and neutral or reasonable ground (whether scientific or moral), found or created. 13 Critics of the first camp maintain that thinking is inextricably bound to, and serves, will-to-power (albeit in sophisticated and convert ways). So far, this second camp has tended (though need not necessarily) to view politics as a battle of competing interests with outcomes determined by their relative strength.14 Perhaps more often, a reconciliation of the first camp's "contemplation without interest" and the second camp's interest without contemplation is sought in some notion of "enlightened self-interest" when expert knowledge or mature, "postconventional" reasoning provides the thoughtless ego with its "real" interests, again with the danger of shielding thoughtless assertion behind claims to "knowledge" or "rationality." ¹⁵

From the perspective afforded by (if not always propounded in) Nietzsche's writings these philosophical and political positions may be judged dogmatic, if in different ways. Together, those who claim that thinking can transcend will-to-power and those who claim that it is simply a vehicle for it (if anyone truly holds the latter view) abet our nihilistic political culture. What each denies (or insufficiently explores) is the capacity of thinking to transform will-to-power without pretending to surpass it. By contrast, Nietzsche denies neither thinking's proximity to will-topower nor the capacities of thought. He affirms the possibilities of thinking in a world saturated with will-to-power's effects. On this understanding, Nietzsche's perspectivism provides us with a way of conceiving the interrelationship of thinking and power

that does not allow the ascension of either term but facilitates the fullest understanding of their unavoidable, and potentially affirmative, interdependence. Political theorizing attends and responds to will-to-power. Perspectival thinking entails recognition of both how one becomes "what" one appears to be and to what views one's location gives inclination, combined with a facility for incorporating alternative perspectives and thus for transfiguring "who" one is becoming.

PLURALITY, COMMONALITY, AND POLITICAL SPACES

Nietzsche is not a political theorist. His interpretation of nihilism indicates its cultural and collective sources but without due or satisfying attention to its political aspects or preconditions. In my view, he disavows that nihilism can be collaboratively alleviated. He surveys modern culture from his particular location within it and determines that few, if any, practice the thoughtful self-transfiguration he esteems. He may be right, though he does not tell us why most appear to be incapable of this thoughtfulness, enhanced vision, honesty, and courage before themselves and the world.¹⁶

Hannah Arendt regarded modern thoughtlessness from a different perspective. Though this principal concern of her work is most conspicuous after Eichmann in Jerusalem (see Arendt 1964, 1965, 1971, 1978), her intuition about connections between reflective capacities and political possibilities is evident much earlier (Arendt 1951, 1953, 1958). On my understanding, Arendt's insight is that thinking (or, negatively, thoughtlessness) is conditioned by the configuration (or absence) of political spaces and by the availability and quality of specifically political power. While both Nietzsche and Arendt value and presuppose plurality, they have quite different views of its relationship to political commonality. For Nietzsche, the latter threatens plurality and thus his (anti)political opinions. By contrast, Arendt believes that the appearance of plurality and its worldly effects become manifest through politically constituted commonality. For Arendt, the ascendance of a unitary perspective (which she, like Nietzsche, associates with nihilism [Arendt 1971, 435]) and the destruction or imperceptibility of a plurality of views are linked to the absence of distinctly political spaces and the political commonality they potentially promise (1958, 58). The appearance of the plurality that Nietzsche values is, in Arendt's view, impossible without political spaces. Yet Arendt often imagines these spaces (I think mistakenly) as necessarily surpassing the demands of the body or "what" we are. We need both of their perspectives, altered by mutual transfiguration, to conceive political perspectivism. The challenge for political thinking is to suggest how commonality may be politically constituted while recognizing that all such political instantiations themselves are manifestations of will-to-power's effect. Additionally, we need stories telling how these effects may be politically addressed without devolving into thoughtless assertion, that is, without either denying that such effects repose in thinking or simply exercising them thoughtlessly.

According to Arendt, contact between human beings facilitates the appearance of both commonality and plurality. Where people speak and act together politically, such spaces engender and sustain whatever perception of commonality we achieve (Arendt 1958, 50, 52–53). This in turn facilitates recognition of our distinctness from other persons and the revelation of individual uniqueness or "natality" (pp. 49, 175-76, 178). Paradoxically, the perception of commonality creates boundaries, or at least a capacity to recognize boundaries as well as connections between oneself and others (p. 52). If political commonality is absent or unimaginable, then not only what we potentially share with others but also what distinguishes us from them (our distinct perspectives upon and locations within the world) become precarious (pp. 57-58, 199). Whatever assails the possibility of political commonality menaces plurality and vice versa. This is Arendt's view of our greatest threat; for "mass society not only destroys the public realm but the private as well, deprives men not only of their place in the world but of their private home" (p. 59). Perception of commonality in the world needs the presence of a multiplicity of perspectives that despite their diversity create and confirm its sharedness (pp. 57-58). "The end of the common world has come when it is seen only under one aspect and is permitted to present itself in only one perspective" (pp. 39, 58, 220). While political spaces create commonality, we each have our different locations in the world and our views are a mixture of "who" we are as revealed in such spaces and the distinct locations that condition "what" we appear to be. Different locations afford differentiating perspectives on a common thing (p. 57). Political commonality enables this plurality to

Arendt is notorious for advocating a hard-and-fast boundary between matters public and private (Arendt 1958, 22–73, 192–220) in the interest of preserving the distinctness of political commonality from "biological sameness." Through political action, we leave behind (or silence for a glorious moment) the necessities of "biological life process" and "what" we are in exchange for freedom in the public world. When she speaks in such terms (and she does not always), Arendt's political theory has a rarefied quality that obscures its relevance to modern liberal democratic polities in which people seem preoccupied with their bodies and interests. But the boundary Arendt draws between public and private matters is eroded by her own insistence on the intrinsic import of plurality for the actuation of political commonality. Plurality is indispensable to the emergence of specifically political spaces and the contour and character of our locations begin plurality's content. Though contrary to some of her most explicit views, Arendt's emphasis on plurality suggests that one's private location and *body* ("what" one is) weigh upon "who" one is revealed to be in political spaces.

Arendt neglected the interrelationship between bodies, locations, and specifically political power, because, unlike Nietzsche, she conceives the body as politically irrelevant. This is in part because Arendt treats the body and its "necessities" as univocal and unalterable (e.g., Arendt 1958, 46, 111-12, 119, 123; 1978, 22, 29, 33, 35, 44), rather than—as Nietzsche maintains—contingent (if weighty), culturally and politically inscribed (sometimes even arbitrarily), and thus contestable and politically significant. Nietzsche recognized that the body as we know it (and as Arendt assumes) appears as an undifferentiated mass, but he also maintains that "the welding of a hitherto unchecked populace into a firm form [like Arendt's "mass society"] was not only instituted by an act of violence but also carried to conclusion by nothing but acts of violence" (1967c, 86). As we are incorporated into such a "firm form," our perspectives are, at least in part, attributable to our different bodies and locations, our habits and beliefs, and their relative effects. 17

Discerning "what" we appear to be—our bodies and private locations—is therefore not irrelevant to, but a prerequisite (if not sufficient) for political deliberation and action. Histories of our locations may offer a critical purchase on Arendt's undifferentiated "mass society" and thereby facilitate the appearance of a plurality of perspectives. Without thinking about and being responsive to such particularities, we lack the basis for a differentiating perspective, one among the many that Arendt says are required to create and assure political commonality (1958, 58-65). Likewise, when political spaces are absent, the distinct perspectives that one's location(s) allow become indiscernible and, again, both plurality and commonality are at risk, and with them political agency and the capacity thoughtfully to affect the conditions of one's life. Absent political commonality and the plurality of perspectives accompanying our different locations, we may lose our world, figuratively and literally (1951, 135-47, 269-302, 460-79; 1958, 57-61, 208-9). Perhaps Arendt discounted the political relevance of "what" we appear to be because she, like Nietzsche, held such a sobering view of how human beings secure their ways of life.

TRESPASSES AND POLITICAL ACTION: HOW ONE BECOMES WHAT AND WHO ONE IS

How do we forge the locations which begin plurality's content? Our locations are *made* as we master necessity by means of force, coercion, and even violence, toward nature and other human beings (Arendt 1958, 31, 129–30, 140). In Arendt's view, necessity, and therefore compulsion, persists (though in modernity it is increasingly hidden) wherever hu-

man life is sustained (pp. 121, 125, 130, 135). The satisfaction of human, bodily needs—needs for food, shelter, and leisure (among other things)-includes compelling nature, oneself, and others to bear the burdens of life. For the most part, in mastering necessity and fabricating locations, "making" is the relevant mode of activity and it is an indispensable manifestation of the vita activa (p. 136-74). But making, as principally a matter of poiēsis, consists in the mastery of others and oneself; thus, an element of violation—and potential violence—inheres in all fabrication (pp. 139, 194-96). By itself, making creates a world where necessity and compulsion, not freedom and political action, predominate and with destructive repercussions for human relationships (pp. 195, 220-30, 233-36).

The means by which each location is made—and, in turn, is affected by others' locations—affords each some of its specific character and commences its differentiating perspective on the world. While Arendt's reflections on the "private realm" tend to represent location as a singular place or "home" her notion of trespasses belies this narrow understanding (1958, 28–37, 58–67, 236–243). Through our activities, we insert ourselves into a weblike world. Through our words and deeds, whether in political space or some other, we make a world, disrupt what precedes us, and may initiate something altogether new (pp. 8-9, 188). When we forge our locations, we trespass against others (p. 240). Her reflections on trespasses show Arendt, like Nietzsche, to be a theorist of will-to-power.

Trespasses are not "radical evil" but signify its everyday variety and they adhere in the very nature of the vita activa's establishment of new relationships (see Arendt 1958, 240-41; idem 1971, 438). Trespasses are effects that one cannot undo oneself, and they occur under circumstances where one did not, or could not, have known what one was doing. One may not have known what one was doing because the effects of human activities cannot be predicted (1958, 188-92). It is the nature of these activities to exceed our intentions and plan because we share a world we did not make with other human beings who are also makers and actors (1953, 389; 1958, 184, 190). Moreover, we are not responsible for "what" we are-how and what we have been "made" to be by history, institutions, and patterns of power that preceded us and for which no one person alone is responsible (e.g., Nietzsche 1954, 500–501). We may become responsible, however, for "who" we are, that is, for how we bear the will-to-power that accompanies "what" we appear to be.

It is unlikely, however, that we can become responsive to our will-to-power without political spaces, for when we trespass we also may not know what we are doing because we often cannot recognize the effects of our *bodies* and locations without the benefit of others' perspectives. One must learn about these effects from others, usually in the form of a story told about or from the perspective of those others whom one effects. Until then, one's effects and actions

(though they are apparently one's own), may remain largely unseen or unacknowledged. "Who" we are only appears to us in the form of an unending story told by others (Arendt 1958, 181–88; Nehamas 1985, 163–69) and is distinguished from "what" we appear to be by how we bear and transfigure the effects of our will-to-power. But we can do so only in political spaces where we act in concert with others because our *trespasses* are extended by patterns of power that we alone cannot change.

The distinction between "necessity" and "freedom," "making" and "political action"—so crucial to Arendt's political understanding—turns on the degree of articulation in the latter term of each pair. Freedom and political action define spaces where we "answer," "talk back," and "measure up to" what has happened or what we have done, the effects of our location in the world (Arendt 1958, 26; idem 1982, 41). Necessity and making define relations that are leveling, forceful, and unresponsive. When necessity and making reign, as they typically do in the daily (nonpolitical) fabricating of our locations, we simply have effects upon others and receive their effects upon us; neither we nor they are called to answer for them. Will-to-power is exercised thoughtlessly. Combative, not collaborative, action prevails.

By contrast, political spaces both may provide ways of reckoning the necessity that reigns outside and may be carried into political spaces. ¹⁹ In Arendt's view, the "ruling and being ruled" typical of making (and which likewise inform modern understandings of politics to the extent that we confuse it with "governance" and its powers) are "prepolitical," because they are relations of necessity, rather than freedom—of making, rather than political action (Arendt 1958, 220–30). The public or private character of relations is less decisive for their being political or nonpolitical than is whether we fathom and respond to our effects upon one another and open them to specifically political deliberation and action. "Who" we are may be judged estimable when we become responsive in this way.

On this understanding, politics is not so much a realm or sphere we enter into as it is a form of relation we undertake. Political spaces do not simply reveal or provide a means of furthering our will-to-power. Rather, in them we encounter others who call how we bear its effects to our attention, and we may do likewise. We constitute political relations and create the possibility of political commonality when we think together about the ruling and necessity—compulsion and violation—that dwell in all "making," whether they become manifest in individual acts, social behavior, or governance. Indeed, if we do not, spaces of cooperative action are undermined then destroyed by thoughtless assertion. In political spaces, we have the opportunity to deliberate—to discern and answer for our effects and, at least potentially, to transfigure them by means of the political power generated when we act in concert. When we associate politically, we enact perspectivism and thoughtfully direct our will-to-power.

TRANSFIGURING THE BODY OF THE POLITICAL ACTOR

It remains to be seen, however, exactly how political deliberation reveals and, most importantly, transfigures our distinct perspectives. How does perspectival political thinking help us to come to see the *trespasses* (unknown, unintended, thoughtless effects) that "made" our locations and the collective histories of which they form a part? How does it facilitate our acting in concert so that we can generate the collaborative power needed to transfigure our assertive effects and the patterns of solidified will-to-power which embolden them? In my view, Arendt's most suggestive formulation follows, in which she conceives political thinking as "representative":

I form an opinion by considering a given issue from different viewpoints. . . . This process of representation does not blindly adopt the actual views of those who stand somewhere else, and hence look upon the world from a different perspective; this is a question neither of empathy, as though I tried to be or feel like somebody else, nor of counting noses and joining a majority but of being and thinking in my own identity where actually I am not. (1977, 241)

Political thinking imagines other views. But what is Arendt practically endeavoring to describe? The Kantian notion of "enlarged mentality" on which Arendt draws in this and related passages (e.g., 1977, 197-264; 1982, 44) has become a popular locus for efforts to conceive political judgment. The virtue of Arendt's rendering of this idea, at least when we read it with eyes transfigured by Nietzsche, is that she does justice to its difficulties. Too often, the imperative and possibility of regarding matters from another's point of view or of "reversing perspectives" are simply presupposed. Though Arendt, following Kant, often invokes forgetting the *body* and "what" we are as the goal of such reflective activity (e.g., 1978, 30-45, 1982, 54), Nietzsche's refiguring of the relationship between body and mind-power and thinking-counsels against it. The goal of perspectival political thinking is not to exceed will-to-power but to transfigure it into the collaborative power we need if we are thoughtfully to (re)direct our effects. Read in light of the shadows cast by Nietzsche, Arendt's formulation suggests just how difficult matters can be when we endeavor to grasp another's perspective so that we can act together. She helps illuminate such political thinking by first telling us what it is not.²⁰

First, representative thinking is not empathy ("as though I tried to be or feel like somebody else"), for it shares the vices of enlightened self-interest (Arendt 1965, 73–94; idem 1982, 43). Like "contemplation without interest" it is founded on the myth that one can consciously suspend "what" and "who" one is and adopt a viewpoint independent of interpretation. The fallacy that one can simply adopt another view is also thoughtless, like interest without contemplation, because another perspective is gained without thinking. In the course of doing so, an act of interpretation

has been performed, but the denial of this fact almost assures the interpretation's arbitrary content. When one presumes to adopt another perspective without reflection on the boundaries of one's own body and location, more often than not one simply imposes the view from there upon another. Indeed, this is a principal way of bolstering one's location and demonstrates the effects involved in doing so. In such cases, one's own view arrogates another's and thus threatens to violate or do away with it altogether.

Neither is representative thinking, according to Arendt, a tally of viewpoints whose calculation results in a majority "perspective." This is the case for two reasons. First, it it so because, like interest without contemplation, amassing multiple perspectives into a single view is an abdication of thought and evaluation. Here the value of a view depends upon its strength or numbers, and politics as a battle of competing interests is thoughtless. Second, and relatedly, this means of creating one perspective from many does not represent a full view (an aspiration toward as comprehensive a view as possible) but the imposition, again in an arbitrary manner, of a univocal view. Political commonality is confused with the ascendance of a monologic truth or consensual perspective. Such thinking is neither "representative" nor political because the viewing of commonality in only one aspect (as we saw earlier) ultimately threatens to obliterate the world in which we appear and act politically. According to Arendt, the creation of commonality depends upon the preservation of a plurality of views. A representation of what is common that forgoes this plurality of perspectives and obscures or destroys each differentiating relationship to it threatens to render both our locations and world indiscernible.

The key to Arendt's alternative and, by implication (I am contending), an elaboration of the political theoretical significance of Nietzsche's perspectivism, is the idea of "being and thinking in my own identity where actually I am not." How does one, like Nietzsche's dramatist, enter into other bodies, other characters? Arendt may be read as trying to convey a precarious balance—the preservation of one's own perspectives and political thinking capacity while incorporating another's view. Political deliberation and action should not result in the obliteration of the perspective—or an assault upon the location—of any participant. When they do, neither thinking nor deed are political but bear a greater resemblance to "making." The mastery of another's perspective is anti-thetical to the "answering" and "measuring up to" the perspective of another definitive of political relations. Political thinking may require the alteration of both one's own perspective and that of another but the total loss of neither.

The effort to think from another's perspective may change "who" I am—my location and how I live it. Our location is the sum of our effects and, by implication, the sum of all that effects us (Nehamas 1985, 74–105). It consists of our habits and beliefs and the sway of the world and its other inhabitants upon how

we live them. To think another perspective—to endeavor to conceive another location-even if ultimately to refuse it (and no doubt we often will), entails some acceptance of that perspective. When comprehending another view, one incorporates it into what one can imagine and seeks to envision one's location from another's perspective until, finally, it may transfigure what that location is. Equally, in this responding to the view of another, the location and perspective one encounters may also be commuted. One's own perspective and another's mix to form an original combination, a new perspective on the world and an alteration of the plurality of locations that sustain both it and whatever political commonality we achieve. If one has actually come to conceive another view and to answer to it, one's habits and beliefs—one's own body and location may never again be lived in quite the same way. 21 To think from the vantage of another, to respond to the perspective born of their location, and to measure up to the relation of your location to it may call for changing "who" you are. Political deliberation offers the world a modified view of another location mixed with "who" you are, thus transfiguring "whom" you and they appear to be, as well as "whom" you and they are becoming.

When we think and act politically (and admittedly, on this understanding, it is rare indeed), we come to recognize the distinctness of our bodies and locations and the perspectives they afford in contrast to the perspectives of others. The manifestation of such a plurality of views is not a "thought experiment" (Benhabib 1992, 158-70) but requires actual encounters in political spaces among persons and their differentiating perspectives. Most importantly, these perspectives, which become manifest through political deliberation and transformed by political power, are born of differential effects of compulsion, force, and violence, which both emerge from and shape our bodies and locations. Each of us carries such effects, whether or not they become manifest in a political or nonpolitical space. But a space becomes political when one's perspective ceases to be thoughtless assertion and becomes a location with effects upon others for which one answers and to which one thoughtfully responds. Political thinking enacts perspectivism. We think theoretically when we attend, and respond to will-to-power. Therefore, thinking is not antithetical to political action but is its precondition. One need not be a philosopher to think; each of us may become capable, with the aid of others' perspectives, of discerning our effects-responding to and transfiguring them. We think and act politically not only (or perhaps not even primarily) when we give strong arguments to justify or "woo acceptance" of our locations and perspectives (Arendt 1982, 72) but when we open ourselves to others' perspectives with a willingness to transfigure "who" we are becoming. 22 Such responsiveness is a condition of the action in concert that generates the collaborative power needed to transfigure our effects and the patterns of power that amplify them.

We cannot reach this understanding of political perspectivism by reading either Nietzsche or Arendt alone. Rather, their views must be brought to bear upon one another to facilitate a mutual transfiguration and the appearance of a new combination, a different perspective. Efforts to preserve Nietzsche's philosophical achievement that neglect his perspectivism or render it as entailing no necessary political preconditions are likely to fail, in my view, because they finally offer no alternative to a vision of politics and culture as a battle of competing interests with outcomes determined by their relative strength and tend to ignore or conceal the assertive effects that remain even when we endeavor to enact perspectivism. If perspectivism is to be actualized or to avoid repugnant forms, it needs to be joined to political theorizing that surmises how we may come to recognize and thoughtfully transfigure our perspectives and assertive effects in response to others and (as I will suggest in a moment) why we might become willing to do so. Arendt's writings are one potent resource for such an understanding, but they too need transfiguration. Arendt (1958) maintained that we could lose our "common world," figuratively and literally, and indeed she presciently regarded this to be increasingly our condition. But her views about the relation of mind and body-thinking and power-prevented her from fully elaborating the ways of thinking that might facilitate political spaces and collaborative power. The imperative to surpass "what" we are can amount to a willful neglect of the effects that, if unattended to, may destroy the world and our capacity to act in concert. Only perspectival political thinking, which discerns our bodies and locations and their effects, and which is the precondition of political action responsive to them, can begin spaces where plurality appears and political commonality may be constituted.

CONCLUSION

To what extent commonality and plurality (or diversity) can and should be balanced (or each sustained) remains among the most pressing and virulently debated questions in contemporary American political, social, and cultural life. As posed, the issue is a false one. Both plurality and commonality presuppose and need one another. Politically constituted commonality affords the only hope of preventing the destruction of diverse perspectives. Plurality can only be preserved through political spaces where we discern, answer for, and potentially alter the effects of will-to-power which threaten to suppress or destroy particular locations and the perspectives they engender. Absent specifically political deliberation and power, plurality is overcome in a contest of strength by a unitary perspective or by the absence of any perspective at all. Likewise, commonality needs plurality if it is not to devolve into thoughtless assertion. When not sustained by such differentiating perspectives, commonality becomes narrower and more exclusive, that is, increasingly less than common and therefore rejected or assaulted by those whose views have not given shape to it. Only when commonality is created through political deliberation and action that preserve each differentiating relationship to it can what is regarded as common actually be shared—and even then only partially and provisionally.

Political spaces can be created and maintained, however, only if we seek to restrain and redirect our will-to-power. The fact that genuinely political spaces are so rare and usually so short-lived tells us something about our ways of being in the world (see Arendt 1965, 215-81). Will-to-power is unavoidable and intrinsic to human living. The decisive issue is whether or not we direct it thoughtfully. Thoughtless assertion destroys political spaces and, with them, both commonality and plurality. It is tempting, I suppose—given the entrenched view that will-topower poses a threat to thinking and, relatedly, that legitimacy is secured by surpassing contestability—to conclude that the only way to create commonality is by deprecating our different bodies and their interests in order to affirm what we are believed to share, whether that be a minimalist aversion to the outright hostilities that unbridled difference is feared to portend or more substantive communal or rational ideals. But that strategy is bound to fail because, most often, it is our different locations that make demands upon others bodies and the world-demands that threaten to destroy those locations and perspectives and thereby obstruct the constitution of what is shared. It is not plurality's very existence that precludes our acting and deliberating together to politically constitute commonality. Such efforts are undermined by our failure to respond to the claims about will to power which different perspectives pose.

Without encountering and responding to others' challenging perspectives I may not even recognize how "what" and "who" I am erodes political possibility and, in turn, endangers a plurality of perspectives of which mine is one among many. Much stands in the way of discerning those effects and becoming willing to answer for and transfigure them-for example, incommensurability or the incommunicability of certain forms of harm, intractable hostility and distrust, want of courage and fear of relinquishing security. But if and when we do recognize and become willing to transfigure the effects of our willto-power, they cannot be altered by our efforts alone. Trespasses come with simply living our bodies and locations. They are an effect of activities that preceded and constituted us—of histories, institutions, and patterns of power we alone did not makethough our words and deeds reinforce and extend trespassess. We must act with others if we are to alter them, and even then our success, no doubt, will only be partial. Every political instantiation or constitution of commonality has effects such that, if commonality is not to deteriorate into thoughtless assertion, it requires a continual willingness to deliberate, to listen and respond to others' claims about our effects, and (possibly) to modify them. We will always need

politics, not because everything is finally determined by a contest of strength but precisely to constrain and redirect thoughtless assertion into the thoughtful exercise of will to power.

From where do we acquire the energy and inclination to become political perspectivists? Why would we take this leap of faith? When we understand that if we do not, plurality is threatened and with it the meaning and existence of our agency, distinct location(s), and world. The will-to-power embodied in our words and deeds may so harm others as to threaten to obliterate the differentiating view they have—one among the plurality that the constitution of political commonality requires. The existence of political spaces that foster the distinct views engendered by my location in the world—and thus the maintenance of that location-calls for the perspectives of "others." Without their views, the uniqueness of my own views will not appear. Unable to discern the particularities of my own body and location, I will be unable to constitute political spaces with others where my perspectives—and the location from which they emerge—can be revealed and sustained, if transfigured.

Notes

Thanks to Peter Euben, Lisa Frank, George Kateb, Alexander Nehamas, Michael Rogin, Alan Ryan and, more recently, Stacey Alaimo, Nick Burbules, Allen Hance, Natasha Levinson, Jacqueline Madden, Laura Stoker, and Deborah Wuliger for their influences on the argument made here. I benefited too from discussions of an earlier version of this essay at the University of Minnesota and Vassar College.

1. The meaning of body shifts throughout my text, because Nietzsche and Arendt conceive its relationship to politics differently—and neither to my satisfaction. I shall introduce the notion of location to refer to the continually changing space in which our body appears and thus seek to keep in play the body's nonconscious and material, individual, social, and political significances.

2. Kant's Critique of Judgment was Arendt's principal resource in doing so (Arendt 1977, 197-264; 1978; 1982). Numerous commentators have adeptly evaluated the perspicacity and limits of Arendt's view of judgment and of Kant's own, especially in relation to Ariesotelian phronesis (e.g., Beiner 1982, 1983; Benhabib 1992, 121-44; Bernstein 1986, 221-37; Castoriadis 1991, 81-123).

3. Those who bring out the Nietzschean influence often return in the end, with Arendt, to Kant (e.g., Villa 1992) or see that influence as leading away from political action (e.g., Beiner 1982, 153–54). Honig interprets Nietzsche and Arendt as together furthering our understanding of political action 1993, 40–125; but if I read her correctly, she retains something of Arendt's antithesis between "thinking," or "theorizing," and "acting" that I query here (though see pp. 59–60).

4. Exceptions to the common view, though to varying degrees, are Connolly 1987, 1991, 1993; Honig 1993; Kateb 1992, 127–51; Warren 1988. Throughout, I use will-to-power to refer to the effects engendered by human activities. While it is crucial to appreciate distinct and various instantiations of will-to-power (e.g., violence, strength, force, and political power or action in concert [Arendt 1972, 143–46]) these forms of efficacy often slide into one another (e.g., as political power recedes, violence or force often emerge to take its place, pp.

105-84)—hence the need for a general term to represent this continuum.

5. Typically, we take a writer's express views about politics as indicative of his or her political significance. Nietzsche's writings have been interpreted as illiberal or protofascist (e.g., Schutte 1984 and Strong 1988); as without any contemporary political significance whatever (e.g., Nehamas 1985); as advancing an exemplary "agonism" (e.g., Connolly 1991 and Honig 1993). Some truth resides in each interpretation. Yet a writer may proffer a theoretical contribution that exceeds or even contradicts such explicit views. Nietzsche's rejection of moral valuation, which confounds the "cause" of a thing's "origin" and it utility (1967c, 76-81) in effect recommends that we not take his political views (the "origin" of his writings as it were) as determinative of their theoretical "utility." concepts and organisms are a whole synthesis of meanings, and "only that which has no history is definable" (p. 80). Nietzsche has his own views about the value of certain concepts or organisms and judgments about the worthiness of political, collaborative power. He would be the first to admit that these are his own, partial interpretations (1973, 34) and to my mind, these views may be distinguished from perspectivism itself, which embodies his way of thinking. Nietzsche does not respond to a plethora of meanings and their indeterminacy by asserting a single, utilitarian definition; for he regards such monologism as dogmatic (Nehamas 1985, 33-41, 56-57) and nihilistic but attempts (though he does not always succeed) to view the question of something's value from multiple, diverse perspectives. "Value for what?" and "Value from what perspective?" are his leading questions (1967c, 17, 20, 55). They exemplify Nietzsche's perspectivism in its simplest form. Nietzsche is not always the best perspectivist (in part because he neglected, I contend, perspectivism's political preconditions), but that in itself need not discredit perspectivism as a way of conceiving thinking. Bracketing our initial impressions of Nietzsche's political views may be the best way to assess the political theoretical significance of his writings. Engagement with those writings affords a different perspective on what it means to think theoretically.

6. I must ask the reader to resist the understandable but nonetheless unjustified assumption that political theorizing must necessarily be epistemologically grounded in the stricter senses of the term. It is more misleading than helpful to conceive perspectivism as an epistemology, at least as the latter is conventionally construed (Nehamas 1985, 73; Warren 1988, 94). Moreover, Nietzsche and those who have borne his influence reject the dual-world metaphysics upon which a distinction between judging and thinking rests (Foucault 1977, 177–78). Similarly, I speak of "thinking" rather than "judging" because I regard "imagining invisibles" (in addition to perceiving what is "close at hand" or commonsensical) to be both possible while acting politically and a condition of

doing so.

7. They verge on this, for example, when Arendt speaks of thinking as necessarily solitary, a withdrawal from the world, and a forgetting of the body (e.g., 1971; 1978, 22–23, 35, 44–45, 64, 69–80, 85, 92; 1982, 73). While she sometimes qualifies or contradicts these characterizations, her view of the political relevance of the *body* and its largely negative influence on "mental faculties" remains relatively constant.

8. On Nietzsche's use of the body, see Blondel 1991 and also Deleuze 1983, 39–72. For specific examples of Nietzsche's unconventional sensorial and corporeal metaphors, see 1967b, 222–23, 236–40; 1967c, 23; 1973, 55, 126, 134–35.

- Thomas Hobbes is an exemplary theorist of this view.
 We would have to include among them Arendt 1958,
- 1971, 1978, 1982.
- 11. Arendt, following Augustine's Confessions 10, introduces a distinction between "what" and "who" one is (1958, 10–11) that I shall interpretively elaborate. "What" we are represents what appears to be given or beyond the reach of our agency, while "who" we are is revealed through our deeds. Since "what" we are cannot finally be known, the relationship between it and "who" we are is contingent, changing, and simply an analytic distinction (e.g., Nietzsche 1967c, 45). For

example, "race" and "gender" are usually regarded as aspects of "what" we are, but their meaning and significance can change over time, depending on how we carry or transfigure the effects they generate (i.e., with "who" we are).

12. The following intellectual cartography could also be extended to map different moments in Hannah Arendt's reflections on action, thinking, and judgment, as well as a variety of interpretive approaches to her writings (as sug-

gested in subsequent notes and citations).

13. Theories of political judgment that draw on a neo-Kantian universalism or sensus communis can be analogous to this perspective in important ways, because they tend to efface rather than address the challenges that will-to-power poses to thinking (even when they evade the positivist tendency to conceive "mental faculties" in epistemological terms). When such accounts invoke a universality, "common world," or "shared taste" that are now thoroughly contested through genealogical critique, some may read them as having a nostalgic or coercive air (e.g., Arendt 1958, 50–58; idem 1982; Benhabib 1992, 89–144).

14. Theories of political judgment assume a similar perspective when they emphasize political action's "agonistic" character (e.g., Arendt 1958, 194) without sufficiently articulating responsiveness for the effects of our activities or performances on others and the world. In other words, like its apparent opposite, this perspective must be wary of occluding or inadvertently authorizing the thoughtless exercise of power. While an interpretation deploying Nietzsche and Arendt's "institutionalism" politicizes agonism (Honig 1993, 69–75, 96–125), in my view, we also need to distinguish between "making" or "governing powers" and collaborative, political power (see below) not only to chasten agonism's potential to slide into thoughtless assertion but also to address and avoid the nostalgic and coercive air sometimes associated with the "first camp."

15. Theories of political judgment informed by this third perspective would contain the agonism or virtuosity of performance by invoking a sensus communis. In other words, Kant is employed to temper Nietzsche (e.g., Arendt 1978, 1982; Villa 1992). As we shall see, in its effort to reconcile the preceding views, this perspective is subject to the problems of each.

16. To be more precise, what he does tell us usually attributes an essential character to whole classes of human beings in ways that contradict his perspectivism and related philosophical commitments. In my view, Nietzsche mistakenly assumes that the diversity of bodies and perspectives can appear without political spaces. This and other matters on which I seek to persuade the reader to follow Arendt rather than Nietzsche—or vice versa—hinge on philosophical issues (ontological and linguistic); for more detailed considerations of these matters than I can pursue here see e.g., Connolly 1991, 1993; White 1991.

17. Arendt relegates all of this to bodies, their "necessities" and "irresistable tendencies," which she regards as without political relevance unless they threaten to intrude upon "the public" (1958, 45-47, 68-73). Arendt does not view the "private," bodies and "what" we are as without value, but she does strictly circumscribe their place in politics (their relevance to "who" we are).

18. In consumer society, making has been colonized by laboring (Arendt 1958, 102, 124, 126, 152-53, 157).

19. Arendt would deny the latter point, given her views about the relationship between our *bodies* and political action.

20. I am elaborating Arendt's thinking as it is transfigured by an encounter with Nietzsche. Thus, in what follows I may challenge or contradict some of Arendt's views, especially her "late" formulation (Arendt 1982; see Beiner 1982, 89–94). But my task in this context is not to document her views but to illuminate political thinking.

21. If this seems implausible, consider (recurring to an earlier example) that actually understanding how "what" you are effects others (e.g., the "sort" of person with a history and location that inclines you to speak often and freely while deliberating, which reinforces others' predilection not to do

so) might lead you to alter "who" you are (how you listen to others, when and how you speak). Though the example may seem banal, in my view it would be impossible to overestimate the number of political spaces that have disintegrated or

mate the number of political spaces that have disintegrated or descended into thoughtless assertion when people are not willing to transfigure "who" they are in this or similar ways.

22. This formulation may not be inconsistent with recognizing, hearing, or letting "others" be (e.g., Benhabit 1992; Connolly 1991; White 1991), but it does reflect a different emphasis. In my view, we need to educate the different different the selection of the s emphasis. In my view, we need to acknowledge the need—perhaps inevitability—of each transfiguring "who" she or he is in response to others (to some extent, Connolly [1993] moves in this direction), because the latter is a condition of the creation and sustenance of political spaces. I understand the centrality given the will-to-power as distinguishing what I advocate from a Gadamerian "fusion of horizons." No doubt, some will find my story still too "moralistic," while others will judge it to be "nihilistic," because insufficiently discriminating.

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RESEARCH NOTES

AN INDIVIDUAL-LEVEL MULTIEQUATION MODEL OF EXPENDITURE EFFECTS IN CONTESTED HOUSE ELECTIONS

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The shall address questions concerning the impact of candidate spending in congressional elections in a new way. We develop a multiequation model of congressional vote choice that takes the endogeneity of expenditures into account. We then estimate both this model and the more traditional single-equation model using individual level survey data. The substantial differences we find between the two models indicates that the simultaneity bias present in the single-equation model is not trivial. The challenger and incumbent expenditure terms are each significant (whereas only challenger expenditures are in the single-equation setup) and of much greater magnitude in the multiequation case. In addition, we evaluate hypotheses grounded in persuasive communications theory concerning the type of individuals most affected by the messages emanating from the campaigns. We find that individuals with higher education, those with greater interest in campaigns, and those with strongly held convictions are unaffected by candidate spending, whereas individuals lacking each of these attributes are greatly influenced by campaign expenditures.

Since the late 1960s, the amount of money spent by candidates for public office has grown dramatically (Alexander 1992, 82). This spending has generated a corresponding amount of scholarly activity concerned with, among other things, the role that all this money plays in the electoral process. This has been particularly true of congressional races, where most of the efforts to determine the impact of money have been focused (Green and Krasno 1988, 1990; Jacobson 1978, 1980, 1984, 1985, 1990; Ragsdale and Cook 1987; Thomas 1989, 1990). Despite these efforts, the fundamental question of whether increased spending is beneficial to both challengers and incumbents remains unresolved. Intuitively, it seems obvious that spending should enhance a candidate's electoral prospects. Empirically, however, the results are far from conclusive; and recent debates have brought this issue to the forefront again (Green and Krasno 1990; Jacobson 1990).

It is also the case that most of the efforts have been conducted at the aggregate level of analysis; total expenditures for each candidate affect the share of the vote the candidate ultimately receives. Much less work has been done at the level of the individual, so we know relatively little about how the effect of money works, that is, the types of candidates and messages that are most influential and the types of individuals that are most responsive to campaign spending. In addition, individual-level analysis might clarify the inconclusiveness that exists at the aggregate level.

We examine the challenger-versus-incumbent campaign-spending question in a new way. Employing an individual-level analysis of congressional vote choice, we model the relationship between money and vote choice as a system of equations. More specifically, we treat the expenditures as endogenous variables, thus taking into account one of the major

points of contention in this debate. In addition, this individual-level design allows us to evaluate hypotheses grounded in persuasive communications theory concerning the type of individuals most affected by candidate spending.

CANDIDATE SPENDING IN CONGRESSIONAL ELECTIONS

Research on the effects of expenditures in competitive congressional elections has focused primarily on aggregate outcomes. Following Jacobson's (1978) seminal article, scholars have attacked the problem in a number of ways. From this body of research, two general results have emerged. First, spending by challengers has consistently been shown to increase their share of the vote. The more challengers spend, the better they do. Second, the same cannot be said of incumbent spending. The evidence in this case is far more conflictual and seems, more often than not, to show that incumbent spending has no effect; that is, increased spending by incumbents does not help them get reelected. This result makes sense empirically, it is argued, because incumbents typically spend large amounts of money in the face of a strong challenge (ibid.).

Many have not been satisfied with this explanation, however, and the bothersome null effect for incumbent spending has generated a great deal of scholarly research. For competitive House races, Jacobson has consistently shown that incumbent spending has no statistically significant effect on the outcome of the election (1978, 1980, 1985). Green and Krasno (1988, 899) argue the opposite and base their conclusions on model misspecification. They show that incumbent expenditures tend to depress the

challenger's share of the vote, although their results are difficult to interpret because of a large number of complicated interaction terms and a nonlinear model specification. Ragsdale and Cook (1987) report inconsistent results. Thomas (1989, 1990) also argues that incumbent expenditures matter. He develops a theoretical model and tests it empirically. The results indicate that incumbent expenditures in the 1978 and 1980 elections are positively related (but not significantly so) to the challenger vote share. Clearly, the aggregate analyses have not settled this debate.

Much less work has been done at the individual level. Jacobson considers campaign spending in models of individual familiarity with candidates for House and Senate races (1978, 1980) and preelection vote preference (1990). The latter article includes expenditures for both challengers and incumbents and finds that incumbent expenditures do not affect the outcome. The methodology of Jacobson's 1990 article was contested by Green and Krasno (1990) because of the presence of identifiable measurement error in the expenditure terms. Ragsdale (1981) models the process by which voters evaluate challengers and incumbents in 1978 congressional races. For the House races in the study, she found that evaluations of challengers "negatively affect" the "opinions of incumbents without the reverse being so" (ibid., 201). Her model examines the structural effect of the campaign, including contact by candidates, both through the media and direct campaign efforts and through recognition of the candidate. These factors, for each candidate, are filtered through candidate evaluations and then affect the vote (ibid., 208).

A solid body of theoretical evidence holds that challenger expenditures, incumbent expenditures, and voting outcomes comprise a system of equations (Green and Krasno 1988; Jacobson 1978, 1980, 1984, 1985; Thomas 1989, 1990). Money and electoral outcomes are closely intertwined, it is argued, and any representation of how spending affects outcomes must take this simultaneity into account. A multiequation model is thus necessary to estimate these relationships correctly. Challenger spending is determined by a set of independent variables. Incumbent spending is a function of challenger spending (and other variables) since incumbents spend in proportion to the challenger they face (Jacobson 1978). Both expenditure terms appear in the outcome equation, as do other independent variables.

In summary, the weight of the theoretical evidence tells us that challenger and incumbent expenditures are endogenous to the voting process and that these terms are themselves functions of other variables. Accounting for this simultaneity at the aggregate level has not cleared up the debate over whether incumbent spending affects electoral outcomes, however. Turning to the individual level does not help because analyses of congressional vote choice typically ignore the effect of spending (Brown and Woods 1991; Kiewet 1983). As a consequence, structural models of individual-level vote choice explicitly incorporating expenditures are not found in the literature.

We shall develop a three-equation model of the effect of expenditures on individual congressional vote choice. Estimating a model of this sort requires sets of variables that do not normally appear together in individual-level surveys, however. Information from different sources was used to construct a data set appropriate for evaluating the sorts of questions we are interested in.

THE DATA

Assessing the effect of candidate spending on vote choice requires both individual-level survey information and the campaign expenditures of the congressional candidates who ran in the various districts. We chose the 1984 Inter-University Consortium for Political and Social Research American National (Pre- and Post-) Election Survey (ANES) for the individual level data used in the analyses that follow. These data provide a measure of congressional vote choice as well as standard sociodemographic and attitudinal controls typically used in individual-level vote choice models.

To the survey we added additional information on the congressional candidates, their expenditures, and the setting in which the election took place. Aggregate-level data on campaign spending by the challenger and incumbent running in the respondent's district were gathered from Federal Election Commission reports and added to the survey. Measures of the number of opponents that the incumbent and challenger faced in primary elections, their political party affiliation, and the level of support for the challenger in the previous election were created from information provided in America Votes (Scammon and McGillivray 1985). A measure of the number of terms the incumbent had served was constructed from information provided in the Almanac of American Politics (Barone and Ujifusa 1986). An objective measure of seat safety and the quality of the challenger were coded from information provided in Elections 84 $(1984).^{1}$

Given the nature of our investigation (competitive districts with an incumbent running) and the multi-equation specification of the model, some reduction in sample size is inevitable. A description of the subsample of respondents and congressional districts included at each stage of the analysis is reported in the appendix.

THE BASIC MODEL

We investigate directly the impact of challenger and incumbent expenditures on vote choice by estimating the following system of equations:

Challenger Expenditures = f (Challenger Party, Challenger Party Strength, Incumbent Terms in Office, Challenger Political Experience, Challenger Opposition in the Primary, Seat Safety) (1)

Incumbent Expenditures = f (Challenger Expenditures, Challenger Party, Challenger Party Strength, Incumbent Terms in Office, Challenger Political Experience, Incumbent Opposition in Primary, Seat Safety) (2)

Vote Choice = f (Challenger Expenditures, Incumbent Expenditures, Same Party, Reagan Approval, Economic Expectations, Sex, Race, Education, Age, Income, Presidential Coattails). (3)

We diverge from other individual-level analyses in the specification of the vote choice equation (equation 3). The dependent variable in our model measures whether a respondent voted for the challenger or not, rather than candidate familiarity or preelection vote intention (Jacobson 1978, 1990). In terms of the spending variables, we include expenditure terms for both candidates, rather than just for nonincumbents (Jacobson 1978, 480). We also include a standard array of demographic controls, such as the respondent's sex, race, age, education, and family income (the specific codings for the independent variables are included in the tables).

The other independent variables are not quite as well known, and a brief word about their construction is in order. Economic expectations is a threecategory variable measuring the extent to which respondents believe the economic future of the country is going to improve, remain the same, or get worse. The respondent's rating of the job president Reagan was doing is measured by five categories ranging from -2 (strongly disapprove) to +2 (strongly approve). Related to this measure is a variable we construct to capture a possible coattail effect. This variable is an interaction between Reagan approval and a dummy variable for challenger's party identification. The dummy is coded 1 if the challenger is a Republican and 0 otherwise. This variable ranges from -2 to 2. We expect that respondents who liked the job Reagan was doing will be more likely to vote for a Republican challenger. Finally, a dummy variable measuring whether the respondent identifies with the same party as the challenger or not is included to capture the degree to which party affiliation affects vote choice.

There are basically two ways that expenditure terms have been incorporated into vote choice equations. The first simply uses the aggregate expenditures reported to the Federal Election Commission. Jacobson (1990) employs this method in his singleequation model of individual-level congressional vote choice. As Green and Krasno point out, however, this method ignores the endogeneity of expenditures: "If incumbents spend more because they feel their support slipping, then the individual-level analysis will show that the more incumbents spend, the more their support slips away" (1990, 371). The second method makes use of the theory developed at the aggregate level in a micro model of candidate choice. Instead of actual expenditures, instrumental variables are constructed by regressing the two expenditure terms on all exogenous variables in the system and obtaining the predicted values from each. The predicted values from these first-stage equations are then used in the second-stage vote choice equation. We argue that the single-equation model produces insignificant incumbent expenditure effects because it suffers from the same simultaneity problems of many aggregate models. Estimating a system of equations accounts for the endogenity of expenditures and should produce the intuitively sensible result that spending improves electoral prospects for incumbents as well as challengers, although probably not quite to the same degree.³

We investigate this possibility by estimating both the single and multiequation models of congressional vote choice described. First we develop and estimate a standard logit model of vote choice for the House races present in the 1984 postelection wave of the National Election Study (NES). We then estimate the same model again, but instead of actual expenditures we include the constructed instruments for both incumbent and challenger spending described by the structural equations (equations 1 and 2). In addition to reporting tables of these coefficients, we use the model to consider the magnitude of the expenditure effects by holding all other variables constant at their mean values and varying the expenditure terms from zero to one million dollars.

These procedures are employed in Table 1 and Figure 1. The right column of Table 1 contains the results of our two-stage estimation and the left column contains the results of the single-equation estimation. Comparison of the estimated parameters for the expenditure terms across the two models is an illustration of simultaneity bias in the single-equation model. Challenger spending is highly significant and its sign is correct in the single-equation model. Incumbent expenditures in this model, however, appear not to be related to vote choice. In the multiequation model, this counterintuitive result is corrected. The estimates for both challenger and incumbent expenditures are statistically significant and have the proper sign. In addition, Figure 1 shows that these coefficients are severely attenuated in the single-equation model. The effect of spending is much greater in the multiequation model, although challengers gain more from increased spending than do incumbents. These results are clear indicators of the presence of simultaneity bias in the parameter estimates.4

The parameter estimates for the other terms are relatively stable. Generally, the same set of exogenous variables are significant in each case. Neither race, sex, education, nor income have crisp statistical effects in either model. Economic expectations fails to meet the test of statistical significance here, though it has been shown to have an impact in other races (Jacobson 1990). Finally, the Reagan approval and coattail variables are strongly significant and properly signed. Individuals who approve the job President Reagan is doing are more likely to vote against the House challenger, unless that House challenger is a Republican, in which case they are more likely to vote for him or her.

TABLE 1

Vote for Challenger by Incumbent and Challenger **Expenditures and Respondent Demographics:** Single-Equation Versus Multiequation Model

INDEPENDENT VARIABLES	SINGLE- EQUATION MODEL	MULTI- EQUATION MODEL
Intercept	-2.9 (.49)**	-2.4 (.52)**
Same party as		
challenger ^a	1.6 (.20)**	1.6 (.21)**
Reagan approval ^b	54 (̀.12)**	51 (.12)**
Economic	, ,	, ,
expectations ^c	.16 (.14)	.20 (.14)
Sex ^d	.04 (.18)	.08 (.18)
Age ^e	.01 (.006)	.01 (.006)
Race ^f	55 (.38) [*]	30 (.39) [°]
Education ^g	.07 (.05)	.07 (.05)
Family income	, ,	, ,
1983 ^h	02 (.02)	006 (.02)
Reagan coattail [/]	.99 (.15)**	.94 (.15)**
Challenger		
spending [/]		.00554 (.0016)**
Incumbent spending/-	00027 (.0006)	00425 (.0017)**
Model chi-squared	287.9	290.4

Note: Dependent variable is coded 1 if respondent reported voting for the challenger, 0 otherwise. Logit estimates. Standard errors are in parentheses. N = 938.

Incumbent expenditures matter in election contests for seats in the U.S. House of Representatives. Spending by the incumbent reduces the likelihood that individuals will vote for a challenger and this spending may have a significant impact on the outcome of the race. We now turn to the general question of how this spending effect works.

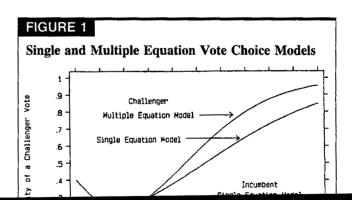
Campaign Spending as Persuasive Communication

So far we have demonstrated that spending by both challengers and incumbents affects individual vote intention when this relationship is modeled in a theoretically appropriate way. But demonstrating an is the source of any direct connection we may uncover between money and votes" (1990, 344).

It is perfectly reasonable to interpret the effect of money on vote intention in terms of persuasive communications. Generally, a high percentage of the campaign budget is spent on producing and distributing messages either in favor of the candidate or against his or her opponent (Goldenberg and Traugott 1984). In order more fully to understand the effects of spending in an election campaign, however, we need to evaluate this assumption directly. Several questions concerning the nature of this effect naturally arise. What type of individuals are most or least affected by the messages? What type of candidates are most appealing? Does the type of message have any bearing on its impact?5

Fortunately, a great deal of theoretical and empirical work in social psychology attempt to describe the influence of persuasive messages.6 These studies have identified a number of factors that influence the extent to which a message is persuasive or not. These factors can be grouped into several categories depending on which part of the persuasion process they affect. For example, characteristics of the individual receiving the message (the target) have been found to be important. Individuals with firmly held beliefs are less easily persuaded, as are the more interested and highly educated (Milburn 1991). Certain characteristics of the source of the message have been found to be effective in shaping attitudes as well. The credibility and attractiveness of the source of the message are examples (Lindzey and Aronson 1985). Finally, certain features of the message itself have been shown to affect its persuasive impact. The types of arguments and appeals (e.g., positive or negative), the message style (forcefulness and speed), and the amount of message material (how frequently repeated) are examples (ibid.).

Evaluating the role of each of these factors and potential others would be impossible in the context of this research design. However, the survey-based



^{*}p < .05.
**p < .01.

^a1 if respondent identifies with the same party as the challenger, 0 otherwise.

^bRange is from +2 (strongly approve) to -2 (strongly disapprove).

1 if respondent believes economy will get better, 0 if no change, -1 if get worse.

I = female, 0 = male.

Age in years.

 $f_1 = \text{nonwhite}$; 0 = white.

⁸Range is from 0 (8 years or less) to 10 (advanced degree). ^hRange is from 1 (less than 2,999) to 22 (75,000 and over).

Interaction between Reagan approval and a dummy variable coded 1 if respondent is a Republican, 0 otherwise. Range is from -2 to +2. ^jActual dollars (in thousands) in the first column model; instrumental

variable (as described in text) in the second.

design utilized in this study is ideal for examining at least one subset of factors common to any discussion of persuasive communications—the characteristics of individuals receiving the message (the target). Measures of education, interest, and strength of conviction are available in these data and can be utilized to test hypotheses about the types of individuals that are most, and least, affected by the campaign communications. While the other factors are undoubtedly important, we cannot evaluate them in this setting. Essentially, we have to assume they are equal in each case, that is, the candidates are equally credible, attractive, etc.

Individual Characteristics and the Effects of Campaign Spending

Are individuals with more education, those with greater interest in the campaign, and those who hold strong partisan beliefs less susceptible to the persuasive communications emanating from a campaign? In order to evaluate these hypotheses, four interaction terms are created for each estimation of the model. For example, to test the hypothesis that highereducated individuals are less affected by campaign spending, two variables are set equal to incumbent and challenger expenditures for individuals with a post-high-school degree and equal to zero otherwise in each case. The other two terms are set equal to incumbent and challenger expenditures for individuals with less than a post-high-school degree and equal to 0 otherwise in each case. Interest and strength of conviction are handled in essentially the same way. Expenditure variables for each candidate are created for those who are very much interested in the political campaigns that year, for individuals who identify as strong partisans, and for individuals lacking both of these two conditions. This setup allows us to test for differences in the way money affects different types of people by comparing the coefficients on each of these interaction terms. We expect that the educated, interested, and strong partisans will be much less affected by campaign spending, that is, the coefficients will be smaller and less significant in these cases.

Table 2 and Figures 2-4 show the results when these three estimations are made. The evidence clearly supports our hypotheses. Campaign expendi-

TABLE 2 Vote for Challenger by Incumbent and Challenger Expenditures and Respondent Demographics: Equations with Interaction Terms

INDEPENDENT	EQUATIONS WITH INTERACTION TERMS FOR		
VARIABLES	EDUCATION	INTEREST	STRENGTH OF PID
Intercept	-2.0 (.60)**	2.2 (.55)**	-2.4 (.54)**
Same party as challenger	1.6 (.21)**	1.6 (.21)**	1.6 (.21)**
Reagan approval	52 (.12)**	51 (.12)**	−.51 (.12)**
Economic expectations	.20 (.14)	.20 (.14)	.20 (.14)
Sex	.08 (.18)	.07 (.18)	.08 (.18)
Age	.008 (.006)	.009 (.006)	.009 (.006)
Race	24 (.39)	30 (.39)	–.31 (.39)
Education	.016 (.065)	.07 (.05)	.07 (.05)
Income	004 (.02)	006 (.02)	006 (.02)
Coattail	.95 (.15)**	.93 (.15)**	.94 (.15)**
High education ^a	.003 (.70)		***************************************
Interest ^b		52 (.58)	
Strength of party ID ^c			11 (. 64)
Challenger\$ × high ^d	.0010 (.0027)	.0036 (.0024)	.0047 (.0029)
Challenger\$ × low ^e	.0077 (.0019)**	.0067 (.0020)**	.0058 (.0018)**
Incumbent\$ × high ^f	0016 (.0030)	0021 (.0026)	0036 (.0032)
Incumbent\$ × low ^g	0060 (.0020)**	0056 (.0021)**	0045 (.0019)*
Model chi-squared	299.8	29Ì.7	290.5

Note: Dependent variable is coded 1 if respondent reported voting for the challenger, 0 otherwise. Logit estimates. Standard errors are in parentheses. N =

^a1 if respondent completed a college degree of some sort; 0 otherwise.

^{*}p < .05.
**p < .01.

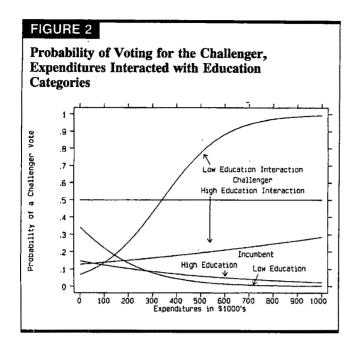
b1 if respondent reported being very much interested in the political campaigns of 1984, 0 if somewhat or not much interested. c1 if respondent identifies strongly with one of the two parties, 0 otherwise.

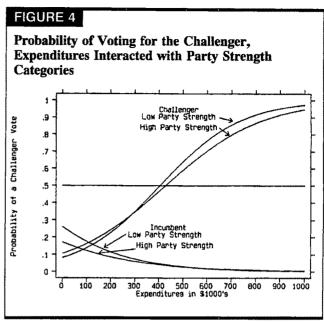
dChallenger expenditures (educated, interested, strong partisans). Col. 1: challenger expenditures for respondents who score 1 on high education variable, 0 otherwise. Col. 2: challenger expenditures for respondents who score 1 on interest variable, 0 otherwise. Col. 3: challenger expenditures for respondents

who score 1 on strength of party ID variable, 0 otherwise.

'Challenger expenditures (less educated, less interested, not strong partisans). Col. 1: challenger expenditures for respondents who score 0 on high education variable, 0 otherwise. Col. 2: challenger expenditures for respondents who score 0 on interest variable, 0 otherwise. Col. 3: challenger expenditures for respondents who score 0 on strength of party ID variable, 0 otherwise.

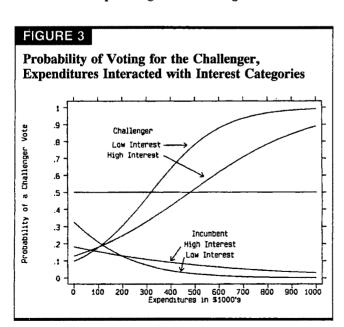
Incumbent expenditures (educated, interested, strong partisans). Same coding for incumbents as described for challengers in n. d. *Incumbent expenditures (less educated, less interested, not strong partisans). Same coding for incumbents as described for challengers in n. e.





tures affect individuals in precisely the way that theories of persuasive communication would predict. Table 2 (column 1) and Figure 2 show that individuals with college degrees are unaffected by spending from either the incumbent or the challenger, whereas those without degrees exhibit very large and significant effects in each case (compare with the baseline result in Table 1, col. 2). Regardless how we interpret this result (skills learned from schooling or traits brought to bear on the education process in the first place), it seems clear that highly educated individuals base their vote decision on factors other than the campaign messages that result from spending money.

Table 2 (column 2) and Figure 3 show the results when interest in campaigns is considered in interaction with the spending variables. Again, individuals



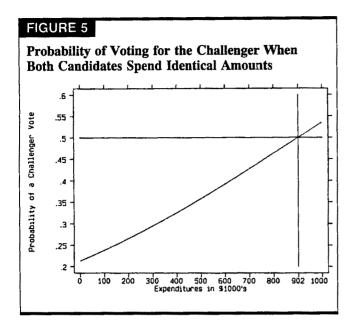
very much interested in political campaigns are much less affected by campaign spending than are those who are only somewhat or not much interested. Less interested individuals exhibit large and significant effects when compared with their more interested counterparts. Although the differences are not quite as pronounced as they were in the case of education, they are still noticeable. Interested individuals undoubtedly find plenty of other sources of information about candidates and do not rely on campaign communications as greatly as less interested individuals.

Table 2 (col. 3) and Figure 4 show the results when strength of partisanship is considered in interaction with the expenditure variables. Once again our expectations are supported. Strong partisans are less affected by spending than are weaker partisans, although the differences are not as great as in the previous two cases. Firmly held beliefs are harder to influence and these results clearly support this notion. Candidates have operated under this assumption for some time, making a pitch for the wavering voters an important part of their campaigns.

SUMMARY AND DISCUSSION

We have argued that the single-equation model of congressional vote choice incorporating actual candidate expenditures is misspecified. As an alternative, we develop a multiequation model in which expenditure variables are endogenous. Our results indicate that the estimates in the single-equation case are biased and inconsistent. Both the challenger and the incumbent expenditure terms are significant in the multiequation case, whereas only challenger spending achieved significance in the single-equation estimation. In addition, the magnitude of these effects is much greater in the multiequation model.

We have also attempted to determine the types of



individuals most affected by these campaign expenditures. Typically, the assumption is made that voters' decisions are not affected by the money per se but rather, that money represents persuasive communications to the electorate, so more money means greater persuasive ability. Deriving our hypotheses from persuasive communications theories, we find that voting behavior is conditioned by the interaction between spending and particular individual characteristics such as education, interest in campaigns, and the strength with which attitudes are held. Respondents with college degrees are unaffected by candidate spending, whereas those with less than a college degree exhibit very large and significant effects. The same pattern emerges for respondent interest in the campaigns and strength of partisanship, although these affects are not quite as pronounced as in the case of education.

These results are not strictly comparable to those reported in the aggregate studies because we operate at a different level of analysis (see Green and Krasno 1988, 1990; Jacobson 1978, 1980, 1985, 1990). They do, however, touch on the main points of contention in the campaign spending literature. First, spending benefits incumbents as well as challengers, although not quite to the same degree. The probability that an individual will vote for each candidate increases as that candidate spends more money. These results support the contentions of, most notably, Green and Krasno (1988, 1990), who argue that incumbents, as well as challengers, benefit from campaign expenditures.

Second, these results may help inform decisions on certain policy recommendations designed to reduce the advantages of incumbency, such as public financing with spending limits for incumbents and challengers. Figure 5 shows the probability of a vote for the challenger when spending by each candidate is equal. The procedure is similar to that employed in Figures 1–4, except that both expenditure terms are

allowed to vary at once. It is clear from this figure that incumbents are advantaged at equal levels of spending until expenditures reach \$902,000 (1984 dollars). It is not until expenditures reach this level that the probability of a vote for the challenger reaches .5, that is, equal spending produces an equal chance of receiving a respondent's vote. While this result seems to suggest that relatively high public assistance and spending limits are necessary to achieve parity, it does not mean that limits and assistance at lower levels could not greatly improve challenger chances (as Figure 5 shows), especially given that challengers typically spend far less than that proposed in recent spending limits legislation.

Finally, the additional knowledge of who is most and least affected by campaign spending suggests that campaigns can perhaps target their resources to an even greater degree than they do now. A common statement made by campaign strategists is that half the money they spend is wasted, the problem being they do not know which half. Our results begin to address this question by describing characteristics of individuals who are unaffected by the expenditures. Table 2, column 3, for example, suggests the overlap (hence loss of efficiency) between spending by political parties to strengthen ties and spending by candidates to influence preference. In general, Table 2 suggests that the individuals most responsive to the campaign spending are also those least likely to vote, since they share many of the same attributes (low interest, low education, and weak partisan ties). A prior analysis has shown that campaign spending, at least for challengers, has its greatest effect on individuals who do not vote (Kenny and McBurnett 1992, 935). In light of this, candidates might consider stepping up efforts to stimulate turnout among groups that traditionally participate at low levels, since a committed person who does not vote does not help the cause.

APPENDIX: DESCRIPTION OF SUBSAMPLES OF 1984 AMERICAN NATIONAL ELECTION STUDY USED IN THE ANALYSES

The data set used in the analyses was constructed by merging campaign-specific information gathered from various sources (see data description in text) with the 1984 ANES study. This procedure created a data set of 2,257 respondents residing in 134 districts. Estimation of the first-stage equations reduces these numbers to 1,368 respondents residing in 103 districts. This reduction occurs because certain respondents have missing values for variables included in the estimations. For the most part, excluded respondents reside in districts that either were not competitive (no challenger) or were for open seats (no incumbent) in 1984 and so are missing on variables such as challenger quality and challenger prior political support. Of the 1,681 respondents residing in compet-

itive districts with an incumbent running, 313 are missing on some other exogenous variable, most commonly the income and/or economic expectations variables. Estimation of the second-stage vote equation reduces these numbers to 938 respondents residing in 96 districts. This reduction occurs because 430 of the 1,368 respondents who survived the first-stage estimations are missing on the congressional vote variable, either because they did not vote at all or because they voted for president but did not vote in the House race.

Fortunately, the distributions on key variables do not change much as the sample size is reduced. Mean challenger spending was around \$117,000 in the 103 competitive districts (first-stage equations) compared to \$118,000 in the 96 districts used in the second-stage estimations. For incumbent spending, these numbers are \$275,000 and \$273,000, respectively. In terms of vote choice, 25.6% of respondents who reside in the 103 competitive districts voted for the challenger, compared to 26.0% of respondents in the 96 districts used in the second-stage vote equation.

Notes

We wish to thank Charles Franklin for his helpful comments on an earlier version of this manuscript.

- 1. Questions may arise as to whether the 1984 ANES study was designed to give representative samples from congressional districts. It is our contention that the sampling within districts is not relevant for the estimation of our model. We have a national sample of voters who were exposed to various campaign stimuli (money) and had personal characteristics relevant to voting (party identifications, etc.). We do not aggregate individuals within districts to estimate district characteristics but rather, estimate an individual-level model of vote choice in which one element of the choice is the candidate spending. In order to estimate the model we need (1) variation in spending across voters (which we have), (2) an assumption that spending (and all other effects) are constant across districts (possibly debatable but certainly the normal practice in modeling congressional voting), and (3) correct model specification. Therefore, whether the sample is representative of the district or not is irrelevant for the analyses we conduct. For an early discussion of this question, see Miller and Stokes 1966.
- 2. This is a standard method of purging any correlation between the instruments and the error terms in the equations. The presence of any such correlation produces inconsistent and biased estimates (Johnston 1984). Bartels explicitly addresses the appropriateness of instrumental variables in a model similar to that used here (1991, 791-93). His model uses aggregate data and does not improve on previous 2SLS estimates of incumbent expenditures. (The t-values indicate that, at worst, the estimate in his model for incumbent spending is zero, and at best, the estimate is inefficient.)
- 3. Many studies have argued that campaign expenditures are subject to diminishing returns and that variables measuring spending should be transformed in some way. We argue that diminishing returns in the expenditure variables are captured in the logit estimations of the equation, which forces the expenditure effects onto the 0, 1 interval. We also estimated the model using the customary logarithmic transformation of the expenditure variables and found little substantive difference from the reported results. The coefficient for incumbent expenditures decreases slightly in significance (p < .10) but remains properly signed.
 - 4. Since our individual-level analysis uses a different de-

pendent variable (vote choice) than has been used in the few previous studies of this sort (preelection candidate preference and candidate familiarity), the reader might be interested to know what difference this made in and of itself. Unfortunately, there is no preelection candidate preference measure in the 1984 ANES (or any other year for that matter) study so a comparison with Jacobson's (1990) results is not possible. When a measure of candidate familiarity is included as a dependent variable (instead of vote choice) in the estimation, the coefficient for challenger spending is significant, but incumbent spending is not. This result holds for both the single and multiequation formats. These results are consistent with those obtained by Jacobson (1978).

5. Obviously, there are other factors that may mediate the effect of spending in congressional campaigns. For example, the medium through which the voter is reached (television, mail, personal contact, etc.) may influence the impact of the message. We take this question up in a separate effort (Kenny and McBurnett 1993).

6. For reviews of this literature, see Lindzey and Aronson 1985; Milburn 1991; Smith 1982.

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CONTROVERSY

POPULAR INFLUENCE ON SUPREME COURT DECISIONS

In their 1993 article in this Review, William Mishler and Reginald Sheehan reported evidence of both direct and indirect impacts of public opinion on Supreme Court decisions. Helmut Norpoth and Jeffrey Segal offer a methodological critique and in their own reanalysis of the data find, contrary to Mishler and Sheehan, no evidence for a direct path of influence from public opinion to Court decisions. Instead, they find an abrupt-permanent shift of judicial behavior consistent with an indirect model of influence whereby popularly elected presidents, through new appointments, affect the ideological complexion of the Court. In response, Mishler and Sheehan defend the direct public opinion linkage originally noted, at both individual and aggregate level; respond to the methodological critique; and offer further statistical analysis to support the aggregate linkages.

COMMENT

Among the most important questions that can be asked about the Supreme Court is the role of this unelected and democratically unresponsive institution as a policymaker in the national political system. Supporters of the Court usually make one of two defenses of the institution. First, in what is commonly known as the Madisonian response, supporters claim that the Court is not supposed to be a democratic institution. Its raison d'être is to check the excesses of legislative majorities. The second response is of more recent lineage, tracing its history not to James Madison but to Robert Dahl (1957). According to Dahl, it simply does not matter very much if the Court is unelected and thus procedurally undemocratic because it is responsive to national majorities and thus substantively democratic.

Dahl's explanation for Supreme Court responsiveness was quite simple: the people elect the president and the president selects the justices with the advice and consent of the Senate. Thus, with the exception of the onset of critical or realigning periods, the president's appointment power should ensure a Court whose views represent those of the dominant political coalition.

Mishler and Sheehan (1993b) take Dahl's argument a step further. They argue that Supreme Court decisions not only correlate with public opinion but are directly influenced by public opinion. According to Mishler and Sheehan, the justices "are broadly aware of fundamental trends in ideological tenor of public opinion, and that at least some justices, consciously or not, may adjust their decisions at the margins to accommodate such fundamental trends" (p. 89). After presenting a variety of time-series models, they argue that Supreme Court justices between 1961 and 1981—but not thereafter—were influenced by whatever levels of liberalism existed in public opinion five years earlier. No significant influence is found for contemporaneous public opinion, public opinion lagged at less than five years, or public opinion lagged at more than five years. Because of what we consider to be the questionable nature of their

main finding, we decided to investigate the purported relationship further.

We first discuss the theoretical reasons why it is unlikely for Supreme Court decisions to be directly swayed by public opinion, especially at substantial lags. (This is not to say that Court decisions may not correspond to public opinion through other pathways.) We then conduct a methodological audit of Mishler and Sheehan's time-series analysis. One of the problems is that the authors perform an aggregate-level analysis on an individual-level theory. As we shall show, an examination of the behavior of the individual justices provides little support for their claims. Moreover, their aggregate analysis overlooked a sizable shift of the Court series in 1969–71. That change, corresponding to the transition from the Warren Court to the Burger Court, seriously confounds the estimation of popular influence on Court behavior. When the Court realignment is introduced into the analysis, the effect of public mood vanishes. The same happens when we conduct separate estimations for the Warren era and the Burger-Rehnquist era. The relationship also disappears when we control for contemporary Court ideology, rather than (as Mishler and Sheehan do) lagged ideology. Whatever influence public opinion exercises on the Court occurs indirectly, through the choice of justices by presidents chosen by the people.

SUPREME COURT RESPONSIVENESS TO PUBLIC OPINION

We recognize from the outset that Supreme Court decisions might correlate with public opinion. Dahl's presidential appointment hypothesis is convincing. Indeed, numerous scholars have found that the Court is not generally out of line with public opinion. The most exhaustive examination of this issue to date found that out of 130 Supreme Court decisions that could be matched with public opinion polls, 82 (63%) were consistent with poll pluralities (Marshall 1989, 78). Mishler and Sheehan find a contemporaneous correlation of .56 between public opinion and Supreme Court decisions, though this drops to -.01

once the autocorrelation of both series is removed. Whatever the real-world correlation might be, it is unlikely to be the result of a direct influence of public opinion on the Court.

The public has substantial influence over the votes cast by members of Congress (e.g., Bartels 1991; Fiorina 1974; Kalt and Zupan 1984; Miller and Stokes 1963). This, of course, is because they are electorally accountable. Supreme Court justices, with life tenure, have no personal incentives to allow such influence. If, in fact, they do so, it is because they feel that they ought to (Gibson 1991).

If the justices were behaving in such a civic-minded fashion, one might imagine that they would advertise such actions to their public. Occasionally, statements in their opinions do suggest the importance of public opinion. Mishler and Sheehan bolster their theoretical claim that the Court is influenced by public opinion with one such quote from Justice Frankfurter and another by Justice Rehnquist (1993b, 89). A more systematic analysis, however, demonstrates how rare such claims are. Public opinion is mentioned in less than 2% of majority opinions (Marshall 1989, 35–36), and in only one-fifth or so of the cases in which it is mentioned do the justices suggest that law should reflect public opinion (ibid., 35–39).

In certain substantive areas, especially death penalty cases, the Court does discuss the importance of public opinion with a frequency greater than nil, but the justices' misuse of public opinion polls there shows how little regard they in fact have for that opinion. Justice Marshall, in Gregg v. Georgia (1976, 232) argued that the Court should be guided not by what public opinion polls say but by what they would say if the public were fully enlightened on the particular subject. On the flip side, Justice Scalia, speaking for the Court in Penry v. Lynaugh (1989, 289) argued that public opposition to the death penalty for the retarded is not proven by the fact that the citizens of Texas, Florida, and Georgia-three states overwhelmingly in favor of the death penalty—opposed such executions by majorities of 73%, 71%, and 66%, respectively.

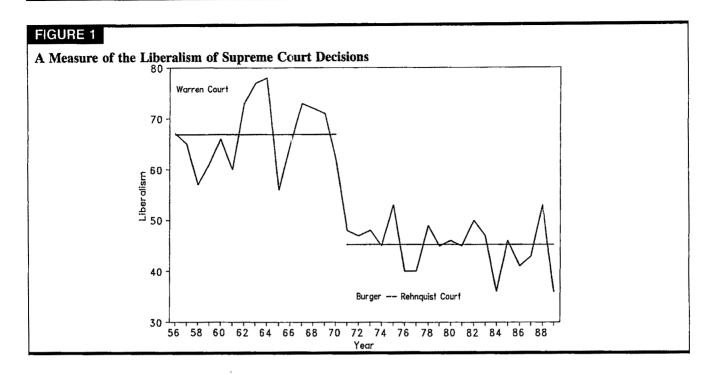
More typical, we presume, of the justices' beliefs, is the view of Justice Jackson: "The very purpose of the Bill of Rights was to withdraw certain subjects from the vicissitudes of political controversy, to place them beyond the reach of majorities and officials and to establish them as legal principles to be applied by the courts. One's right to life, liberty and property, to free speech, free press, freedom of worship and assembly, and other fundamental rights may not be submitted to vote; they depend on the outcome of no elections." (West Virginia Board of Education v. Barnette 1943, 638. The overwhelming indifference of the justices' opinions to public opinion supports the perceived validity of Jackson's statement. It is possible, nonetheless, that the justices react to public opinion even though they may not be aware that they do so, which is where political science enters the picture.² Empirical research on this question is far from complete (that is no doubt why Mishler and

Sheehan embarked on their study), but the evidence that does exist shows that while elected state court judges respond to public opinion (Brace and Hall 1990; Gibson 1980; Kuklinski and Stanga 1979), lifetenured federal court judges show no such influence (Giles and Walker 1975; Songer, Segal, and Cameron 1994).³

Despite this evidence, let us grant the possibility that the Court might be directly influenced by public opinion. Even if it were true, we find it extremely difficult to accept that the Court is influenced by public opinion that is five years old, regardless of the direction that opinion has taken in the intervening years. Yet that is the claim that Mishler and Sheehan make. Their justification is as follows: "Even where the Court responds directly to perceived changes in public opinion as proposed by the political adjustment hypothesis, it still takes time for justices to perceive a change in public mood. More importantly, it takes time for societal trends to be absorbed and reflected in the attitudes and beliefs of the justices or for justices to become convinced that the change is of sufficient salience, magnitude, and endurance as to threaten the Court's authority" (1993b, 91-92).

It is hard to state precisely what one would predict about judicial behavior from such a statement. If it takes a significant amount of time for the justices to learn of the public mood, then there conceivably might be distant lags without contemporaneous ones, as in the five years that Mishler and Sheehan find. But if the justices need to be convinced that the change is salient enough to threaten the Court's authority, then it must be the case that the justices perceive that the change has endured, for there could not be a threat to the Court's authority from public opinion five years removed. There are two contradictions here. First, if the Court only acts on change that has endured, their decisions should be influenced by contemporaneous as well as lagged public opinion. Yet Mishler and Sheehan find no support for contemporaneous opinion. Second, since it takes long periods of time for the justices to learn about changes in public mood (a dubious assumption in and of itself), the justices would never know whether any change five years old had endured to the present.

The theoretical plausibility of the influence of a five-year lag might best be assessed by trying to imagine the internal processing—be it conscious or subconscious—that would lead to such behavior. A justice might, for instance, think: "X is the decision that I prefer (or that I believe the Constitution requires). But public opinion is much more conservative on this issue than I am. Because we are, but should not be, an undemocratic institution, I'll give the public the benefit of the doubt and vote Y." What we find far more questionable is that a justice would say: "X is the decision I prefer, but public opinion five years ago (but not necessarily any longer) was much more conservative on this issue then than I am now. Because we are, but should not be, an undemocratic institution, I'll give the public what it wanted five years ago." To ask whether a justice could think this



way is to answer the question. In that case, however, we must ask how Mishler and Sheehan could have amassed such convincing evidence for a lagged effect of mass opinion.

THE FIVE-YEAR LAG

It is important to note that the evidence comes from an analysis of time-series data. Time, as one sage put it, is a great teacher, but it is also a mischievous one, who loves to play tricks on its students. Over time, many things rhyme for no reason whatsoever, like Super Bowl victories and stock market rallies. This tends to occur rather frequently when observations are in short supply. In their analysis of Court behavior, Mishler and Sheehan work with only 34 observations; that number effectively shrinks to 29 when the public opinion series is lagged five years. Tests of statistical significance may afford some protection against falling victim to artificial relationships, but time, again, has its ways of disarming those alarms. This is especially true when one searches a large number of lags, as Mishler and Sheehan do. While the probability of falsely finding a significant lag for any particular lag is .05, the probability of falsely finding at least one significant lag when one searches lags 0-10 is 1-.95¹¹, or .43.4

To be sure, Mishler and Sheehan are wary of the pitfalls of time series and display great care in avoiding many of them. In particular, they identify the five-year lag from ARIMA-prewhitened cross correlations and control for serial dependence of errors in all their structural estimations. They also test for parameter stability over time and make a substantial adjustment as a result of that test. Nonetheless, Mishler and Sheehan make some judgment calls (or, better,

noncalls), with game-breaking consequences in their time-series analysis.

In a nutshell, the authors treat the Supreme Court during the 1956–89 period as one when it is really two very different courts: a Warren Court and a Burger–Rehnquist Court. The decisions handed down by those two courts have little ideological ground in common. The contrast is especially sharp when (as in the analysis at hand) annual aggregates are used. Figure 1 plots those annual values, as graciously provided to us by Mishler and Sheehan. There is not a single year in which the Warren Court comes out on the conservative side of the mean level for the whole period and not a single year in which the Burger–Rehnquist Courts wind up on the liberal side.

At the very least, such behavior violates the assumption of mean-stationarity. A series is stationary in the mean if its realizations derive from a distribution with a fixed central tendency (Granger and Newbold 1986, 4). That does not hold true for the Court behavior series from 1956–89. Instead, the outcomes of the Warren Court fluctuate around one ideological level and those of the Burger–Rehnquist Court around another one. Failure to control for this gap is bound to confound the effect attributed to any explanatory variable (however plausible) and to give rise to spurious dynamic effects.

A simple way to control for the sharp contrast in Court behavior is to inquire whether the hypothesized relationship between public mood and Court behavior can be confirmed for each Court separately. For that purpose, Table 1 presents correlations up to five lags between Court liberalism and public mood for the Warren Court and the Burger–Rehnquist Court. As can be seen, the correlations at lag 5 are small and insignificant for both Courts, nor are any other correlations significant. There is no evidence

TABLE 1

Correlations between Public Mood and Supreme Court Decisions by Court Era

	SUPREME COURT DECISION		
PUBLIC MOOD	WARREN ERA	BURGER- REHNQUIST ERA	
Unlagged	.03	13	
Lagged one year	.10	.05	
Lagged two years	04	.03	
Lagged three years	.06	.05	
Lagged four years	.25	.14	
Lagged five years	.24	.10	
Standard errors	(.2632)	(.23)	
Number of cases	10–15	19	

Note: Correlations are the Pearson correlation coefficient (R). For the Warren era, the standard errors grow with increasing lags because of a shrinking number of available cases.

that either the Warren Court or the Burger–Rehnquist Court was responsive to variations of the public mood, no matter what the time lag.

Instead of splitting up the time series, we can also reexamine the relationship between public mood and Court behavior by adding a specification for the transition from Warren Court to Burger-Rehnquist Court. For comparison's sake, Table 2, column 1, reproduces results for the whole Court series in the absence of such a specification. Those results show a significant effect for public mood lagged five years as well as for the autoregressive error structure. But these estimates change dramatically when the transition from Warren Court to Burger-Rehnquist Court is introduced. This Court realignment (as we have called the transition) shifted the balance of Court behavior from liberal to conservative in the manner of an abrupt-permanent intervention (Box and Tiao 1975; McDowall et al. 1980). The control for this intervention wipes out the apparent effect of public mood on Court behavior. In addition, the control exposes the errors as being purely random, not first-order-autoregressive, as specified by Mishler and Sheehan. That specification also proves to be an artifact of overlooking the Court realignment.

How, then, the patient reader will ask, could the estimate for a five-year lagged effect of public mood arise? The answer is that exactly five years prior to the Court realignment the mood of the American public, as captured by the Stimson measure, must have undergone a swing of staggering dimension. That is indeed what happened. In 1965–66, the mood series falls from a high point of liberalism to a deeply conservative level. The tandem movement of the Supreme Court five years later is enough, with fewer than 40 observations, to create the appearance of a five-year lagged effect of public mood. As demonstrated in Table 2, were it not for this singular parallel in the two series, no finding of an effect with a five-year lag would have emerged.

Granted, there is some broad semblance between the contours of public opinion and Court behavior, with the generally liberal Court decisions of the Warren era corresponding to a like-minded public mood and the more conservative decisions of the Burger–Rehnquist era doing so for a more conservative public. Nonetheless, compared with Court behavior, the public is far more "moody," shifting frequently from liberal to conservative and vice versa. There is no evidence that the Court pays any attention to all those mood swings of the general public, either immediately or five years later. What remains is that in one singular incident a change of Court behavior duplicated a shift in public opinion five years earlier.

Whatever the reasons may have been for the public mood swing in the mid-1960s, there is little mystery about what changed the Court's behavior in the early 1970s. Within barely two years of taking office in 1969, President Nixon was able to replace four of the nine justices, every one of the newcomers being decidedly more conservative than his predecessor. In their first year of service, the new members (Burger, Blackmun, Powell, and Rehnquist) averaged a liberalism score of 29 compared with an average of 64 for their predecessors (Warren, Fortas, Black, and Harlan) in their last year. A "generational replacement" of such volume and contrast is surely capable of accounting for the Court's ideological realignment in the early 1970s.

Among the remaining justices of the Warren Court, Douglas, Brennan, and Marshall remained loyal to their liberalism, and Stewart to his moderate position. As the correlations for individual justices in Table 3 indicate, the public's mood had no significant bearing on the decisions of the three liberals, regardless of the time lag. For Justice Stewart, we detect a statistically insignificant hint of some instan-

TABLE 2

Supreme Court Decisions as a Function of Public Mood Lagged Five Years and Court Realignment

INDEPENDENT	COEFFICIENTS		
VARIABLES	I	II	
Constant	53.21** (3.63)	66.21** (2.04)	
Public mood (t - 5)	.61** (.26)	.19 (.15)	
Court realignment	_	19.92** (3.00)	
AR(1)	.60** (.17)	09 (.19)	
Ljung-Box Q(8)	2.9	4.8	

Note: Standard errors are given in parentheses. Court Realignment is a binary variable scored 0 for the years 1956–70, and 1 afterward. The public mood series was centered before model estimation to make the constant more interpretable. AR is the autoregressive parameter. $^*p < .05$. $^*p < .01$.

TABLE 3

Correlations between Public Mood and Decisions of Individual Justices

Public	Decisions of				
mood	Douglas	Brennan	Marshall	Stewart	White
Unlagged Lagged one	.03	.16	.21	.39	.15
year Lagged two	.06	.16	.16	.32	.23
years	.11	.20	.23	.24	.32
Lagged three years Lagged four	.23	.16	.08	.24	.27
years Lagged five	.25	.17	.07	.29	.46*
years	.18	.18	.10	.14	.47*
Standard errors Number	.22–.26	.17–.19	.21–.24	.20–.23	.192
of cases	18-23	29-34	18-23	19-24	23-28

Note: Correlations are the Pearson correlation coefficient (R). The standard errors grow with increasing lags because of a shrinking number of available cases. *p < .05.

taneous effect, but nothing with a delay of five years. Only one justice appears to exhibit in his individual behavior what the Court underwent as a whole. There exists a bivariate correlation between Justice White's votes and public opinion five years earlier. And like the Court as a whole, this justice shifted from liberal to conservative during the transition years. But whether his conversion can be attributed to the public's turn toward conservatism five years earlier is doubtful. After statistically controlling for his abrupt-permanent shift, White shows no evidence of responding to public mood. Alternatively, prewhitening renders the lagged correlations at four and five years insignificant. Further evidence is provided by the fact that he has shown little inclination to follow other mood swings of the general public, especially the liberalization of the 1980s. Why would he have reacted to public opinion then, but not more recently? White's conservative shift in the early 1970s could be just as plausibly (if not more so) attributed to a new conservative president (Epstein, Walker, and Dixon 1989), a new conservative Court and/or chief justice (Murphy 1964), or simply to changes in his own preferences that were totally uninfluenced by changes in public preferences half a decade earlier.

To be fair, let us acknowledge that Mishler and Sheehan are not oblivious to the change in the Court's ideological composition. Indeed they control for a compositional effect by introducing a measure of ideological preferences designed by Segal and Cover (1989). While the addition of a Court membership variable does attenuate the effect of public mood, Mishler and Sheehan are still able to report significant results. It is not a trivial feature of this control,

however, that Court membership enters the analysis with a lag of one (year). In other words, the authors suppose that the Court's decisions this year derive from the Court's ideological complexion last year. But why last year and not this year? In particular, how telling are last year's ideological inclinations for this year's decisions when the membership of the Court has greatly changed? As Table 4 shows, when ideological composition enters the analysis unlagged, the effect attributed to public mood disappears.'

In an important way, Mishler and Sheehan themselves qualify the finding that the Supreme Court is responsive to public opinion. The authors note that "the ideology of the public mood and the decisions of the Court have diverged rather sharply since President Reagan's election in 1980" (1993b, 94). To accommodate this divergence they include an interactive term for public mood during those years. The estimate for this interaction is negative, while the estimate for mood across the whole period is positive. Taken together, this suggests that during roughly one-third of the period examined, public opinion had little effect on Court behavior. But why did the Court suddenly prove unresponsive to public opinion? There is no evidence that the Court's ideological tenor changed in 1981. What did change was public opinion. Surprisingly, Stimson's mood measure swings toward the liberal side during the Reagan years. It is a turn that the Court did not follow in its decisions. But if the Court appears free to ignore the public mood at one moment, it may be simple coincidence that its decisions square with the public mood at another moment.

In short, Mishler and Sheehan's aggregate results are seriously confounded by a violation of the mean-stationarity assumption (to put it technically). But even if we ignored this assumption, their estimates would have proved insignificant had they controlled for contemporary Court ideology, not lagged Court ideology. Moreover, even without this control (and again ignoring the stationarity problem), the results they provide are entirely due to a shift in public mood in 1965–66 that coincided with a change of Supreme Court voting behavior in 1970–71. That result, as our

TABLE 4

Supreme Court Decisions as a Function of Lagged Public Mood and Contemporaneous Court Composition

INDEPENDENT VARIABLES	COEFF.	S.E.
Public mood (t - 5) Court composition (t) AR(1)	.09 3.29* 35 × 10 ⁻²	(.19) (.59) (.19)
Ljung-Box Q(8)	5.1	

Note: All variables were centered prior to model estimation—hence, no constant. N = 34. AR is the autoregressive parameter.

*p < .01.

individual-level analyses demonstrate, is virtually entirely due to changes in the Court's composition. Finally, the authors' extensive search for lags ignores the corresponding increase in the probability of a type I error.

DISCUSSION

Does public opinion influence Supreme Court decisions? If the model of influence is of the sort where the justices set aside their own preferences and abide by what they divine as the *vox populi*, our answer is a resounding *no*. In particular, we find no evidence that such an influence occurs with a delay of five years. What we do find, however, is that a sizable ideological shift in Court behavior took place in 1970–71 in the wake of an unmistakable swing of public opinion roughly five years earlier. This might be pure coincidence. With no more than one incident of this kind in the data available, caution may be well advised. On the other hand, this incident may be consistent with a model of influence, but of a very different sort.

While justices are not accountable to the populace, presidents and senators, who share the power to choose them, are. Whatever configuration of public opinion elects a president, in particular, could transpire in his or her Court appointments. Given plentiful opportunity to put new members on the Court, a newly elected president can alter the Court's ideological complexion. It is not that the justices pay keen attention to public opinion but that they have been chosen by a president (with the advice and consent of the Senate) who presumably shares the public's views. If, however, the president is elected on the basis of issues orthogonal to those decided by the Court or in the absence of voter concern for ideological issues, then there is no reason to believe that his or her appointees will reflect public opinion in their judicial opinions. This is the crucial difference, from a responsiveness standpoint, between justices who are directly (versus indirectly) influenced by public opinion. Supreme Court responsiveness will not necessarily exist but will be dependent on the nature of presidential elections. For instance, if Ronald Reagan had been elected in 1980 because of the economy and foreign affairs and not his conservative social agenda, then we would expect little match between public mood and Court decisions in the 1980s. Alternatively, if we assume that the public's mood swing in the mid-1960s helped carry Richard Nixon to the presidency, we may be able to credit the public with an indirect hand in the Court's shift to the right in the early 1970s. By 1971, Nixon's appointments of new justices had definitely shifted the ideological balance of the Supreme Court to the conservative side. This shift was reinforced during the next 20 years, since all vacancies on the Court were filled by Republicans in the White House. Every quarter century or so, it seems, public opinion has its day in court, although it may take years to be sure.

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RESPONSE

In their wide-ranging critique, Helmut Norpoth and Jeffrey Segal raise both theoretical and methodological objections. They assert not only that we fail to justify the direct effects observed between public opinion and Supreme Court decisions but also that there are good theoretical and empirical reasons to doubt such effects. They also conduct "a methodological audit" of our analyses, alleging that we (1) use "aggregate-level analysis on individual-level theory," (2) specify an atheoretical lag structure of dubious empirical validity, and (3) fail to recognize or control serious problems of mean-nonstationarity in our measure of Court decisions.

In response, we argue first that theory and research on the impact of public opinion on Supreme Court decisions are both in short supply. None of the hypotheses in the literature, including that which they prefer, is derived from well-developed theory. Yet all are sufficiently plausible to warrant empirical scrutiny. Second, the debate over the impact of public opinion on Supreme Court decisions raises both aggregate- and individual-level questions, but the aggregate questions are logically prior and more important. Moreover, contrary to Norpoth and Segal's claim, data on individual-level effects are wholly consistent with our aggregate conclusions. Finally, we defend the lags in our models, demonstrate that nonstationarity is not a problem (in part because the principal variables are cointegrated), and argue that Norpoth and Segal misspecify our model. A properly specified model, estimated appropriately, confirms that public opinion has both direct and indirect effects on Supreme Court decisions during the Warren and Burger years.

THEORIES OF POPULAR INFLUENCE ON SUPREME COURT DECISIONS

Norpoth and Segal grant our larger point that the Supreme Court is responsive to public opinion. The controversy centers on whether the effects of public opinion are wholly mediated though the impact of presidential appointments on the ideological composition of the Supreme Court or whether public opinion can influence Court decisions more directly. The most perplexing criticism, they advance, is that we fail to justify the hypothesized direct effects of public opinion. In fact, we devoted more than a page to explaining possible effects, most of which discussion

focuses on the rationale for direct effects (1993b, 88–90). Importantly, the direct-effects hypothesis is *not* original to us. It has a long and distinguished pedigree, as we documented. Our contribution is

simply to test the hypothesis empirically.

The direct-effects hypothesis has two distinct explanations. One, linked to Justice Frankfurter, holds that the Court is a political institution whose authority depends on public deference and respect. Sensitive to this, justices are careful not to jeopardize the Court's authority by departing too far or too long from majoritarian views on fundamental issues. The theory is not that justices routinely change their decisions based on public opinion polls but that some justices occasionally modify their decisions (if not their personal beliefs) on important issues in response to long-term and fundamental changes in public opinion perceived as threatening the Court's authority. A second explanation, which we link to Justice Rehnquist, holds that justices are influenced (albeit often unconsciously) by the same forces that impinge society as a whole. Here, the argument is not that justices vote contrary to their beliefs in response to public sentiment but that the beliefs of at least some justices occasionally change in response to fundamental longterm shifts in what Stimson calls the public mood, and Lippman, the public philosophy. In retrospect, we probably erred in applying the label, "political adjustment hypothesis" to both versions. The Rehnquist perspective might better be labeled a "political socialization" or "attitude change" hypothesis. Both perspectives, however, predict that public opinion will influence Court decisions unmediated by presidential election/selection.

Norpoth and Segal go further, however, and assert that the direct-effects hypothesis is positively insupportable in theory and contradicted by existing research. They reason that since justices are not electorally accountable, they will respond to public opinion only if they "normatively feel that they ought to." Moreover, if justices respond to public opinion out of a sense of civic-mindedness. Norpoth and Segal claim that they certainly would advertise this in their decisions, which, apparently, they do not.

This, however, is a straw man. First, the hypothesis that direct effects can occur only if justices hold democratic role orientations is not one we advanced or discussed. Second, the supportive quotations cited from Justices Jackson, Scalia, and Marshall are normative statements to the effect that public opinion should not influence Court decisions. They do not address the empirical question whether public opinion does influence the Court or speak to the plausibility of either the political adjustment or socialization hypotheses.2 Third, the observation that justices rarely cite public opinion as justification for their decisions is irrelevant. If justices respond to public opinion via the political adjustment or socialization pathways, they are unlikely to advertise the fact that they are "caving in" to public pressure or have changed their minds in response to evolving social mores. Consider, for example, Segal and Spaeth's

(1993) argument to the effect that judicial decisions are driven principally by the attitudes and beliefs of individual justices. If this is true (and we are generally persuaded), in what percentage of cases does the majority defend its decision in terms of the justices' personal attitudes and beliefs? We doubt the figure is as high as 2%. The point, of course, is that such evidence is meaningless.

Norpoth and Segal also cite, without discussion, two articles that, they claim provide evidence contradicting public opinion effects (Giles and Walker 1975; Songer, Segal, and Cameron 1994). In fact, neither article addresses the issue in any substantial manner. Giles and Walker report a nonsignificant, zero-order correlation between an indirect measure of federal district court desegregation policies in 1970 and the percentage of votes cast in judicial districts for George Wallace in 1968—hardly a stern test of public opinion effects on Supreme Court decisions, nor do Giles and Walker claim otherwise. Songer, Segal, and Cameron examine whether individuals on the U.S. Courts of Appeals decide search-and-seizure cases according to personal policy preferences or the policy dictates of the Supreme Court. Public opinion is not addressed except in a single note claiming that the inclusion of Stimson's public mood variable in a multivariate model produced an insignificant coefficient. The results are not shown, and no mention is made as to whether the specification for public opinion was appropriately lagged, included an interactive term for public opinion during the Reagan years, or attended to other important methodological and specification issues. Since this is the totality of research cited in defense of the proposition that public opinion effects are contradicted by previous research, we think it fair to conclude that our decision to explore public opinion effects in a more systematic and rigorous fashion is reasonable and appropriate.

More generally, the attack on the theoretical plausibility of public opinion effects seems misplaced. What passes for theory in this area on both sides of the issue is little more than speculation mixed with substantial doses of wishful thinking and post-hoc rationalization. Norpoth and Segal have their preferred hypothesis. Other scholars and jurists suggest several others. None is deduced from first principles or is supported by much systematic research—which is why we chose to examine the issue more rigorously.

ON THE THEORY AND EVIDENCE OF A FIVE-YEAR LAG

Norpoth and Segal also take us to task for failing to justify the five-year lag specified for the impact of public opinion. They acknowledge our prediction that the political adjustment and socialization effects should occur at significant lags, but they criticize our failure to provide a "precise" theoretical prediction of the lag's length. This seems unreasonable. Existing

theory does not-and cannot reasonably-provide a precise specification of this lag any more than the Segal-Spaeth attitudinal model predicts the precise impact of judicial attitudes on Court decisions. Segal and Spaeth estimate empirically the impact of attitudes on Court decisions just as we use empirical methods to estimate lag-lengths. Consider a group of men during the 1960s and 1970s who began to comprehend the reality of gender discrimination when it was experienced by their wives and daughters seeking educational and employment opportunities. Precisely how long does theory predict it will take the average male to begin to reexamine and change long-held attitudes and values about the role of women? No doubt it varies across individuals. We know of no theory that specifies the precise lag or even a rough estimate. Even if such theory existed it likely would be of little help in identifying the lag specifically for the small and atypical group of Supreme Court justices. Rather than resort to post hoc rationalization, we think it preferable to admit the limits of theory and proceed to specify the lag-lengths

Norpoth and Segal express doubts about these empirical procedures, too. They argue that by searching across lags up to 10 years we had a 43% chance of "falsely finding" a significant lag purely by chance. First, the 43% figure holds only if the 10 correlations represent independent observations; they do not, since all are drawn from the same interdependent time series. Second, as Norpoth argues in his aggregate time-series research on British economics and elections, statistical tests of significance are not the acid test of the importance of a relationship when dealing with a historical population and very short time series. The small number of observations makes finding any significant relationships extremely difficult. At least as important as the significance of the correlation at the five-year lag is the fact that the size of this correlation (.40) is almost three times larger than the correlation at any other lag (see Mishler and Sheehan 1993b, tbl. 1). Third, because the public opinion and Court decision variables are both filtered by a powerful first-order autoregressive process (AR = .73), the probability of finding any significant correlation is severely circumscribed (see Norpoth 1986; Norpoth 1992, 141-42; Norpoth and Yantek 1983). When the two series are analyzed without prewhitening, as Norpoth and Segal suggest, all of the correlations are statistically significant at lags 0–5, though lag 5 is still the strongest. Finally, examination of cross correlations is only a first step in identifying appropriate lags. If a lagged correlation is falsely significant, the relationship should prove spurious when examined in a fully specified model. The five-year lag, however, remains statistically significant and robust when included in a properly specified multivariate model.

Finally, Norpoth and Segal ask why, if justices are influenced by public opinion at a lag of five years, there is no evidence of effects at earlier lags. This is a reasonable question and one we should have ad-

TABLE 5
Distributed Lag Estimates of Public Mood Effects on Supreme Court Decisions, 1956–1989

COEFFICIENT	ESTIMATE	S.E.
Mood,	12*	(.06)
$Mood_{t-1}$	06	(.05)
$Mood_{t-2}$	01	(.05)
Mood _{t-3}	.06	(.05)
$Mood_{t-4}$.12	(.07)
$Mood_{t-5}$.18*	(80.)
$Mood_{t-6}$.24*	(10)
Mood _{t-7}	.31*	(.12)

Note: Standard errors are given in parentheses. Lag estimates calculated from the coefficients for the first-order polynomial specification for public mood in the following Almon distributed-lag equation:

 $D = a_t + b_1 C_{t-1} + b_2 R_t + b_3 (R \times M)_{t-5} + b_4 M_t ** 0 + b_5 M_t ** 1 + u_t,$

where D= liberalism of court decisions, C= liberalism of court composition, R= intercept for Reagan years, $R\times M=$ interactive term for public opinion in Reagan years, and M= public mood. * $p\leq .05$ (one-tailed test).

dressed. Logically, both the political-adjustment and attitude-change hypotheses predict that public opinion will have a gradual, permanent impact on the Court's decisions. The impact should be small at the moment but should gradually increase over time before ending or leveling off at some impossible-to-predict future point. Nevertheless, we modeled the relationships as an abrupt-permanent effect at five years. We did so not because we were indifferent to the shape of the effect but because we were concerned with preserving precious degrees of freedom given a relatively complex model and a very small number of cases.

To determine the consequences of this decision, we have reestimated the impact of public opinion on the Court's decisions as a first-order polynomial in an Almon distributed-lag model.⁴ In addition to the specification for public mood, the model includes controls for Court ideology⁵ and the effects of public opinion during the Reagan years. The results, reported in Table 5, support the hypothesis of gradual effects albeit over about seven years rather than five. The results indicate that the instantaneous effect of public opinion on the Court's decisions is slightly though significantly negative. Rather than indicating that justices react perversely to changes in public opinion, the negative coefficient is better interpreted as a measure of the Court's inertia—a reflection of the time it takes justices to perceive and respond to a significant change in the public mood. The impact of public mood turns positive by year 3 and grows steadily larger through year 7. Importantly, year 5 is the first in which the positive lagged effects are statistically significant. Models (not shown) specifying maximum lag-lengths greater than seven years show insignificant effects for years beyond seven. Models specifying less than a seven-year effect have significant lagged coefficients similar to the sevenyear model but a weaker fit (i.e., smaller R-squareds). Clearly the five-year lag is a simplification of reality, but it seems a reasonable compromise between theoretical complexity and methodological necessity.

INDIVIDUAL VERSUS AGGREGATE ANALYSIS

Norpoth and Segal further charge that we perform "aggregate-level analysis on individual-level theory." It is true that individual-level theory underlies some of the aggregate-level hypotheses. We always intended to conduct additional research on individual dynamics and have subsequently done so (Mishler and Sheehan 1993a). However, these micro concerns are of secondary importance. The more important questions concern macro processes or cut across levels of analysis such that the appropriate level is at least debatable (see, e.g. Erbring 1989; Kramer 1983). As we made clear our interest is not to test theories of individual decision making but to answer a series of guestions about the role of the Court in democratic theory, specifically, (1) Are majority (i.e., aggregate) decisions of the Supreme Court broadly consistent with majority rule (i.e., aggregate public opinion)? and (2) Is the Court's aggregate responsiveness to majority rule mediated exclusively through the aggregate-level process of presidential election and judicial selection, or can it occur through other means as well? The important question for democratic theory is not whether individual justices respond to public opinion but whether decisions by the majority of the Court are countermajoritarian. Moreover, one need not reject methodological individualism to recognize that group outcomes often are not definable simply as sums of individual behaviors. Composition rules (i.e., how individuals interact in groups and how behavior is aggregated) also are important and can produce aggregate results quite at odds with individual actions (Brodbeck 1968).

Even conceding that individual dynamics are an important, subsidiary issue, we find nothing in the individual data reported by Norpoth and Segal to contradict our aggregate results. Although most of the 30 correlations in Table 3 between public mood and the decisions of five justices in one policy area are not statistically significant, every one of the correlations has a positive sign. If there really is no relationship between public opinion and individual decisions, we would expect about half of the correlations to be negative purely by chance. Assuming (erroneously, of course) that these are independent observations, the probability of obtaining 30 consecutive positive correlations from a population where the true correlation is zero is approximately ½30 or slightly less than one in a billion! This seems a reasonable indication that variations in the decisions of at least some Supreme Court justices are related to fluctuations in the public mood. Indeed, our subsequent research and related work by Wood and Flemming (1993) confirm that this is the case. Analyses of the 15 longest serving justices on the Court between 1953 and 1992 indicate that the decisions of as many as a third of the justices are significantly related to fluctuations in public opinion at various lags (Mishler and Sheehan 1993a).

Of course, it is not necessary for all members of the Court to respond to public opinion with equal sensitivity, at the same speed, or even at all. A single member, who is significantly affected by public opinion, such as Justice White, can alter majority decisions in cases where the Court is closely divided. Interestingly, the evidence in Table 3 that Justice White is significantly influenced by public opinion has considerable face validity. It accords with his reputation for having changed his personal views on a variety of issues during his tenure on the Court (see, e.g., Segal and Spaeth 1989). That he appears to respond to public opinion at a lag of four to five years, that this change occurred mostly during the late 1960s and early 1970s (not shown), and that his opinions become increasingly conservative with time all correspond remarkably well with our aggregate results.

Norpoth and Segal want to dismiss the evidence of Justice White's responsiveness to public opinion because most of the observed correlation disappears when the dramatic changes in public opinion that occurred in the mid-1960s are controlled. However, by discarding this most dramatic change in public mood, you effectively ignore the only substantial and sustained shift in public mood that occurs before the 1980s. Understandably, if you remove the principal source of variation from a short time series, the residuals are unlikely to correlate with much of anything. Given that Justice White's decisions become increasingly conservative over time, it is hardly surprising that eliminating the conservative mood swing in the 1960s significantly reduces the correlation between public opinion and his decisions. In effect, Norpoth and Segal demonstrate that when big shifts in public opinion are ignored, there is little evidence that the Court or its members are influenced by the small and seemingly random fluctuations in public opinion that remain. But then our argument always has been that the Court is most likely responding to substantial and sustained changes in the public mood. Far from contradicting our arguments, the individual-level data reported by Norpoth and Segal complement our aggregate-level results.

NONSTATIONARITY

A final criticism of our methods is that we violate the assumption of mean nonstationarity in analyzing the liberalism of Court decisions. Standard diagnostics confirm that the liberalism of Court decisions is nonstationary. The usual way to handle this is to difference or prewhiten the variable before analysis and then to apply the same transformation to the independent variables. Norpoth cautions, however, "these transformations risk 'throwing the baby out

with the bath water' ... and may yield positive results only under rare circumstances" (1992, 141-42). The technical problem is that the transformed series may be "overdifferenced" and important information lost especially regarding long-term effects.

Mindful of this, political scientists usually assume that nonstationarity in the dependent variable poses few problems as long as its residuals are free of autocorrelation after a properly specified analytic model is applied (Norpoth 1986; Norpoth 1992; Norpoth and Yantek 1983). This is the approach Norpoth and Segal use in their reanalysis of our data. They do not difference or detrend Court decisions prior to analysis but analyze the data as if it were stationary, apparently because the residuals are white noise. This is precisely the same approach we followed. Thus, according to the identical criteria Norpoth and Segal apply to their models, nonstationarity is not a problem in our original analyses either. The standard time-series diagnostics we reported indicate that the residuals of all of the models are white noise. In every case, the Durbin-Watson and Ljung-Box Q statistics are nonsignificant indicating that the residuals are appropriately free from autocorrelation (see Mishler and Sheehan 1993b, tbls. 2-3). Importantly, as well, inspection of the residual autocorrelation functions for our models (not shown) indicates that there are no significant residual correlations in any of the models at any lags up to 12 years. Thus nonstationarity does not distinguish our models and theirs.

There is, however, a different problem both with our models and with the alternatives proposed by Norpoth and Segal that we previously overlooked. The problem stems from the fact that the major independent variables, public mood and Court composition, are also nonstationary.6 Recent work in econometrics holds that if time series with similar levels of nonstationarity are regressed on one another and produce stationary residuals, then the series are cointegrated (Engle and Granger 1987). It now appears that the relationships between Court decisions and both Court composition and public opinion are cointegrated.⁷ This is true both for our models and those estimated by Norpoth and Segal. Although ordinary least squares regressions of cointegrated series produce consistent estimators, these estimates are not fully efficient, and the distributions of the estimators (standard errors) are generally nonnormal. Thus inferences cannot be drawn regarding statistical

Engle and Yoo (1989) propose à three-step procedure for adjusting ordinary least squares estimates and producing appropriate standard errors for cointegrated series. Table 6 reports both the original and the corrected estimates for the basic model (originally reported in Mishler and Sheehan 1993b, tbl. 2-B). In these models, the Courts' decisions are regressed against public mood (modeled for simplicity with a lag of five), Court composition (with a lag of one), and an interactive term for public mood during the Reagan years. The corrected results are very similar to the original. The principal differences are that the

TABLE 6 Original and Error-corrected Regressions of

Supreme Court Decisions, 1956-89 ERROR-INDEPENDENT **ORIGINAL** CORRECTED **VARIABLES ESTIMATES ESTIMATES** Intercept -4.87 -29.95 (25.57)(19.20)Public mood, 5 .52 .94* (.24)(.32)1.47* Court composition,_______ 2.73 (.79)(.80)Reagan/mood,-5 -.97-.32 (.55)(1.10)Reagan intercept^c 93.24 40.95 (59.04)(96.89)AR(1) -.52-.01 (.18)(.21)R2(reg)d .71 R2(tot) .78 Durbin-Watson 1.92 (ns)

Note: Estimates are maximum likelihood estimates. Standard errors are in parentheses and are consistent only for error-corrected estimates. N = 34.

5.42 (ns)

Ljung-Box Q

(df = 12)

corrected effects of public mood are somewhat larger and those of Court composition are somewhat smaller than originally reported. Nevertheless, both public mood and court composition have significant effects (.10, level) on the Courts' decisions at the specified lags.9 Of course, this strengthens our conclusions. Reestimations (not shown) of the other models in our original analysis also produce results very close to the original.

In sum, the alternative analyses reported by Norpoth and Segal have exactly the same statistical properties as those reported originally. All are stationary (because they are cointegrated, not because of the inclusion of dummy variables) and when appropriate error corrections are applied, all produce results consistent with those initially reported.

SPECIFICATION ISSUES

What, then, do we make of the fact that the inclusion of the dummy variable by Norpoth and Segal significantly attenuates the coefficient for the public mood and causes it to become statistically nonsignificant?

[&]quot;Index of the liberalism of public opinion on domestic policy concerns.

bSum of liberalism scores of Supreme Court justices at time of initial

Dummy varible coded 1 for years 1981-89, 0 otherwise.

Variance explained by model excluding autoregressive parameter.

Variance explained by model including autoregressive parameter. $p \le .05$ (one-tailed test). $p \le .01$ (one-tailed test).

What Norpoth and Segal do not show in their analysis is that inclusion of the same dummy variable in their model (Table 4) causes both public mood and Court composition to become nonsignificant. The same results obtain when the dummy variable is included in a model with Court composition as the only independent variable. 10 This is true, moreover, whether Court composition is lagged one year or not. Just as the dummy variable makes it appear that the effect of public mood on Court decisions is spurious, it has exactly the same effect on the impact of Court composition—suggesting, nonsensically, that neither Court composition or public mood influence the Court's decisions. The problem, again, is that by removing the principal source of variation from a short time series, it is almost inevitable that the series will fail to correlate with anything.

The problem with dummy variables, as Hanushek and Jackson point out, is that their use "admits to a lack of knowledge and/or data" (1977, 103). The dummy variable in these analyses indicates only that "something happened" in the late 1960s and early 1970s with respect to public opinion, the composition of the Court, and the Court's decisions. The dummy variable cannot tell us what happened, in what order, with what causes, or with what direct, indirect, or intervening consequences. By labeling the dummy variable "Court realignment," Norpoth and Segal arbitrarily designate membership change as the principal dynamic during this period and credit it for virtually all of the change in Court decisions before and after during this critical period. This, however, is entirely speculative; it is not based on any data regarding public opinion or Court composition. Had they chosen to call the dummy "public opinion change," then public opinion would be credited as the overriding determinant of changes in the Court's decisions. In the absence of other information, both interpretations/speculations are equally justified. Indeed, given the evidence that the conservative tide in the Court's decisions began before the conservative shift in the Court's composition but after the rightward change in public opinion had begun (Mishler and Sheehan 1993b, figs. 1-2), the logic of causal inference would seem to place the burden of proof on those who contend that the latter-occurring change in the Court's composition was the cause of the earlieroccurring conservative trend in Court decisions.

More importantly, there is no need to engage in such ad hoc speculation about the meaning of a dummy variable. In this case, we have direct measures of both public mood and Court composition with which to estimate empirically their relative effects on Court decisions. Indeed, one of our main purposes was precisely to avoid such arbitrary judgments and to assess empirically the strengths of different pathways by which public opinion might influence the Court. A dummy variable cannot do this. We believe we have demonstrated that when measured directly, both public opinion and Court composition¹¹ can be shown to have significant and

independent effects on Court decisions. Although the effect of Court composition probably has somewhat stronger direct effects (a point we always have acknowledged), public opinion has significant independent effects as well.

Why, then, do Norpoth and Segal find, when they replicate our model without the dummy variable, that Court composition is significant but public mood is not (Table 4)? The answer is that they do not replicate our model. Our model includes a multiplicative term for the effects of public opinion during the Reagan years, whereas theirs does not. Because public opinion has little or no effect on Court decisions during the Reagan years, the failure to distinguish the two periods results in an underestimation of the effects of public opinion during the pre-1980 period. The only difference in our two models is that they treat the impact of public opinion as constant across the entire period, whereas we allow the possibility that public opinion has a different impact during the Reagan years.

But why should this be the case? Why should the Reagan years be different? And if they are, why was this not also the case for the entire Burger-Rehnquist period? To elaborate, briefly, an argument offered in our conclusion, we believe the answer lies in the way that the Court aggregates individual preferences into collective decisions. The Court's majority-rule criteria means that the public opinion can influence majority decisions only if the impact of public opinion on individual justices is widespread or if the ideological distribution of opinions among justices is roughly balanced. Since we assume that judicial attitudes are relatively resistant to change, it is unlikely that public opinion will have widespread individual effects. Thus collective effects are likely to depend on the existence of ideological balance. As long as the Court is reasonably balanced, the impact of public opinion on even a single justice may be sufficient to alter the Court's decisions at the margins. But when the ideology of the Court becomes such that a change in the opinion of one or two justices is insufficient to alter majority outcomes, then public opinion is likely to have little direct influence on the Court's decisions (at least, so we continue to speculate. ¹² In effect, President Reagan's appointments of Justices O'Connor, Scalia, and Kennedy turned a moderately conservative Court into a highly conservative one, thus insulating the Court from direct public opinion effects.

Finally, implicit in Norpoth and Segal's critique is an assumption that the direct-effects hypothesis contradicts the attitudinal model of judicial decision making defended most recently by Segal and Spaeth (1993). According to this model, judges make decisions "by considering the facts of the case in light of their ideological attitudes and values" (p. 72). We believe, in contrast, that the direct-effects hypothesis is fully consistent with an attitudinal model. Except in the rare cases where a justice votes contrary to personal beliefs to preserve the authority of the Court in the face of public opposition, our view is that the

impact of public opinion is likely to be mediated entirely through the attitudes and ideologies of the justices. This is obvious in situations where the public elects a new president who selects like-minded justices to fill judicial vacancies, thereby altering the ideological balance of the Court. Although less obvious, this also is the case for the political socialization process where public opinion alters the Court's ideology by causing one or more incumbent justices to change their individual attitudes. Rather than viewing aggregate ideological change as a consequence of attitudinal *replacement*, the political socialization hypothesis attributes aggregate ideological change to attitudinal *conversion*.

Our analysis, then, does not argue against the attitudinal model of judicial decision making but in favor of a more subtle version in which individual attitudes are treated not as static and immutable but as fluid and dynamic. We do not doubt that judicial attitudes are highly viscous, especially compared to the political attitudes of individual citizens. What we question is the summary dismissal of the possibility of change. Thus, while the impact of public opinion on Court decisions is inconsistent with a naive attitudinal model that assumes the impossibility of individual change, it is fully consistent with a more sophisticated version that views individual change as one of the important dynamics underlying aggregate changes in Supreme Court decisions.

SUMMARY

It should be obvious that our understanding of the relationship between public opinion and Supreme Court decisions remains rudimentary. There is much to learn. Norpoth and Segal are correct in identifying individual-level effects as one area where additional work is needed. If, as we suspect, public opinion has widely varied effects on different individuals, then it would be interesting to know which justices have responded most (or at all) to these effects. Are moderates more responsive to opinion change than ideologues? Are younger or newer justices more responsive than older ones? Do liberal justices respond more to liberal mood shifts and vice versa? Under what conditions are individual effects most likely? Aggregate-level questions abound as well: Are we correct in our speculation that the ideological balance of the Court is critical for public opinion effects? What other conditions may constrain or enhance this relationship? Are the effects of public opinion symmetrical or asymmetrical? Is there a critical threshold of opinion intensity or duration necessary for public opinion to have an impact? Does responsiveness to public opinion vary by issue domain? And (of course) is the responsiveness of the Court during the Warren and early Burger years typical or an aberration? Much work remains. We appreciate Norpoth and Segal's attention to our work. Although we believe their

criticisms mostly miss the mark, we look forward to continued fruitful exchanges.

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Notes

Norpoth and Segal thank William Mishler and Reginald Sheehan for generously providing us with their data. Mishler and Sheehan wish to thank Harold Clarke for providing technical advice regarding a variety of methodological issues.

1. One might also quote Felix Frankfurter, who apparently was of two minds on this subject: "The Court has no reason for existence if it merely reflects the pressures of the day" dissenting; (West Virginia Board of Education v. Barnette (1943, 665).

2. It is also possible that the justices knowingly react to public opinion but feel that they ought not to and thus do not advertise it in their opinions. This is an explanation that neither rational choice models nor role theory models can account for.

3. Cook (1977) shows a high correlation between public opinion about the Vietnam War and sentencing in draft cases, but the claim of influence is effectively negated by Kritzer (1979).

4. See Mishler and Sheehan 1993b, 15. The situation would not be as severe if there were a priori predictions about particular lags. For instance, in quarterly data, lags at multiples of four could be expected, and in election results, lags of two, four, or eight years might be expected.

5. Data for the justices were generated from the U.S. Supreme Court Judicial Database, using citation plus split votes as the unit of analysis.

6. These correlations were not prewhitened, since the decision of the individual justices, except for Justice White, displayed no significant autocorrelations.

7. We further note that the Segal and Cover data, designed to predict civil liberties decisions, may not be a good measure of the values of the justices in economic and union cases, which make up a substantial proportion of the Mishler–Sheehan data base. As demonstrated by various scholars, economic decisions and civil liberties decisions are distinct dimensions (e.g., Rohde and Spaeth 1976; Schubert 1965). Indeed, the r-squareds between Court ideology, (as compiled using the Segal–Cover scores) and the Court's annual economic/union decisions are only .03 and .02, respectively. Thus the Segal and Cover scores may not sufficiently control for ideological change on the Court for the type of data used by Mishler and Sheehan, particularly at a one-year lag.

8. We cited Justices Rehnquist and Frankfurter to the effect that public opinion can and does influence the Court. No doubt, Norpoth and Segal could find additional skeptical quotations, just as we could find additional supportive ones. We will refrain, however, since we have little confidence in the quotation/counterquotation method of social-scientific inquiry. Ironically, on the day we received Norpoth and Segal's critique, a colleague, commenting on the article, claimed that he was not surprised by our conclusions, since all we had really done was to demonstrate statistically what everyone already knew!

9. The simple correlation between the unfiltered and unlagged measures of public mood and Court decisions is .52. The correlation increases steadily for lagged values of public mood reaching a maximum correlation of .68 at a lag of five years. The correlation decreases for lags in public mood greater than five years.

10. Almon distributed-lag models assume that the coeffi-

cients for successive lags of the independent variable can be approximated by a suitable degree polynomial. The approach uses ordinary least squares or maximum likelihood estimates procedures to estimate the coefficients for the polynomial, using variables constructed from lags of the original independent variables and then calculates the specified lags of the independent variables from the weighted polynomial coefficients. Unfortunately, both the order or the polynomial and the length of the lag need to be specified apriori. The hypothesis that public opinion has a gradual permanent effect on Court decisions suggests either a first-order (i.e., linear increase in effects) or second-order (asymptotic increase in effects) polynomial. We estimated both and found that whereas the first-order polynomial had significant coefficients and a high R-squared, the coefficient for the second-order polynomial was nonsignificant. For a good discussion of distributed-lag models, see Gujarati (1988, 534-41).

11. Norpoth and Segal also question the one-year lag for the Court compositions variable. Two considerations underlie this lag. First, because most new justices during this period joined the Court in midterm and participated, on average, in fewer than a third of the cases, it seemed logical to assume that the full impact of most new appointments would not be realized until their second year (i.e., at a lag of 1). Second, the prewhitened cross correlation between Court composition and decisions was more than half again as large at a lag of 1 as at a lag of 0 (r = .44 versus .28; see Mishler and Sheehan 1993b, tbl. 1). Indeed, the unlagged correlation is not statistically significant. Thus a one-year lag makes sense on both theoretical and statistical grounds. In practice, however, substituting an unlagged ideology term for the one-year lag used in our original models has no appreciable effect.

12. Dickey-Fuller tests indicate that the hypothesis of nonstationarity cannot be rejected for any of the three series. The

Dickey-Fuller results were as follows:

Time Series	Parameter	t-ratio
Court decisions	21	-1.87
Public mood	15	-1.53
Court composition	03	57

The critical t-ratio (.01 level) for N = 25 is -3.75. A ratio less than (i.e., a larger negative than) this threshold is necessary to reject the null hypothesis of nonstationarity. Thus in all of these series the hypothesis of nonstationarity must be accepted. Augmented Dickey-Fuller tests for lags up to five years produce identical results for all three series.

13. The cointegration regression of Court decisions on public mood, and Court composition, produced residuals with a Durbin-Watson d of 1.74. The coefficient for the first-order lagged residuals is -.87 with a Dickey-Fuller t of -4.74 and an augmented Dickey-Fuller t (for five lags) of -3.87. Given critical values (.05 level) for these three statistics of .78, -3.67, and -4.15, respectively, the null hypothesis on nonstationarity must be rejected. Similar results are obtained when either public mood or Court composition is regressed on Court decisions independently or when both are included in a cointegrating equation with controls for public mood during the Reagan years.

14. The three steps involve (1) estimation of a standard regression of the cointegrated series: $Y_t = \alpha X_t + Z_t$, where Z is the ordinary least squares residual; (2) estimation of a second-step dynamic equation using the residuals from the first stage to impose a long-run constraint: $\Delta Y_t = \Phi(L)\Delta Y_{t-1} +$ $\Omega(L)\Delta X_T + \delta Z_{t-1} + u_t$; and (3) estimation of a third-step equation consisting of a further regression of the independent variables from the first-stage equation multiplied by minus the error correction parameter from equation 1, regressed against the residuals of the second-step equation: $u_t = \varepsilon(-\delta X_t)$ $+ v_t$. The third step provides a correction to the parameter estimates of the first-step, static equation which makes them asymptotically equivalent to full information maximum likelihood and provides a set of standard errors that permits the calculation of standard t-tests. The correction for the first-step

estimates is then simply $\alpha^3 = \alpha^1 + \varepsilon$, and the correct standard errors for the adjusted step 1 estimates are those given by the step 3 regression. In effect, the second-step equation provides an estimate of short-term effects, the third-step equation measures long-term dynamics, and the sum of the two provides an estimate of total effects.

15. Although the corrected coefficient for public mood during the Reagan years remains negative, it is no longer statistically significant. In other words, the increasing liberalism of public opinion had little or no impact on the Court's decisions during the Reagan years. However, it does not have the perverse effect, implied in the original analysis, of causing the Court to become even more conservative. Of course, the number of observations post 1980 are so small that the interactive term for the Reagan years should be interpreted with caution. Its primary function is as a statistical control.

16. For models including public mood and the realignment

dummy, the coefficient for Court composition is .81 with a standard error of .83. In the model including only Court composition and the dummy realignment variable, the coefficient and standard error for Court composition are 1.01 and

.62, respectively.

17. Norpoth and Segal also question our use of the Segal and Cover (1989) measure of the ideology of Supreme Court justices. They note that this measure is based on the justices' prenomination positions only on civil liberty issues and may not reflect judicial attitudes on other issues (esp. economic issues). Unfortunately, the Segal-Cover measure is the only measure of Supreme Court ideologies, with which we are familiar, which is not based on the actual decisions of justices. Although relatively more abundant, measures of the latter type suffer the disadvantage of being circular: ideology is measured based on a justice's decisions and then used to predict that same justice's decisions. Although imperfect, the Segal-Cover measure is the best available and is superior to the dummy-variable alternative, which assumes that all of the change in the Court's decisions before and after 1970 are consequences of unmeasured ideological change. Importantly, the prewhitened correlations between the Court composition index and separate measures of the liberalism of (1) all Court decisions (i.e., the measure used in our analysis and by Norpoth and Spaeth), (2) Court decisions in the subset of criminal procedure cases, and (3) Court decisions in the subset of race-related civil rights cases are remarkably similar, especially given the small number of cases and the different detrending filters used in each case. The correlations for the unlagged Court composition measure are .28, .40, and .34, respectively. The correlations when Court composition is lagged one year are .44, .47, and .23, respectively. This provides a measure of reassurance that the ideology measure has broad applicability.

18. The logic of this argument implies that what is important about ideological balance is the distribution of justices on either side of an issue and not the relative intensity or distance from the cutting point of their ideological preferences. Thus the observation that the net liberalism of Supreme Court justices was larger in the early 1960s than their net conservatism in the 1980s is less important to this argument than the relatively more balanced sizes of the liberal, conservative, and moderate voting blocs in the 1960s com-

pared to the 1980s.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN DECEMBER, 1994

The following articles, research notes and controversies have been tentatively scheduled for publication in a forthcoming issue.

James E. Alt and Robert C. Lowry. "Divided Government and Budget Deficits: Evidence from the States."

Stephen Ansolabehere, Shanto Iyengar, Adam Simon, and Nicholas Valentino. "Does Attack Advertising Demobilize the Electorate?"

Mary G. Dietz. "'The Slow Boring of Hard Boards': Methodical Thinking and the Work of Politics."

Jorge I. Dominguez and James McCann. "Shaping Mexico's Electoral Arena: The Construction of Partisan Cleavages in the 1988 and 1991 National Elections."

James H. Lebovic. "Riding Waves or Making Waves? The Services and the U.S. Defense Budget, 1981–1993."

Steven C. Poe and C. Neal Tate. "Repression of Human Rights to Personal Integrity in the 1980s: A Global Analysis."

John T. Scott and Vickie B. Sullivan. "Parricide and

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Lonna Rae Atkeson and Randall W. Partin. "Economic and Referendum Voting: A Comparison of Gubernatorial and Senate Elections." A research note.

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James C. Clingermayer and B. Dan Wood. "Disentangling Patterns of State Debt Financing." A research note.

Kevin B. Grier, Michael C. Munger, and Brian E. Roberts. "The Determinants of Industry Political Activity, 1978–1986." A research note.

Robert Rohrschneider. "Report from the Laboratory: The Influence of Institutions on Political Elites." A research note.

George F. Bishop, Alfred J. Tuchfarber, and Andrew E. Smith; Paul R. Abramson and Charles W. Ostrom. "Question Form and Context Effects in the Measurement of Partisanship: An Experimental Test of the Artifact Hypothesis." A controversy.

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POLITICAL THEORY

Postmodernism and Democratic Theory. By Aryeh Botwinick. Philadelphia: Temple University Press, 1993. 288p. \$39.95 cloth.

Postmoderns are commonly criticized for what Jürgen Habermas has called a "performative contradiction": in order to attack modern reason they must also employ it. As Botwinick showed in his earlier Skepticism and Political Participation (Temple, 1990) tensions between the implicit presuppositions and the explicit formulations of philosophical arguments-or problems of self-referentiality—are hardly unique to postmodernism. However, postmodernism is his focus here. The central question of Postmodernism and Democratic Theory can be formulated as follows: How might a self-reflective postmodern read postmodernism today? Botwinick presents his response in two parts. The first addresses epistemological-metaphysical issues and the second turns to their politicaleconomic analogues. Throughout Botwinick stresses relationships between theory and practice, especially the historical convergence of postmodern philosophies and liberal-democratic traditions.

In Part One, Botwinick reformulates several criticisms of skepticism to suggest that thinking always begins "in the middle" and continually "unmasks itself" (p. 6). Perhaps most striking is his challenge to Habermas's distinction between tacit knowledge and communicative rationality, both of which are lifeworld phenomena. Botwinick argues that speech-act theory, including Habermas's, is best construed as a consistent pragmatism: sentences work simply because they work. Habermas, he claims, recognizes the cultural origins and limitations of his supposedly "universal pragmatics." He acknowledges them indirectly through his fascination with Benjamin's concept of "Jezt-Zeit" or "now-time." "Jezt-Zeit" refers to the convergence of past and future in the present moment. According to Botwinick, it is an "ultimacy which refuses to take its own ultimacy for granted," which accepts our presence in the world as a "protracted stationary moment (pp. 88-89)." So understood, Habermas's theory of communicative rationality is considerably closer to the ethos of postmodernism than his criticisms suggest.

Botwinick calls the epistemology which corresponds to our existential condition a "generalized agnosticism." It presupposes that "we are creatures living in time," whose insights, however profound, are ephemeral, and who simultaneously create and destroy meaning(s). This epistemological stance allows Botwinick to employ Levenson's distinction between tradition, as "an unself-conscious acceptance of a way of life," and traditionalism, as the "self-conscious affirmation of tradition in the face of competing alternative" (p. 2). Traditionalism, he argues, is quite compatible with postmodern critiques of modernity: "Under a generalized-agnostic dispensation we notice (or confer) value without ever leaving the plan of a continually expanding present" (p. 10).

In Part Two, Botwinick presents the political analogue to this epistemology. A "generalized-agnosticism" is enacted and institutionalized through the always unfinished character of democratic participation. Botwinick argues that what he names a "postmodernist liberal

communitarianism" corresponds to the existential condition and the economic circumstances of humanity today. Regarding the former, a dual emphasis on participatory and procedural aspects of democratic traditions allows the expression of deeply rooted identities within a framework of individual rights. With respect to the latter, Botwinick suggests that through democratic conversation, i.e., by "wagering" ourselves in community with others, modern individuals can learn to accept conditions of scarcity and to affirm existence "divorced from concerted exploitative interaction with the world" (p. 229).

The above overview only scratches the surface of a complicated and sophisticated argument. Botwinick supports his position with interpretations of numerous philosophers, e.g., Plato, Hobbes, Rousseau, Freud, Wittgenstein, Lyotard, Strauss, etc., besides those mentioned above. At times, his many shifts between authors and texts give the book a disjointed quality. I found Botwinick most effective when he analyzed metaphors to illustrate how philosophers can reveal our present limitations, e.g., his discussion of Hobbes's Leviathan to show how spatial constructions of representation create conflict under conditions of scarcity. With such analyses, Botwinick performs the intellectual's vocation which he, following Edward Shils, defines as the "sacralization of existence" through the "attaining of the greatest possible degree of coherence about oneself and the world" (p. 175). Still, I wish Botwinick had given less weight to theoretical arguments. In the introduction, he says, "History in the 1990s seems to be keeping pace with and embodying the latest insights of theory, so that what might appear as abstract philosophical argument in the pages that follow has its 'payoffs' in the current shape of political reality" (p. 2). Like Botwinick elsewhere, I doubt our ideas have such power to shape the events with which they are always already intertwined. For that reason, I hope he will develop empirical applications of his argument (perhaps along the lines of David Kolb's Postmodern Sophistications: Philosophy, Architecture and Tradition, [1990]), to tell us even more about our "continually expanding present." Where is the participatory democratic alternative to the "political spectacle" of representative government which Murray Edelman has so powerfully described (1989)? According to Botwinick, it is there we should seek our present salvation.

Pennsylvania State University

NANCY S. LOVE

Democratic Community: NOMOS XXXV. Edited by John W. Chapman and Ian Shapiro. New York: New York University Press, 1993. 400p. \$42.50.

This volume marks the end of John Chapman's distinguished editorial participation in the NOMOS series. It carries on the characteristic virtues of its predecessors: first-rate contributors representing a wide range of views, engaged debate, painstaking documentation of sources, and careful editing. The editors are to be congratulated, as well, for a detailed index and a brisk

but serviceable introduction. I missed only a brief consolidated bibliography, which would have facilitated classroom use.

Taken as a whole, the collection represents a state-ofthe-art meditation on relations, theoretical and practical, among a familiar triad of themes: communitarianism, liberalism, and democracy.

We are offered what is in part a continuation, in part the termination, of the liberal-communitarian debate. On the termination front, Alan Ryan argues (conclusively, I believe) that the debate between "atomistic" and "socially constituted" conceptions of the self does not map neatly onto the quarrel between liberal and illiberal conceptions of social relations. One can, without contradiction, combine a holistic view of the individual and a rights-based or liberty-based view of politics. (This point has been conceded by Charles Taylor.) Not surprisingly, Ryan goes on to sketch a "communitarian liberalism" in which our complex social interdependence is deployed on behalf of an individuality that goes beyond the fulfillment of social roles.

I am struck by how few of the symposiasts even tried to define community. Yet this is not wholly without reason: by 1955, a rural sociologist had already catalogued 94 different senses of the term and proposed a minimum condition (common locality), only to have his suggestion rejected by nearly all later scholars. Robert Post cites Michael Sandel: a community is a form of social organization that provides its members not just relationships they *choose* but attachments they *discover*, "not merely an attribute but a constituent of identity" (p. 165). Berry argues that this constitutive interpretation of communal identity poses risks to individual liberty. Jane Mansbridge defines community as a group "in which the individual members can trust other members more than they can trust strangers not to 'free ride' or 'defect' in social dilemmas, not to exploit the members of the group in other ways, and, on occasion, to further the perceived needs of the group rather than their own needs" (p. 340). Carol Gould suggests a "minimal sense" of community as agents who have a "common interest in shared ends, in pursuit of which their cooperation is voluntary and not merely constrained by law or habit, or effected by coercion," reserving judgment about the centrality of the more affective and self-sacrificing bonds Mansbridge stresses (pp. 398-99).

Underlying and animating this volume is worry about the authoritarian risks of communal life. Undemocratic communities typically succumb to these tendencies. But does a democratic organization of political life by itself suffice to protect individual freedom? (Carmen Sirianni poses just this question in his revealing account of the vicissitudes of participatory governance in the feminist

According to one line of thought, this question is poorly posed: the only freedom that counts is the freedom to participate fully and equally in the making of the laws under which all must live. The contributors to this volume vary in their stance toward this Rousseauian claim, but none seems entirely comfortable with it. The reason is familiar but not trivial. At least two other kinds of liberties seem to have independent moral weight: (1) equal protection in the ways laws are applied and enforced, through police powers, courts, and juries; (2) privacy in the broad sense—the right to be left alone,

the enjoyment of a zone insulated against public (and social) interference.

Amy Gutmann has long argued that properly understand as a core commitment to collective deliberation and individual autonomy, democracy entails the protection of many classic liberal freedoms. She continues that line of argument in her contribution to this volume. She insists that the greatest freedom is the freedom to "deliberate and decide political issues consistent with the like freedom of every other adult member of one's society"; but she acknowledges that the tension between liberalism and democracy does not entirely disappear: democractic deliberation may result in the subordination of a range of personal freedoms to other social goods (p. 152). She is not unduly troubled by this, because she regards the subordination of these personal freedoms as legitimate when produced by, or in the service of, public deliberation (p. 154).

Many staunch liberals will, of course, continue to take a different view. Gutmann's move is, in effect, challenged by contributors such Christopher Berry and Kenneth Minogue, who see a much more troubled relationship between the demands of community and the requirements of individual liberty as they understand it.

Bruce Rutherford's essay asks whether undemocratic groups can contribute to the process of democratization. He poses this question concerning Muslim fundamentalism in Egypt. But it raises a broader issue, which Rutherford traces back to Tocqueville: What stance should democratic nations adopt toward subcommunities whose internal norms and governance structures do not conform to democratic equality as currently understood? A newly dominant view in the United States is that these groups should be tightly regulated and compelled to democratize through the coercive power of the federal government. Rutherford tacitly gives us reasons to reconsider this policy.

University of Maryland

WILLIAM A. GALSTON

Burdens of Proof in Modern Discourse. By Richard H. Gaskins. New Haven: Yale University Press, 1993. 362p. \$35.00.

With the contemporary erosion of faith in scientific, religious, and political authority, our attention turns naturally to the conditions of public discourse. Are the forums within which putative experts seek the redemption of their claims (whether to truth or to moral rightness) sufficiently unconstrained? Do institutional judgments ultimately rest upon the pragmatic force of the best argument? Richard H. Gaskins argues that they do not but suggests that they could.

In presenting an argument-from-ignorance, a speaker contends that the inability of the opponent to refute the thesis constitutes sufficient grounds for accepting it. Rather than developing a positive case for the thesis, the speaker presumes its truth and shifts the burden of proof onto the opponent. Logic textbooks rightly treat the argument-from-ignorance as fallacious, but Gaskins contends that it nevertheless pervades modern dis-

Gaskins specifically treats dialectical discourse, defined as argumentation oriented toward decision. He locates dialectic midway between logic and rhetoric. Like rhetoric, dialectic does not possess the apodeictic certainty of logic. Nevertheless, like logic but unlike rhetoric, dialectic seeks both finality and legitimacy in its judgments. In order to secure finality and legitimacy, dialectical tribunals develop doctrines that distribute to one side the presumption (e.g., of truth); to the other side they allocate the burden of proof. The side with which the presumption rests enters the argument greatly advantaged, for it can rely on the argument-from-ignorance. Their opponents, on the other hand, bear the burden of proof and so must present a positive argument.

In the American legal system, the burden of proof rests invariably with prosecutions in criminal cases, while defendants enjoy the presumption of innocence. Civil courts distribute the burden of proof to plaintiffs and the presumption of nonliability to defendants. But in constitutional law, Gaskins argues, the Supreme Court exercises considerable latitude in distributing presumptions and burdens. Reviewing primarily cases involving due process and affirmative action, Gaskins identifies such assignments as the principal instrument with which recent Supreme Courts condition the outcomes of subsequent case law and thereby effectively establish public policy.

Burden-shifting strategies appear likewise in academic and other professional discourses. Gaskins shows burden shifting to be the primary discursive strategy of the Critical Legal Studies movement in particular and among deconstructionists more generally. The movement's representatives offer few reasoned justification for their heterodox views. Indeed, most of them deeply suspect that claims to reasoned justification are rhetorical camouflage for ideological preferences. In exposing these preferences, deconstructive critics shift burdens of proof onto prevailing orthodoxies that would otherwise claim presumptive truth for their positions. Whether critical legal scholars acquired this technique from their law school mentors (as Gaskins asserts) or the practice reflects a rhetorical tradition more deeply embedded in Western scholarship (as he argues later), Gaskins rightly locates the argument-from-ignorance at the heart of the deconstructive enterprise.

Examining the relationship between public regulatory policy and science, Gaskins questions whether the latter requires quasi-judicial tribunals to adjudicate scientific issues that impinge on public policy decisions. In the postpositivist period, of course, science will not issue final judgments. At least, scientific judgments cloaked in the claim to finality would, for most scientists, lack legitimacy. In the post-Popperian era, moreover, science now finds itself incapable even of pointing to a body of thus far irrefuted hypotheses. Where scientists divide on scientific issues with policy relevance (e.g., regarding ecological matters), advocates on either side of a policy debate can muster scientific support in order to defer to their opponents the burden of proof. Gaskins observes that by failing to institutionalize scientific tribunals, advocates of judicialized science lost an opportunity to impose finality upon factual disputes involving scientific questions. Yet he does not consider the obvious dangers tĥereby avoided.

Scientific authority might be salvaged (for scientists, if not for policymakers) by relocating scientific truth to a postulated transcendental plane beyond the reach of practical human activity. Ethical authority, after all, has risen by descending from just such a plane. But the liberal theorists whom Gaskins reviews postulate their own, idiosyncratic transcendental tribunal for ethical judgment. On the earthly plane of practical judgment (as in the decisions of bioethics panels), this diversity creates immense scope for strategically allocating presumptions and burdens.

Gaskins finds in Kant's Critique of Pure Reason the foundational source for burden shifting. In his famous antinomies, Kant deploys parallel arguments-from-ignorance to establish the strict separation of scientific and ethical discourse—of facts and values. Only now, two centuries later, have we begun to breach this attractive, yet fundamentally mistaken, conceptual divide. Hegel first recognized the error in his later work, and Gaskins devotes his final chapter to an exigesis of the framework for recovering reason embedded in Hegel's Science of Logic—a framework, Gaskins reveals, that guides his exposition from the beginning. While Gaskins's account of Hegel's dialectical logic is difficult, most readers will find it far more accessible than the original.

In the end, Gaskins' readers will want more. They will wish, for instance, that he had returned, armed with his dialectical method, to the practical controversies discussed earlier in the book. How, specifically, would the dialectical approach resolve or otherwise affect the controversies that rage currently in the law schools? How would it affect constitutional interpretations or ethical judgments regarding medical practices? Some readers will also wish Gaskins had considered a wider range of controversies. How would a dialectical understanding of scientific controversies affect the ad hoc tribunals that review manuscripts for publication, particularly in the theoretically pluralistic social sciences? Moreover, some might wish Gaskins had attended to argumentative discourses-like negotiations-that occur in the absence of arbitrating tribunals. Do presumptions and burdens shift with greater rapidity where no institutional mechanisms exist for enforcing them? Would a dialectical understanding of rational controversy among the parties help to produce outcomes that each would find just?

This wish list reflects more the provocative nature of Gaskins's contribution than any real deficiency. The exposition is elegantly crafted and difficult to put down. Its concerns address many of the specialities of our discipline. Students of constitutional law will find illuminating Gaskins's accounts of the judicial discourse surrounding affirmative action and due process. Political theorists will find utility in his comparisons of contemporary liberal theories. Students of bioethics and of public policy, particularly environmental policy, will find helpful his accounts of discursive strategies in their fields. Finally, the work will also prove useful to those political methodologists who recognize their responsibility to attend to the discursive conditions under which truth claims are argued and warranted.

Syracuse University

GAVAN DUFFY

Making Men Moral: Civil Liberties and Public Morality. By Robert P. George. New York: Oxford University Press, 1993. 241p. \$42.00.

Making Men Moral is a worthy contribution to perhaps the most constructive debate taking place in political theory today. George asks whether the liberal state can respect individual freedoms and act legitimately to preserve a moral climate in which citizens live, even if the latter requires legislation that criminalizes certain "victimless" actions. More broadly, George suggests that liberal freedoms themselves only make sense against the background of a theory of the human good and thus offers a perfectionist account of civil liberties.

George's moral concerns are shared by communitarian critics of neo-Kantian liberalism and, increasingly, by liberals, as well. The vision of the neutral state presented by John Rawls, Ronald Dworkin, Bruce Ackerman, and others has begun to strike more and more readers as neither a good empirical description of what the liberal state does nor a good ideal of what it should do. The state is not (nor should it be) indifferent to the ethical question that guided roughly two thousand years of preliberal thought: What is the good life for humankind, and how is it best promoted?

George seems to be moderately liberal in his political inclinations—a fellow traveler, at least, though he does not fit his own rather odd definition of the term (p. 167). Indeed, he bends over backward to assure his audience (whom he clearly presumes to be more liberal than he) that his theories do not lead to draconian restrictions of civil liberties that some may fear. Yet like Joseph Raz, William Galston, and others, he critiques major neo-Kantian liberals (including Rawls and Dworkin) for failing to offer an explicit account of the good but then slipping a suspect individualist notion of it into their theories of justice (p. 133). He also allows for a degree of moral paternalism on the part of the state that neo-Kantians would reject. It must be said, however, that George is too reluctant to point to anything beyond the most obvious examples of the types of moral laws that may be justified. Indeed, of his staple examples (prostitution, drug abuse, and pornography) two out of three are illegal in most jurisdictions, and the third is not afforded full free-speech protections even by those who would not accept his perfectionist premises in theory or practice. One suspects that there is more at stake in his arguments than he is willing to let on in this work.

Be that as it may, George's arguments are consistently intelligent and challenging. In his view, what ultimately grounds civil liberties to speech, press, privacy, worship, and so on is that they advance intrinsic human goods. Each of these liberties is an instrumental good, so that, for example, speech "that fails to advance any human good is valueless, for the value of speech is instrumental, not intrinsic" (p. 195). Such a view opens the door to morals legislation on the grounds that speech, association, and so on that are used for vicious, rather than virtuous, purposes are entitled to no legal protection. Such legislation, the author contends, cannot make men moral, because morality must come from within; but it can dissuade people from pursuing—maybe even considering—"the grosser forms of vice," which is a legitimate function of the state (p. 72).

George takes the side of Lord Devlin in his classic debate with H. L. A. Hart in arguing that there is no sphere of privacy necessarily protected from state power—or, as the author puts it, there is no intrinsic right to do wrong. However, George dissents from Devlin's view that public morality (and thus permissible morals legislation) is essentially a matter of public opinion. Rather, the rationally ascertainable truth of the proposition that a given practice is immoral is essential to the determination that the practice should indeed be outlawed.

How successful is George's perfectionist liberalism? I must register some doubts. George goes to great lengths to reassure liberals that though his political theory draws upon what he calls the central tradition of Western political thought from Aristotle through Aquinas, it escapes the potential threats to liberty found in these teachings. It does so largely be recognizing the great diversity of basic goods (and thus of morally worthy practices), which, in turn, assures that not much can be banned by the state after all. The great failure of the central tradition, George argues, was its blindness to this moral pluralism and thus its coming to decidedly antiliberal conclusions reflected both in theories of liberty and in social structure.

Yet George's "correction" of the central tradition's defects relies on diversity to do too much theoretical work. It is a misreading to suggest that Aristotle lacked "an appreciation of the diversity of human goods" and thus of good way of life open to people (p. 39). No reader of the Nicomachean Ethics could take this claim seriously. Aristotle believed, however, that such goods are internally related (so that the virtues are a package) and that these goods can be ranked. For example, one cannot simply say that friendship is among the basic human goods, so that one may choose the life of fealty to friends as one form of the best life. Friendship not informed, for instance, by a sense of justice can become quite vicious indeed. The problem here is that George cannot simply stop short a perfectionist rendering of the human good to better fit it to liberal sensibilities. A real coherence to Aristotle and Aquinas's thought leads to antiliberal conclusions that an admirer like George should not so lightly dismiss.

There are other ways in which George seems to compromise his argument to make it more palatable to liberal critics. He consistently relies on prudence to expand the scope of liberties, forgetting that it is in the nature of prudence to cut in more than one direction. In one revealing passage, he suggests that theory aside, it might be unwise to relinquish too much morals enforcement power to the modern legislator, given "what it takes to achieve public office" today and the flawed people these conditions place in power (p. 40). For a heartier perfectionist, such a denunciation might lead to a critique of our way of choosing leaders, perhaps of democracy itself. For George, it is a fact to which his theory should be accommodated.

George's most fundamental (and most heartfelt) reasons of principle push him to restrict civil liberties to advance the good, while prudence (and one weak argument on diversity) pulls him back. This is not enough for many liberals to jump on his bandwagon. Yet this closely reasoned book is more than enough to lead them to think about why they value liberty as much as they do, and this is all to the good.

University of California, Davis RICHARD C. SINOPOLI

The Descent of Political Theory: The Genealogy of an American Vocation. By John G. Gunnell. Chicago: University of Chicago Press, 1993. 330p. \$47.00 cloth, \$17.95 paper.

Gunnell tells a story, beginning in the mid-nineteenth century, of the emergence of political theory as an academic field of study. The account he offers is an

"internal" one, focusing on the writings of notable political scientists and theorists, examining how they characterized their enterprise, criticized each other, and appropriated the work of their predecessors and contemporaries. According to Gunnell, the central preoccupation of political science has always been its relationship to political practice. Political scientists have consistently sought to provide the kind of knowledge necessary to sustain, or perfect, democracy, though their understandings of both democracy and the knowledge it requires have been contested.

Until the 1940s, there was no clear definition of political theory as distinct from political science. At various times, it even disappeared as a classification of fields in the publications and programs of the American Political Science Association, and there was little consensus about its content, methods, or role in the discipline. There were, of course, debates in political science that foreshadowed the "behavioral reformation" (as Gunnell calls it) of the 1950s, most notably Merriam's call for a more "scientific" study of political life, one that would generate reliable knowledge that would give political science the necessary authority to address policymakers. But Merriam himself was trained as a theorist and continued to teach the history of political ideas throughout his career. His vision did not require the repudiation of political theory nor even a clear understanding of what it involved.

The critical event, in Gunnell's view, was the arrival of German scholars during the 1930s. Gunnell's discussion of this period—the range of intellectual positions they held, how their concerns articulated with those of American political science, and the reception of their ideas—is among the most original and interesting parts of this work. The German émigrés, notably Strauss and Voeglin, did not share the commitment of American political science to liberal democracy. Indeed, they viewed liberalism (particularly in the pragmatist version that was popular at the time) as part of the problem and launched a powerful attack on the whole idea of political science as it had historically been understood. Their attack was joined by many American theorists, such as John Hallowell, who had also studied in Europe. This attack, Gunnell argues, precipitated the so-called behavioral revolution. But this was not so much a revolution as a reaffirmation of the historical commitment to the role of political science as providing the knowledge necessary to the realization of democracy. Far from a radical break with traditional political science, the behavioral movement thus represented a conservative response to the European challenge, albeit one that one-sidely emphasized the "scientific" understanding of the enterprise.

Although Gunnell's story is fascinating and insightful, it relies too heavily on the role of Easton's work in launching the behavioral movement and on the conflict at Chicago between Strauss and his more scientifically inclined colleagues. Considering the virtual irrelevance (not to say unintelligibility) of the Straussian critique to most political scientists outside of the field of political theory, it is hard to believe that Easton's appeal could be explained in terms of the challenge of the German émigrés and their American followers.

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The challenge of Strauss also figures prominently in the final descent of political theory, its marginalization within political science, and its de facto transformation into an interdisciplinary field including practitioners from various academic disciplines. Noting that the principal response within political science to Strauss came from Wolin and his colleagues at Berkeley, Gunnell argues that the real significance of the émigré challenge was to initiate a contest for the soul of political theory. In the end, the Straussians lost, but political theory emerged alienated both "from politics and from itself," becoming a purely academic discipline, without any connection to political practice (p. 274). Political theory lost even its distinct role of providing a "reflective and critical voice" for the discipline of political science and had become hostage to the discourses of philosophy or the history of ideas (p. 269).

Gunnell had advanced this characterization of political theory in a series of publications over the past decade, but it is not made more plausible in this work than it was before. It is certainly true that political theory has become much more autonomous and more closely linked to other fields, but that development characterizes many fields within the humanities and the human sciences. There is both more specialization and more boundary crossing of conventional department lines; and both developments are, at least arguably, desirable.

One of the main weaknesses of the overall argument of the book is that the concept of the relationship between theory and practice—between the study of politics or political theory and political life—is not analyzed very closely. Gunnell offers one model of that relationship in the person of Francis Lieber, who associated with the "public figures of his time" (p. 25), but it is not clear that he did so to much effect. In any event, universities have become an important source of personnel for political office, particularly in the executive branch of the national government.

A second model is that political science might be an important source of ideas for public policy. There is an obvious tension between democracy and expertise; but if we do not suppose that the academy is to speak with a single voice, imposing its authoritative knowledge on the policy, the university can contribute to democratic life by generating ideas and subjecting them to critical scrutiny. Gunnell complains that political theory fails to speak to public issues and that its language is too esoteric to be accessible to a broader public. No doubt, that is true of much academic political theory, but his discussion (confined largely to the story of the "failed journal," democracy) is too limited to support that claim.

These weaknesses result, in part, from the fact that Gunnell's history is an "internal" one, examining the conversations of political scientists and theorists among themselves. This is a valuable approach, and makes an important contribution to our understanding of the development of the discipline. But it is not designed to capture either the impact of external events on the discipline or how the work of its practitioners may have been appropriated by others, including citizens and policymakers.

Wesleyan University

J. DONALD MOON

A Theory of Liberty: The Constitution and Minorities. By H. N. Hirsch. New York: Routledge, 1992. 276p. \$49.95 cloth, \$15.95 paper.

H. N. Hirsch has written an engaging book presenting a sustained argument for the *liberty theorem*, which holds

that the Court should strike down laws that unduly restrict liberty even if the passage of those laws has met the requirements of procedural due process. With lively writing, gripping examples, discussion of cases not often discussed in general treatments of the Supreme Court (most notably civil commitment cases), and a concluding chapter that attempts to relate constitutional law to the contemporary liberal—communitarian debate in political theory, Hirsch provides a welcome addition to liberal jurisprudential thought. Hirsch does not shy away from taking a stand. He is mostly supportive of Justice Brennan's decisions and bitingly critical of Justice Rehnquist, who at one point he accuses of applying an "extraordinarily tortured logic" that results in an opinion filled with "nonsense" (pp. 175, 177). Hirsch does not rely on such rhetoric, however; he offers reasoned arguments that are often quite convincing.

That said, I want to point to some difficulties I find with Hirsch's liberty theorem. The liberty theorem holds that "liberty can be restricted only for agreed, limited purposes" (pp. 40-41, cf. 131). Where a law unduly restricts liberty, the Court may strike it down. One of the textual grounds for this argument is the Due Process Clause of the Fourteenth Amendment: "Nor shall any State deprive any person of life, liberty, or property, without due process of law." Most laws deprive us of liberty in some way: the phrase "without due process of law" is therefore a crucial qualifier. Hirsch's liberty theorem demands substantive due process: according to it, the mere passage through fair democratic procedures of a liberty-restricting law that is neither vague nor unfairly applied is not sufficient to satisfy the demands of the due process clause.

What, then, are the substantive requirements for a liberty-restricting law if it is to be constitutional on Hirsch's view? What does due process require? The text of the Constitution is unclear, and we lack "clear 'proof' of specific framers' intent" (at least of the due process clause), so Hirsch turns to "the general ideological background of the constitutional period" and of the Republican party in the decades leading up to the passage of the Fourteenth amendment (pp. 31, 55, 60, 66). Hirsch shows that Montesquieu, Locke, and Blackstone all emphasize the importance of freedom from "arbitrary government action" (chap. 2). Blackstone, for example, opposes restrictions on liberty that are "wanton and causeless" (p. 40). The liberty theorem, then, requires that a law abridging liberty not be arbitrary, or "wanton and causeless." Regulating behavior with such a law denies due process of law no matter how scrupulously fair were the procedures in passing and enforcing the law.

Blackstone's criterion that laws not be wanton is easily met by most laws, as is another of his criteria, that laws restricting liberty must not be "without any good end in view" (p. 40). The laws curtailing liberty at issue in Lochner, Roe, and virtually all of the other cases that Hirsch discusses were not pointless: legislators had (nonarbitrary) reasons for passing these laws. We need some further criteria for deciding what counts as an "arbitrary" law. Locke and Montesquieu do not help us much with this question. Hirsch's point in turning to them as sources of authority in constitutional adjudication is unclear to me, especially since Hirsch ends up turning (in effect, though not explicitly) to John Stuart Mill instead. Hirsch's project can be described as in part

the constitutionalization of Mill's harm principle (which holds that my liberty may be legitimately restricted by the state only when necessary to prevent me from harming others).

As the argument proceeds, the requirement that a liberty-curtailing law not be arbitrary becomes more stringent: to satisfy the (substantive) demands of due process, such a law, it turns out, needs "extraordinary justification" that "can only come in the form of (a) reliable empirical evidence of concrete harm, or (b) a unanimous or nearly unanimous moral sentiment" (p. 100). The freedom to engage in homosexual sodomy or to enjoy sexually explicit books and films should, on Hirsch's account, be constitutionally protected against restrictive laws because empirical evidence shows that the expression of these freedoms does not cause harm, and there is no nearly unanimous moral consensus for such laws (see pp. 99–100, 103 [n. 88], 146).

It is hard to see why originalists or strict textualists should be persuaded by this position. We might expect Dworkin, who is neither, to argue that if we think these principles of due process derive from the best political philosophy, we should use them to interpret an unclear constitutional text. But Hirsch, in chapter 1, criticizes Dworkin, in part because he finds Dworkin so ahistorical. Yet it is hard to see how Hirsch's constitutionalization of the harm principle is itself historically grounded.

Even as a moral argument, the liberty theorem is troubling. Liberties that do not cause harm to others are not obviously all equal: some seem more basic than others, and their constraint seems to demand more justification. The liberty of speech and the right to vote seem more worthy of respect than the liberty to use narcotics; and some forms of the liberty of sexual expression (homosexual sodomy) may be worthy of respect than others (viewing hard-core pornographic films). Such distinctions are controversial but should at least be considered in a legal theory about liberty. Even if we accept as worthy of constitutional protection all nonharm-producing liberties, we still need to decide what counts as harming others. There is a growing body of thought on this problem (most notably the work of Joel Feinberg) that Hirsch does not address. Hirsch's other criterion for upholding a liberty-restricting law is that the law reflect a nearly unanimous moral judgment. Hirsch implies that the Court should uphold a law like Georgia's that prohibits homosexual sodomy if "the sentiment against homosexuality were as strong as the sentiment against murder," which, he says, is not the case (p. 146). But if the due process clause or other liberty-protecting clauses provide substantive protection of liberties or rights against majoritarian impulses, why-morally or constitutionally-should the level of consensus on restrictive laws determine the level of protection these liberties or rights receive?

Hirsch's book is to be commended first as an engaging introduction to important constitutional issues and second, for providing a sustained argument that liberty is the guiding principle of the Constitution. Hirsch has laid the groundwork for further consideration of whether Mill's harm principle should be given constitutional effect.

Stanford University

Mark Tunick

Postmodern Contentions: Epochs, Politics, Space. Edited by John Paul Jones III, Wolfgang Natter, and Theodore R. Schatzki, New York: Guilford, 1993. 210p. \$18.95 paper.

Politics, Theory, and Contemporary Culture. Edited by Mark Poster. New York: Columbia University Press, 1993. 300p. \$55.00 cloth, \$17.50 paper.

Perhaps it is an exaggeration to say that the postmodern epoch came into its own when the Berlin Wall was torn down. But certainly, the event symbolized an emerging consciousness of a future opened to new possibilities but also a future that, perhaps necessarily, cannot be mapped, least of all by a triumphant liberal-democratic ideology that has come to mean everything to everyone. We are caught in a moment in which ideas—no longer frozen by the Cold War—have a window of opportunity. And yet events are overtaking ideas at a pace that threatens to leave us only with retrospective reflections.

The essays collected by Mark Poster reflect this sensibility. They are "postmodern" in their view that received metanarratives (e.g., liberal, Marxist, neoconservative), no longer tell us what to prefer and how to choose. These essays are less concerned to show that the various disciplines that provide initial focus (literary criticism, political theory, feminism, urban studies, and psychology) are "political" than with rethinking what "the political" has become. The best essays do precisely this. In perhaps the most original and theoretically ambitious essay of the volume, Ernesto Laclau examines whether the "exhaustion of the great narratives of modernity" underwrites a retreat from politics by robbing politics of its meaning. This view, he notes, assumes that the meaning of political activity reduces to its representation of a grand narrative. In fact, there is retreat not from politics but (more specifically) from revolutionary designs unwritten by grand modernist narratives. Politics is, as it were, relocating itself within the "ontology of the social"; that is, politics is increasingly local, contingent, and part of the fabric of society. It is often radical but no longer revolutionary. So we need to reconceive politics generically, as "the ensemble of the decisions taken in an undecidable terrain" within social relations generally, which Laclau conceives as sedimented power relations that have blurred their own contingency. In such a context, Enlightenment ideals do not lose their meaning but, rather, become "pragmatic social constructions" that reorient those social relationships that have become "political," that is, visibly held together only by power (p. 295). The relationship between politics and meaning can emerge locally and contingently, just insofar as political settlements allow "positive communitarian" goods to develop within a terrain that had been undecidable (p. 296).

In much the same vein, Jean-Francois Lyotard, reflecting on the Gulf War, suggests that struggles for emancipation must now take on the character of immanent critique, rather than wholesale opposition. What we ought to favor is the "openness (or looseness)" of systems, which will tend to survive those that claim a "closed" spiritual center such as Iraq. An open system "no longer needs to be legitimated on a metaphysical basis. Rather, it only needs the free space" within which critique and argument can thrive (p. 270). One wonders whether Lyotard (had this essay been written after Desert Shield gave way to Desert Storm) might have

seen residues of metaphysics in "the system's" sometimes genocidal conduct.

Other essays are less synoptic. An intriguing essay by Samuel Weber finds a postmodern ethos in Lacan's "Ethics of Psychoanalysis." Whereas traditional ethics seeks external containments of desire, Lacan finds an ethics in desire itself. Roughly, only limits to desire can constitute an other that can be desired. But "desire for the other" involves breaching these limits, meaning that desire is inherently tragic in its mode of satisfaction. There is an ethos inherent in this dilemma, namely a recognition of the other as irreducibly different, something that cannot be subsumed. In contrast, traditional ethics have a "constitutive incapacity to respect limitation and, in particular, self-limitation" (p. 140). Such ethics justify an excess of external power against desire that defeats the more subtle if tragic dialectic of desire and, with it, a self-limiting subject who is open to difference. Two other competent if more limited essays probe psychological issues: Grabriele Schwab criticizes Fredric Jameson's Political Unconscious for reducing the sexual elements of desire to economic desires, thus missing a terrain that is central to feminism. And Teresa de Lauretis reads Freud's Three Essays on the Theory of Sexuality to show that even in Freud's terms, heterosexuality is but one possible organization of the drives. "Normal" sexuality is normative and not natural (p. 126).

In a subtle and attentive essay, Catharine Stimson asks about the political implications of feminism's increasing reflexivity and maturity, noting that in shifting away from grand narratives—a shift she does not regret-feminism has yet to find a substitute means of galvanizing a feminist movement. An innovative if not entirely successful essay by John Carlos Rowe reinterprets Marx's concept of class by focusing on the increasing importance of symbolic values in an era of information intensity; high technology; paper debt, credit, and wealth; and symbolically produced and manipulated needs. Class, Rowe argues, is now less determined by industrial location than by location within symbolic structures. Political agency now requires new intellectual skills of symbolic manipulation. In an essay entitled "Community after Devastation," David Carroll asks whether community is possible after the Holocaust, in which the rhetoric of community covered for massive atrocities. In surveying several attempts to rethink community as a space of public criticism, Carroll rejects the language-based approaches of Habermas and Arendt (arguing, incorrectly, that these approaches are based on a metaphysics of consensus) in favor of Jean-Luc Nancy's view that literary criticism provides the kind of reflexive horizon that works against closure. This essay reflects a failure-not uncommon among those whose academic home is literary criticism—to distinguish questions about self-making and consciousness raising from recommendations for political institutions, notwithstanding obvious interrelations. It is possible to institutionalize conversations if conditions favor talking over fighting. But what can it mean to institutionalize politically the consciousness of a work of art? The medium is inappropriate. Finally, in a typically masterful performance, Jacques Derrida reflects on a series of travel narratives on Moscow (his own included) in order to show how they operate as projections of utopian hopes

The essays collected by Jones, Natter, and Schatzki are good (if not pathbreaking) essays. They are careful,

disciplinary reflections. They amount to taking stocksurveying the existing terrain of academic discourse in political theory, philosophy, literary studies, geography, and anthropology. If they add up to a single point (one intended by the editors), it is that we should resist the polarities of the modern/postmodern divide, just because the discourses are so interwoven that they do not fall out along these lines without leaving a remainder. In their introduction, the editors trace themes that are key to postmodern styles (the crisis of the subject and representation, the need for irony, the overfullness of life) as far back as Schlegel and Novalis, while pointing to the political events today that have revived such reflexive critique of Enlightenment. Although an interesting survey, the editors lapse into the intellectual historian's tracing of themes, a method that always turns up antecedents and is thus incapable of identifying originality. An essay by Fred Dallmayr is more precise: he views postmodernism as an internal critique of modernity, which results in "a radical relativism in which no part can claim absolute primacy or supremacy" and which for this reason coheres with a radically democratic ethos (p. 34). Dallmayr rejects Lyotard's definition of postmodernism as "the rejection of metaphysics," since reversals too easily repeat metaphysical formulas. Likewise, in an interview with the editors, Richard Bernstein argues that the modern, postmodern polarity obscures more than it reveals: Habermas, for example, is a thoroughgoing critic of the Enlightenment, while Derrida has certainly never argued against "reason." Quoting Albrecht Wellmer, Bernstein suggests that "postmodernism at its best might be seen as a self-critical-a skeptical, ironic, but nevertheless unrelenting-form of modernism" (p. 103). In a rich and suggestive essay, Edward Soja notes likewise that postmodernism is a living dynamic that can be identified only as reactions to successive modernist hegemonies, as far back as 1789. This does not detract from the uniqueness of contemporary postmodernism, occasioned by the emergence of a symbolic hyperreality. As a geographer, Soja is especially concerned to develop a critical theory of space, a project he locates by noting that human existence is defined simultaneously within spatial, historical, and social dimensions. With the important exceptions of Foucault and Henri Lefebvre, Soja suggests that the spatial dimension has been neglected within critical theory and postmodern writings. In an essay comparing postmodern writings in literary criticism and geography, Wolfgang Natter and John Paul Jones III argue that weaknesses of postmodern styles tend to mirror the disciplines: literary criticism has neglected space, while geography has neglected representation. In a disciplinary survey of anthropology, Catherine Lutz notes that feminist anthropologists (who have been developing postmodern approaches since the early 1970s) were marginalized and unacknowledged as postmodern styles were initiated into the anthropology establishment in the early 1980s. In a clever set of comparisons, Lutz indicates the extent to which "establishment" postmodernism feeds off of simple reversals of male/female stereotypes (p. 156).

Three essays address the politics of postmodernism directly. Soja draws useful distinctions among a postmodernism of the Right that feeds off of symbolic manipulation, a "fence-sitting" postmodernism that glorifies the "play of signifiers" without developing any particular commitments, and a postmodernism of the

Left that aligns itself with radical democracy. A lucid and sensible essay by Theodore Schatzki compares Foucault and Lyotard, suggesting that Foucalt's radically liberal commitments complement Lyotard's emphasis upon systematic oppression. An essay by Ernest Yanarella provides a careful comparison of Derridan and Gramscian politics, while recommending the kind of synthesis developed by Ernesto Laclau and Chantal Mouffe in *Hegemony and Socialist Strategy*.

Taken as a whole, these collections provide articulate thoughts on emerging political terrains, as well as useful guides to emerging academic reflections.

Georgetown University

MARK E. WARREN

The Morality of Pluralism. By John Kekes. Princeton: Princeton University Press. 1993. 227p. \$29.95.

This book is about not the moral significance and consequences of political, social, or cultural differentiation but, rather, a more abstract issue internal to much contemporary moral and political philosophy: How do values stand in relation to one another, and how are conflicts among them to be resolved?

Value pluralism, according to Kekes, has several components-indeed, six theses: pluralists (1) "deny that there is an authoritative system of values and, consequently, that there is any value that is always overriding" (p. 19); (2) take value conflicts to be unavoidable, because values can be both incompatible and incommensurable (though incompatible values need not be incommensurable and vice versa); (3) believe in "reasonable conflict-resolution," holding that conflicts can, for the most part, be rendered tractable through "balancing" against the background of an accepted tradition; (4) take values to be "possibilities of life" that we may "reasonably" value, albeit many of them are incommensurable and incompatible; (5) recognize the "need for limits," defending some values against others on "context-independent grounds" (esp., "deep conventions" that "protect the minimum requirements of all good lives," p. 34); and (6) entertain the prospect of "moral progress" in relation both to individuals and to traditions. For individuals, "the ideal is to construct for ourselves a reasonable hierarchy of the possibilities we value" (p. 36). Moral progress occurs in a tradition that is "as receptive to a plurality of conceptions of a good life as is consistent with the limits needed to maintain that tradition" (p. 35).

Kekes's value pluralism is a descendant of the idea first launched by Sir Isaiah Berlin in his various writings, and then taken up by a variety of philosophers. But unlike Berlin, Kekes seeks to underplay the tragic dimension of pluralism and to deny the claim that there is any affinity (or even compatibility) between pluralism and liberalism. On the other hand, like Berlin, he tries to distinguish pluralism from both monism and relativism.

Monism he takes to be the claim that there is either a summum bonum (as Plato and Aquinas thought) or a medium in whose terms values can be ranked (as utilitarians and subjectivist theorists of values as preferences maintain) or a canonical principle for ranking values (which can take a stronger or weaker form, the latter coming close to pluralism). The key contention of pluralists against monists, to which Kekes accords overriding importance, is that no value or set of values can be overriding.

The other flank of Kekes's attack is relativism. What distinguishes relativists from pluralists, he says, is that "relativists conclude that the plurality of values excludes the possibility of objective justification and criticism of values, while pluralists believe otherwise" (p. 46). Relativism can, of course, be more or less radical, depending on whether they hold all values to be context-dependent or only some. Kekes's general way with relativists is to insist on "context-independent grounds" for what he calls "primary values" (benefits that are universally human and part of all conceptions of a good life) and "deep conventions."

Kekes's argument is a tightly woven fabric, though it is not highly colored by examples, either of the thickly described anthropological or historical sort or the thinly described philosophical sort. It also has many loose ends. There is no extended discussion of key notions essential to his argument, such as "reasonableness"; the distinction between primary and secondary values is insufficiently and even inconsistently specified (sometimes the latter are said to be interpretations of the former); and the idea of "balancing" is never properly explained (an odd omission, since it incorporates the metaphor of "scales," yet the values to be weighed are said to be incommensurable). Nor is the very notion of incommensurability adequately treated, though, in this respect, Kekes is at one with almost all contemporary writers who have appealed to this notoriously slippery notion.

Least satisfactory of all is the concluding discussion of the relations between pluralism and liberalism. Kekes argues that there are good reasons for holding that these are incompatible. (By liberalism he means the contemporary political-philosophical versions.) His central reason is that liberals are committed to "overriding values," whereas pluralists are not. But overridingness is another notion he both fails to explicate and uses inconsistently. Sometimes he seems to mean the according of priority to a value, irrespective of social context. Yet Rawls, for example, is very clear that his principles of justice only come into play beyond a certain level of social and economic development. Moreover, it is clear, at various points in his argument, that Kekes systematically gives priority to the value of autonomy (e.g., p. 215). Nor is it evident what the "politics of pluralism," as Kekes sketchily describes it, would actually look like. Nor, finally, is it clear how the state could "take an active role in advocating very many substantive values" (p. 215) if these are incompatible and incommensurable.

European University Institute

STEVEN LUKES

Reconsidering the Democratic Public. Edited by George E. Marcus and Russell L. Hanson. University Park: Pennsylvania State University Press, 1993. 478p. \$55.00 cloth, \$13.95 paper.

The chronic and usually debilitating tensions that exist in political science between "theorists" and "empiricists" have been fruitfully utilized in this book to produce a series of interesting and imaginative essays on problems of democracy. The book is an outgrowth of an extended discussion among the scholars held at the Democratic Theory Symposium at Williams College during the summer of 1989. To enhance reader interest and

understanding, each leading essay is followed by an extended critical analysis.

In the lead chapter, Benjamin I. Page and Robert Y. Shapiro agree with the commonplace view that the opinions of ordinary people on important issues are ill informed, irrational, and unstable. But they argue that collective public opinion (as distinguished from individual opinion) is real, rational, and stable. This is so because collective public opinion comprises the statistical aggregation of individual opinions. More important, the authors emphasize that the collective public opinion is influenced and shaped by intercommunication among governmental, social, and media elites and among elites and the public. The collective public opinion is declared rational and stable because it responds in a reasonable way to changing realities and ideas and this public opinion tends to form coherent patterns that are attached to American values.

Benjamin Barber is less than satisfied with the Page and Shapiro study because, among several reasons, they fail to investigate the key issue whether public opinion is an outgrowth of citizen discourse, judgment, and deliberation or (as the determinists would have it) shaped by external forces. Unwittingly, Barber argues, the authors tipped the scales in favor of determinism by using a language of opinion, private interest, elites, and individualism, rather than a democratic vocabulary that underscores the importance of citizen participation.

The cleavage between a normative and an empirical orientation to politics was more successfully breached in the Conover, Leonard, and Searing essay. To contribute to a clearer understanding of what constitutes a democratic conception of citizenship, the authors conducted an empirical study to determine whether liberal and/or communitarian concepts of rights and duties are influential in the way citizens think about their rights and responsibilities of citizenship. It is no surprise that most of the people tested had a liberal understanding of rights, an understanding that underscores the importance of civil rights, in contrast to social rights. Similarly, the common understanding of responsibilities of citizens is basically liberal in character; that is, citizen responsibilities are primarily limited to those necessary to preserve negative rights. Within this context, it is not difficult to understand why Americans tend to be indifferent, if not hostile, to the plight of the homeless. The authors point out that irrespective of whether people embrace liberal or communitarian attitudes, when it comes to "particular situations," they find themselves in agreement on what constitutes appropriate action. Because of the readiness of citizens to reconcile differences when it is deemed expedient to do so, the authors suspect that liberal-communitarian debate is not fruitful toward reaching an adequate conception of citizenship. In a sharp analysis of sexual violence and liberalism, Virginia Sapiro and Mary Lyndon Shankley call for a restructuring of both the theory and the practice of democracy as an essential means to achieve equality of citizenship between women and men.

It is encouraging that as many as four of the essayists were able to draw "cautiously optimistic" conclusions that segments of the American political system that they studied were in good health. Donald Kinder and Don Herzog bolstered and enriched, Page and Shapiro's claim that owing to the "miracle of aggregation," collective public opinion is indeed "extremely sensible [and] reasonable" (p. 371). In back-to-back essays John King-

don and Jane Mansbridge reaffirmed that ideas are not necessarily tied to self-interest and that they can be significant forces in shaping political outcomes. And Jennifer Hochschild reenforces the judgment of "cautious optimism" about the system by showing that disjunction and ambivalence, for a multiple of reasons, act as a democratic catalyst to the deliberation process.

Judged individually, the quality of the essays in this book range from good to outstanding. However, it is deficient in the one fundamental respect that it is incremental in its vision: it focuses exclusively on various parts of the system, not on the whole. Thus, ironically, it is unable to address (much less answer) the most crucial question, namely, How healthy is American democracy?

Temple University

PETER BACHRACH

Beyond Relativism: Science and Human Values. By Roger D. Masters. Hanover: University Press of New England, 1993. 248p. \$24.95 cloth.

Twelve years ago, Thomas C. Wiegele published a book on an emerging revolution in social sciences. He argued that new findings in biological sciences made such a revolution inevitable. Masters's new book demonstrates that this revolution is really taking place. He challenges some basic premises of modern science, including social sciences, and provides ideas and material for a new paradigm. His targets include, among others, Locke's tabula rasa model of the brain, the long held ideal of value-free science, scientific value relativism, cultural determinism in social sciences, the natureversus-nurture dichotomy, and the so-called naturalistic fallacy. His major thesis seems to be that the gulf between is and ought is not unbridgeable and that as ancients taught, knowledge of nature is at the foundation of human values and goals.

The book is divided into three parts. In the first part, Masters describes the nature of science and makes comparisons between modern science and ancient science. One very interesting insight is the assumption that the friendship between Leonardo da Vinci and Niccolò Machiavelli was a central event in the emergence of modernity and of modern scientific method. In the second part, he discusses the nature of facts and values and argues against relativism. He differentiates three models of knowledge-intuition, hypothesis testing (or empirical verification), and pattern matching-and rejects the relativistic or nihilistic assertion of postmodernism, according to which "humans are alone in an alien universe with no inherent principles of value or meaning" (p. 8). Masters thinks that it is possible to discover objective knowledge by either verification or pattern matching and that knowledge provides a factual basis informing our understanding of what is good for humans "according to nature." He connects the three models of knowledge to distinct processes in the brain: the limbic system, the left hemisphere, and the right hemisphere. In the third part, he tries to build a bridge from facts to values—from the naturalistic fallacy to naturalism—and to integrate nature and nurture into a new paradigm in the social sciences.

Masters's essay is based on very extensive interdisciplinary material and evidence. Notes covering 80 pages in the end of the book provide an eloquent demonstration of his enormous expertise, which ranges from ancient philosophers to modern science (esp. political science, political theory, biopolitics, evolutionary biology, behavioral ecology, and neuroscience). Bibliographical notes provide rich information of relevant literature.

The significance of this book is in its attempt to prove that some major premises of social sciences are obsolete and that we need a new paradigm. Let us look at one point in which the contemporary environmental paradigm of social sciences is clearly obsolete. His argument that Locke's tabula rasa model of the brain is wrong is based on convincing evidence. He claims that "the behaviorist model of the brain has been demolished by recent research" (p. 74). There are innate ideas in the brain. Phenotypic behavior is always a result of the interaction between genetic and environmental factors. Social sciences, however, have limited their attention to environmental factors.

One could say that Masters succeeds better in the demolition of the obsolete environmental paradigm of social sciences than in the formulation of a new paradigm based on the principles of naturalism, but he makes it clear that the new paradigm should reflect the integration of nature and nurture and the possibility to derive values from facts. On the basis of this book, it is not yet easy to see how values can be derived from knowledge of nature and human nature, although he gives some examples. He says, for example, that because of the natural inevitability of differences in perception and judgment, we should expect and respect individual differences (p. 123). Consequently, tolerance of others is a moral virtue. His conclusions on the moral virtues of abortion and infanticide are more provocative. He argues that because Darwinian principles lead to variation in the extent to which mammals care for their young, we cannot establish any absolute moral values concerning abortion and infanticide. They are adaptive strategies that vary depending on the environment. Therefore, we can only say that "abstinence is preferable to contraception, contraception is preferable to abortion, abortion is preferable to infanticide, infanticide is preferable to murder, and murder is preferable to war and plague" (p. 128).

Many readers will probably disagree with Masters in some major or minor points. Let us take some examples. He refers to a contradiction between competition and cooperation. However, one could argue that there is no contradiction between them. Cooperation is just another evolved mechanism of competition. Usually people cooperate to further their selfish interests. He does not pay much attention to assumed evolved predisposition to reciprocity. However, it is quite possible that many values are based on expectation of reciprocity. He discusses the genetic basis of individual variability in behavior convincingly, whereas he pays less attention to the existence of numerous behavioral universals and their relevance from the perspective of human values. Donald E. Brown's Human Universals (1991) might have been useful.

This is a very important theoretical book. It is true that Masters does not formulate a complete new paradigm, but he introduces a research project intended to produce a new paradigm for social sciences. We need theories and hypotheses that combine evolutionary and environmental factors. The integration of the theories and findings of new biological research into a new paradigm

of social sciences is an enormous task. The process of change has been slow for the reason that most social scientists are still ignorant of the relevant research outside their own branch. Masters's book helps to correct this lack of information. It is worthwhile reading for political scientists interested in the basic premises of our branch of science.

University of Helsinki

TATU VANHANEN

Profits, Priests, and Princes: Adam Smith's Emancipation of Economics from Politics and Religion. By Peter Minowitz. Stanford: Stanford University Press, 1993, 345p. \$45.00.

Any account of this book has to ignore the alliterative but largely decorative title and concentrate on the subtitle. Even so, the reader will find this overdramatic, because what Peter Minowitz actually wishes to maintain is that Adam Smith was engaged in a process of "deflecting," rather than emancipating economics from politics. What Smith was doing to religion is rather more complex: "disguised subversion" seems the best description. Nor is it always entirely clear whether or not Minowitz wishes to maintain that deflection and subversion were self-conscious aims on Smith's part. More often, it seems as though he wishes to claim that Smith began a process that was "consummated" in the work of his more positivistically minded successors as economists and social scientists, with Marx being given a final chapter as the consummate consummater.

Although this study of Smith begins and ends by situating itself against the background of the demise of communism, Marxism, and the "end of history" (with many other excursions into current affairs introduced along the way, especially those associated with contemporary environmental dilemmas), it is as a contribution to Smith scholarship that the work will mainly be judged here. Many of the book's theses could be sustained without reference to Smith, but a particular reading of Smith is the chosen vehicle on this occasion. In this choice (if not in how he conducts his detailed argument), Minowitz is working with the grain of an established stereotype. Economists may famously disagree about most things, but they are usually united on the paternity of their discipline; and it is not difficult to obtain their agreement on the related proposition that the act of emancipating economics from politics (let alone from religion) ought to be celebrated, rather than regretted. Minowitz does not collude with the one-eyed habit that sustains such professional hubris by confining himself to the Wealth of Nations. Rather, he deals with the problem of reconciling this work with the Theory of Moral Sentiments and comes close to reviving an influential account of the relationship advanced by Jacob Viner in 1926 and withdrawn later; namely, that the providentialism of the moral philosophy is at odds with the Godless realism of the political economy. Minowitz's version of the thesis that there truly is an "Adam Smith Problem" is more complex and pays more attention to religion. Thus, both works are held to contain neglected contradictions within and between themselves.

In other respects, however, Minowitz departs from the usual perspective adopted by historians of economics as a positive science. He believes that Smith "facilitated the *continued* process of narrowing of political

economy" (p. 3, my emphasis)—an observation that could just as well be replaced by the statement that in repossessing political economy, Smith broadened it by placing it within the context of a secularized version of natural jurisprudence and moral philosophy that emancipated it from the restrictive mechanical and political arithmetic cages within which it had previously been confined. Smith's enterprise, taken as a whole, was as generously proportioned as his physiocratic contemporaries and far more so than any of those predecessors he castigated as the "pretended doctors" of the mercantile system. It also entailed a break with "narrow" (Hobbesian or Mandevillian) conceptions of economic man. What has usually preoccupied historians of economics has been how Smith's successors (Jean-Baptiste Say, J. R. McCulloch, and Nassau Senior) formally announced the divorce of political economy from politics, criticizing the master for overinclusiveness. For Minowitz's metahistorical purposes, such details can presumably be over-

Announcements that Smith was responsible (usually single-handedly) for deflecting politics in an economistic direction are more common in histories of political philosophy written during the second half of this century. The works of Joseph Cropsey (Polity and Economy, 1957) and Sheldon Wolin (Politics and Vision, 1960) provide distinguished earlier examples; but a longer list of younger followers could be compiled, some (but by no means all) of whom appear in Minowitz' bibliography. Though critical of Cropsey's insights on matters of detail, Minowitz seems closest to him in deploying Straussian criteria for deciding in what respects Smith is responsible for replacing or undermining concepts that are essential to any truly political standpoint. There may also be a Straussian warrant for the underlying historiography of the work, judging what Smith was doing in terms of what he licensed later authors to do and even in employing a version of Strauss's "esoteric" approach to texts: on religious matters especially, Minowitz maintains that there is a subterranean agenda beneath Smith's writings that he may have been following covertly, even unconsciously.

Since Smith is being judged throughout more by what he was not trying to do (or was neglecting to do), it is essential to have some clear criteria by which this record of failure or avoidance can be judged. In this respect, Minowitz did specify the standards by which Smith can be deemed unpolitical or apolitical, almost a positivist avant la lettre. The notorious difficulty of establishing negative propositions (the infinite number of nonconclusive possibilities) is turned into an advantage. Here are some of the political and religious things that Minowitz finds absent by the chosen criteria. Smith "says nothing about promoting the peaks of courage and intelligence," nor does he "endorse a ruling class or extol a particular set of political institutions"; he "never presents a typology of regimes, much less an evaluation of the different forms of government" (pp. 19-21). Normative conceptions of justice and equality are absent. Biblical injunctions, the after-life, and Jesus Christ are missing, and morality is deprived of sanctity. Whereas Rousseau is "brimming with political philosophy," Smith's cup is almost empty.

When Plato, Aristotle, Socrates, Cicero and others are brought into the picture, the *nowheres* and *nevers* fly thicker and faster. Even the "joy of sex" is deprecated by Smith's "nonerotic prose" (with Nietzsche, rather than

Alex Comfort, providing the relevant standard here). Smith says "nothing to encourage war" and little to foster "national pride." Even Smith's placement of commas and use of footnotes is invested with negative significance. When "complexity" is recognized, it is said to approach "unintelligibility" (p. 79). Only the "bourgeois virtues" of economic man are left, with the result that Smith becomes Machiavelli's economic alter ego and Marx's flawed predecessor.

Alternatively, Smith is presented as an atheistic proto-Darwinian, concerned only with opulent survival and species propagation, the author of a vision of order without design that leaves only "paradoxical vestiges of traditional perspectives" (p. 133). What seems at stake here is no less than "mankind's hubristic drive for cosmic sovereignty" (p. 239). Smith is judged not to be politically correct on the environment. Priests, but not political economists, are prevented from exploiting workers, leading to the sweeping conclusion that "the study of Smith's texts is unlikely to vanquish the disorienting and demoralizing effects of modern capitalism" (p. 254).

With so much of Smith left on the floor after this compulsive chipping-away process, the result was perhaps predictable. Less heroically normative and more catholic conceptions of political speculation and debate would have yielded very different results. Serious consideration of Smith's Lectures on Jurisprudence might also have led to a different destination. Smith is too cool a customer for those who yearn for life in warmer climes. Yet if, as Minowitz is forced to conclude, we have no alternative but "to muddle through," Smith's understanding of what it is like to prescribe for a world that is always second best may be a valuable starting point. I have made little direct reference to all the modern problems laid at Smith's door, but one final reflection is provoked by Minowitz' belief that technology and the rampant tendencies of capitalism continue to lie at the heart of world disorder. Perhaps the skeptical Smith, together with his atheistic friend David Hume, understood something about the political dangers of religious factionalism and "enthusiasm" that Northern Ireland and Bosnia (and other recent East European events) have brought to our attention once more.

University of Sussex

DONALD WINCH

Not by Reason Alone: Religion, History, and Identity in Early Modern Political Thought. By Joshua Mitchell. Chicago: University of Chicago Press, 1993. 252p. \$32.00 cloth.

Joshua Mitchell argues that the major early modern political theorists began from an interpretation of history rooted in biblical interpretation. He focuses his attention on four thinkers: Luther, Hobbes, Locke, and Rousseau. He thinks that virtually no contemporary scholar realizes that early modern political theory requires an appreciation for the interconnections between politics, history, and religion. In particular, contemporary readings of these thinkers distort their views by evaluating them as either liberal or antiliberal, a distinction that would not have been meaningful to them. The proper question, according to Mitchell is, "What kind of individual or community does the thinker develop from his theory of history?" The key notion in this investigation

is that of "politically authoritative history," which Mitchell explains as "that history invoked, the implications of which authorize or legitimate a particular political order, extant or envisioned, which simultaneously situates psyche or the soul in a narrative it knows to be true yet might wish to evade" (p. 2).

In addition to an introduction and conclusion, there are four chapters, each devoted to one of his four protagonists. The chapter subtitles identify Luther's dialectic as one of "Supersession and the Politics of Righteousness," Hobbes's as one of "Renewal and the Politics of Pride," and Locke's as one of "Clarification and the Politics of Reason." All three dialectics allude to the meaning of Christianity in relation to the pre-Christian. Rousseau, not being Christian, has no dialectic: "The History of Diremption and the Politics of Errancy."

According to Mitchell, the structure of Luther's authoritative history consists of three parts: the carnal world of the Old Testament, the spiritual world of the New, and the revelation that will occur at the end of the world. This view seems to me to be the generic Christian view. The carnal world is superseded by the spiritual, and the revelation at the end of the world will supersede the spiritual in the Hegelian sense that what supersedes something incorporates and transforms it into something better. In the current state of the world, secular authority is necessary because the carnal world has not been obliterated. Mitchell constructs convenient dichotomies to characterize the carnal and spiritual worlds. The carnal world is active, the spiritual passive; the carnal exterior, the spiritual interior; the carnal relies on good works, the spiritual on faith. These dichotomies strike me as ripe for deconstruction, and Mitchell has an inkling that they are oversimplifications.

Mitchell sees Hobbes as a Christian thinker, whose central worry is how to eliminate the snares of pride. Obedience is the answer. Mitchell calls this a Reformation theme. Again, this element seems to me to be a part of generic Christianity. The moral of the story of the first sin is that all of the miseries of humanity came into the world because Adam and Eve disobeyed God.

Most scholars underemphasize the religious dimension of Hobbes's thought. Mitchell overemphasizes it. He treats the political thought of Hobbes with hardly a mention of the state of nature and no discussion of the laws of nature or the logic of the social contract. Mitchell is correct to see Leviathan as having quasi-divine characteristics but wrong to claim that Leviathan is "the shadowy, mysterious sovereign who never shows his human face" (p. 59). The illustrated title page of *Leviathan* is worth a thousand words of refutation. Mitchell claims that Christ is a central political figure for Hobbes. I think that Hobbes's insistence that Christ came to give counsel, not commands, and that he did not establish a kingdom show that Hobbes depoliticized Christ.

Locke differs from Luther and Hobbes in that for him, there is no tension or conflict between religion and government. Rationality is the principle of politics, and reason dictates that religion and government be separate. This is the basis for Locke's stance on toleration. The dialectical element in Locke's thought is supposed to be the idea that the Old Testament clarifies the New. This continuity between the two does not seem to me to merit the title "dialectical."

Mitchell points out that the reason that Filmer and not Hobbes is Locke's main target in the *Two Treatises* is that for Filmer, Adam is an individual human being, who

received political authority from God, while for Locke, Adam represents all humanity. On Filmer's view, subjects should be treated as children—hence, his patriarchalism. In contrast, on Locke's view, subjects are adults, competent to make their own decisions. Adam's role is "clarified" by Christ, not superseded. He has, then, no special political role to play.

Rousseau's thought is wholly secular in content even though it retains the form of biblical history. It lacks Christ; and that makes all the difference in the world, according to Mitchell. There can be no dialectic between the promise and fulfillment of redemption or the presence and absence of Christ. For Rousseau, there are two states, which Mitchell takes to be historical categories: the state of nature and the civil state. The state of nature is Edenic but lost. The civil state is deficient and cannot be stable for Rousseau, because he cannot appeal to the salvation brought by Christ. Rousseau invents the general will as the savior of people. Mitchell thinks that Rousseau's problem is the same as the one posed by Christian thinkers and that Rousseau cannot solve it because he does not have the resources of Christianity. Assuming that Rousseau's problem is structurally similar to the Christian problem, I do not see why each cannot be seen as instances of a general problem for humans, which is not peculiarly Christian. Mitchell does not consider that possibility.

The text is roughly 150 pages long; the notes occupy about 85 pages. They often seem only tangentially relevant to the text. They could have been pruned by at least half.

This book may appeal to those who talk in terms of trace, diremption, privileging, errancy, and unconcealment. Others will probably find the reading slow and the argumentation lacking or dubious.

University of Texas, Austin

A. P. MARTINICH

The Moral Economy of Labor: Aristotelian Themes in Economic Theory. By James Bernard Murphy. New Haven: Yale University Press, 1993. 244p. \$25.00.

This is a conceptually powerful book that deserves a wide audience. Despite its title (a better one might have been simply *The Moral Foundations of Work* or *Work*, *Human Nature*, *and Morality*), the book is not just for those interested in labor theory or Aristotle. Nonetheless, it fits with a reviving inquiry into Aristotle's economic views—for example, William James Booth's (1993) *Households: On the Moral Architecture of the Economy* (with which it also shares common territory in Marx) and articles by Abram N. Shulsky, Darrell Dobbs, and Scott Meikle.

Besides its lucidity and erudition, what makes Murphy's book compelling reading for all kinds of political theorists (if not all political scientists) is its two forceful and broad conceptual clarifications: of the relation between moral and technical reason and of the relations among nature, custom, and law or deliberately stipulated norms. Abstracted from Murphy's main theme—the character of productive labor—these fundamental philosophical distinctions are in themselves illuminating and will illuminate other problems in political theory and other fields. By clarifying the complexity of the overlapping relations between "the natural order of physical, chemical, and biological processes, . . . the customary order of habitual social practices, . . . and the stipulated order of deliberate design," for example, Mur-

phy exposes the oversimplicity of the nature/convention dichotomy and the sophistic nature/nurture or nature/culture debate it has generated, providing a significant intellectual service that should influence research in many fields, from anthropology to sociology (p. 46).

The book's central concern is no less an original and profoundly insightful contribution to existing scholarship. Following Aristotle, Murphy argues that the exercise of our faculties is fundamental to human flourishing. Reconstructing Aristotle, he argues that even productive labor or economic work should therefore aim to unify the tasks of conception and execution. But it is neither Murphy's call to humanize labor in the workplace (found in his prologue) nor his practical proposals for how to do so (found in his epilogue) that are new or the focus. Similar and related calls and proposals have recently been put forth in edited collections like Nussbaum and Sen's Quality of Life (1993) and Ellen Frankel Paul and her colleagues Économic Rights (1992). By contrast, Murphy's project—an important contribution to the history of economic theory and must reading for anyone whose work invokes Marx or Smith-is to account for why dehumanizing divisions of labor are the norm in both capitalist and communist countries. We must diagnose the disease, which lies in economic theories put forth by thinkers on both the Right and the Left, before we can see the cure.

According to Murphy, degrading divisions of labor exist because they have been theoretically justified in the name of efficiency by classical political economy and classical political thought. Political and economic thinkers from Plato to Taylor failed to conceive a division-oflabor theory that promoted the self-development of each worker because they were handicapped by two erroneous assumptions. First, they assumed that efficiency requires not simply the division of tasks (the technical division of labor) but also the assignment of those tasks to individual persons (the social division of labor). Not grasping the two senses of the division of labor, they assumed that the efficiency criteria were the same for both, blinding them to the insight that productivity is in part a function of worker morale. Second, they thwarted the discovery of a humane division of labor because they assumed that efficiency requires dividing labor according to natural differences, such as age, sex, race, and ability. They failed to see that although efficient divisions of labor must make use of natural powers, custom and stipulation often determine the natural differences on which such divisions of labor are based. Even where custom and stipulation select natural aptitude as the basis for job distribution, matches between jobs and persons are imperfect because our aptitudes are shaped by custom and stipulation.

According to Murphy, then, nature (and thus human nature) is plastic: "All human activities are subject to natural causal laws, but to a considerable extent, custom and stipulation select which causal laws govern a particular activity" (p. 62). As in Stephen Salkever's important Finding the Mean: Theory and Practice in Aristotelian Political Philosophy (1990), Murphy indicates that natural and social science can share a fundamental unity of method that is both explanatory and evaluative. Accordingly, Murphy's analysis avoids the democratic or populist mistake of reducing our resistance to nature to mere choice or will or of implying that morality is a matter simply of choice making: "Nature creates potentialities that are actualized by custom but also imposes restric-

tions on the emergent customs; customs open possibilities for deliberate stipulations but also limit those possibilities" (p. 61).

Murphy's thinking thus again harmonizes with Aristotle's. Although, Murphy observes, Aristotle did not posit a divine personality as the source of natural law, he transformed the traditional concepts of universal unwritten divine law and of the particular unwritten law of equity "into a doctrine of natural law," making a "signal contribution to legal theory" that anticipated Stoic and Thomistic natural law doctrine (p. 141). Murphy thus importantly narrows, without closing, the gap that Leo Strauss sees between Aristotle and Aquinas. What Murphy and adherents of Strauss's interpretation do not consider, however, is that paradoxically, Strauss's own interpretive method (i.e., resolving a philosopher's conflicting statements by giving more weight to less frequently than to the more abundantly made statement) highlights Aristotle's explicit reference to natural law in the Rhetoric against his discussion of natural right in the Nicomachean Ethics.

An overall analysis of Murphy's book reveals, however, a problematic tendency: although he expertly presents the intricate reciprocities between nature, custom, and rational stipulation, he tends (or appears ultimately) to accord rational stipulation the upper hand over nature and its pull through custom. Even if the skeptical reader sets aside doubts that most productive labor can be redesigned to include a stimulating conceptual dimension and that such redesigned work can motivate the unmotivated, one wonders if promoting self-development through challenging work might notby cultivating pride, competitiveness, or ambition in workers-threaten, rather than achieve, the resulting "solidarity" in the workplace that Murphy anticipates. He criticizes Marx for predicating social solidarity on a homogenized species beingness, instead of on differentiation; but at least Marx understood that we cannot perfect both individuation and social solidarity.

In the epilogue (the only disappointing part of the book) the paradox of Murphy's larger argument crystallizes. By sketching tired and unproven communitarian proposals for promoting better jobs-unions, workplace democracy, and education of workers in engineering (to gain mastery over conception)-Murphy inadvertently compels the reader to recall his earlier devastating critique of the pseudoscience of society management called "political economy": since that science presupposes "a collective subject that acts to maximize social welfare" (the "communist fiction" that society is organized into a single household or firm), it "points inexorably to socialism," "in short, is possible only in the absence of politics" (p. 145). Surely, individuation increases politics (as Athenian democracy proved), which in turn threatens the communist fiction on which rest the "strong and progressive" unions that Murphy says are "a minimal prerequisite for self-realization at work" (p. 230).

The main problem, then, with Murphy's self-realization-through-work argument is that (ordinary) work is done at work, with multiple others. To achieve the level of social harmony we need to get work done at the workplace, we need to compromise and to some degree suppress the very drives (which the Greeks called *eros* and *thumos*) that fuel self-realization or excellence. If work has such a political dimension that impedes the pursuit of excellence or development of our true selves, then the logical alternative is to develop ourselves

through nonpolitical activities. Addressing in his prologue the possibility that leisure might become the primary sphere for the realization of human potential, Murphy points out that there is a fruitful dynamic between work and leisure: "Challenging work leads to challenging leisure" (p. 4). While the habits and skills developed at work may enrich leisure, might not leisure (as Plato and Aristotle argue) develop different and higher skills that most ordinary work cannot? And might not the attitudes and opinions assimilated at work-especially at deliberately democratized workdiscourage self-realization after work? Indeed, Nietzsche may be right that stipulated or mandatory communality cultivates a herd mentality that discourages the pursuit of differentiating activities. Thus, although the call to make work more developing of the self is well intentioned, perhaps the democratic tendencies or demands of work frustrate, as much as they foster, the realization of our highest individual potentialities.

Boston University

JUDITH A. SWANSON

The Nature of Rationality. By Robert Nozick. Princeton: Princeton University Press, 1993. 226p. \$19.95.

The Concept of Political Judgment. By Peter J. Steinberger. Chicago: University of Chicago Press, 1993. 300p. \$53.00 cloth, \$17.95 paper.

On the face of things, these books form a good pair to review together; in fact, they make a very odd couple: readers who find the style of argument of both books to their taste are likely to be rare. They reflect the odd way that the label "political theory" is used in political science—as (1) a commentary on, and continuation by close reading of, texts of the long tradition of philosophical reflection on politics and (2) the often mathematical analysis of politics as the aggregate effects of the narrowly rational choice of individuals responding to incentives, modeled after, and descending from, economic theory.

Nozick is a philosopher best known for a work about politics (Anarchy, State, and Utopia, 1974). Steinberger is a political scientist whose central concerns are philosophical (a previous book was on Hegel). At a sufficient level of generality, both books are about the same kind of difficulty, namely, that we seem to have two modes of judgment: seeing-that (immediate, subjectively processless intuition) and reasoning-why (step-by-step reasoning). But what if the two conflict? Or what if a seemingly compelling direct intuition cannot be backed up by cogent reasoning-why? Or what if what seems like compelling reasoning-why yields a result that seems intuitively intolerable? Both writers are focused on this family of concerns; but there all resemblance ends.

Steinberger's book is one long argument, mostly in the form of a commentary on famous earlier writers, from Plato and Aristotle, through Hobbes and Kant, to Oakeshott and Arendt in our own time, all on the fundamental theme of the intuitive sense of things that yields good political judgment and the need to tie that to careful reasoning-why—in particular, so that we have a chance to notice, discuss, and perhaps step beyond the absolute thrall of the prejudgments (prejudices, as Steinberger frankly labels them) that facilitate but also inevitably entrap our intuitions.

All this seems entirely reasonable. But it was hard for

me, as an outsider to the mode of discourse used here, to see what emerged from the discussion that was substantially different from my impressions before starting the book. An oddity of Steinberger's argument seems to be the lack of any reference to Max Weber's famous essay, "Politics as a Vocation," which has a lot to say on the character of good judgment in politics and how it differs from good judgment in more private realms. On the other hand, I expect that readers with a taste for political theory in the first sense may find a good deal more that is controversial and insightful in Steinberger's analysis than I have managed to appreciate.

When we come to Nozick, things could not be more different. There is a thread of a common style through the book, but not of a single sustained argument. Rather, the book is something of an anthology of puzzles of rational choice that have caught Nozick's attention, which he then offers his ideas on resolving. The resolutions are cleverly presented; but they usually have a mechanical and unconvincing character, like fixing up difficulty in Ptolemaic astronomy by adding more epicycles. They seem to me to paper over, rather than resolve, the difficulties of rational analysis of human judgment.

The most central of Nozick's puzzles concerns rational belief where there is a conflict between what rational analysis would lead a person to believe and what would do the most good to believe. Nozick uses the example of a mother whose son appears, by usual standards of judgment, to be clearly guilty of a horrible crime: Might it be rational for her to refuse to believe her son in fact is guilty? And (as in past work which has spawned a large literature) Nozick uses "Newcomb's puzzle" as an artifactual exemplar of such dilemmas, where common sense does not seem to want to go along with good reasoning. In stark form, suppose you are the latest in a long line of people confronted with this choice: there are two boxes, one transparent and containing \$1,000, the other opaque from the side you see it but transparent from the back. That box contains either \$1,000,000 or nothing. A television camera looking at the back has shown everyone in the country (except you) whether that second box is empty or contains the \$1,000,000. Your choice is to take both boxes (giving you \$1,000 plus whatever, if anything, is in the second box) or to take the second box alone.

The usual rational choice analysis is that you take both. The million is either in the second box or it is not, so how could you lose by taking both? But Newcomb's premise is that after hundreds of such episodes, you know that without exception, each time the chooser took both boxes, the second box turned out to be empty and that without exception, each time the chooser took the second box alone, he found it contained the million dollars

Nozick thinks (as most readers would and I certainly do) that it may not be irrational for the mother to refuse to believe her son is guilty and that it also would not be irrational for the chooser to take only the opaque box, despite a clear violation of the usual sure-thing axiom of rational choice. So Nozick would like an extension of the notion of rational choice that allows for choices like these to be deemed rational. Nozick's resolution is to make belief a weighted function of what he calls "credibility," which is not precisely defined but is akin to a Bayesian prior: this is the standard rational choice component. But that is to be augmented by an anticipated value component (the value to the mother of leaving open the

possibility that her son is innocent, the value to the Newcomb's chooser of probably winning the million) and also a symbolic value (e.g., the psychic cost to the Newcomb chooser of violating rational principles).

All this, though cleverly worked out, does not seem to me to really help. The assessments required will usually be highly vulnerable to the prejudices that worry Steinberger but do not seem to bother Nozick, and the weights given the three motives are left undefined. All in all, it seems a scheme which can rationalize any belief ex post, whereas the great value of careful reasoning and more formalized rational choice analysis is that it helps us confront inconsistencies in our intuitions, (which had been taken for granted) and eventually reach more reliable intuitions. If we lived in a world where Newcomb's problem was actually credible, then (with lots of experience with that) we would find our intuitions about how sure the sure-thing principle is would change, just as physicists eventually found their intuitions about what surely must happen in subatomic processes changing. And the mother's dilemma seems better understood in terms of human sympathy for another human in a terribly painful situation than as an extension of rational choice theory.

On the other hand, though I have mostly complained about first Steinberger's, and then Nozick's, book, it is characteristic of really important problems that fundamentally satisfactory resolutions are hard to come by, sometimes not even possible. In their very different ways, both books reveal the intelligence and seriousness with which the authors have pursued their projects. If very few readers would find both books to their taste or either one wholly convincing, it is probably also the case that few readers would not find at least one a profitable exploration.

University of Chicago

HOWARD MARGOLIS

Morality and Politics in Modern Europe: The Harvard Lectures. By Michael Oakeshott. Edited by S. Letwin. New Haven: Yale University Press, 1993. 115p. \$22.50. Religion, Politics and the Moral Life. By Michael Oakeshott. Edited by Timothy Fuller. New Haven: Yale University Press, 1993. 164p. \$22.50.

Although Michael Oakeshott lived into his ninetieth year and thus came within a twelvemonth of outliving the Hobbes on whom he had thrown more light than any other twentieth-century interpreter, he published only three books: Experience and Its Modes (1933), Rationalism in Politics (1962), and On Human Conduct (1975). It is therefore unusually important when "new" works by the most eminent "skeptical liberal" since Montaigne and Hume are brought into the light. This is especially true because two of the three published books, wonderful (and wonderfully written) as they are, represent Oakeshott only incompletely. Experience and Its Modes is most notable for its brilliant reflections on the philosophy of history (which R. G. Collingwood praised to the skies) but so denigrates mere "practice" as to leave political theory little place: while philosophy aims at experience without reservation or arrest, without presupposition or postulate, without limit or category, 'practice" (including politics), by contrast, is indeed an "arrest" in experience because it one-sidedly interprets the world sub specie voluntatis, exhausts itself in the (necessarily doomed) enterprise of converting what is into what "ought" to be: "The presupposition of practical experience is that 'what is here and now' and 'what ought to be' are discrepant. . . . Permanent dissatisfaction is inherent in practical experience; the explicit assertion of reality in practical judgement is never complete and consequently the world of practical experience is . . . abstract and incoherent" (Experience and Its Modes).

To be sure, Oakeshott had retreated from that extreme demi-Hegelian position by the time he published Rationalism in Politics 30 years later. But one can wonder whether that all too brilliantly successful volume, with its insistence on the "sympathetic pursuit of intimations" within inherited historical "traditions" does not cast a misleading "Burkean" light on Oakeshott's vision of politics; for the most famous essay in Rationalism, "Political Education," first annihilates two straw men ("empirical" and "ideological" politics) in a way that reduces one to helpless laughter, then magnifies "our tradition of political behaviour" in language deliberately reminiscent of Reflections on the Revolution in France ("Politics is steady because, though it moves, it is never wholly in motion. . . . Authority is diffused between past, present and future"). Following Rationalism's attack on "ideology," such philosophy as survives is more or less identified with the "rationalist" schemes of second-hand Cartesians and third-rate Kantians: "With every step it has taken away from the true sources of its inspiration, the rationalist character has become cruder and more vulgar. What in the seventeenth century was L'Art de penser has now become Your mind and how to use it, a plan by world-famous experts for developing a trained mind at a fraction of the usual cost." In much of Rationalism in Politics, one sees an Oakeshott wonderfully in command of a satirical idiom but also one more purely "English," less philosophical, than elsewhere.

Only On Human Conduct (1975), his magnum opus, presents a fully rounded (though less charmingly funny) Oakeshott. The book, like Gaul, is divided into three parts: part 1 is a compressed, dense, difficult account of human agency, of intelligent self-determination and "self-enactment"; part 2 is an account of the "civil condition" or respublica (above all, authoritative lex) that cives are to "subscribe to" in their pursuit of self-chosen individual satisfactions—with politics restricted to maintaining lex (rather than indulging in a Baconian transformation of the world through collective "joint enterprise.") Part 3 is called "On the Character of a Modern European State" but really deals with the advent of every significant moral "disposition" that has appeared in Europe, though it does much to distinguish between the state as a societas of authoritative rules suscribed to by cives in their chosen self-enactments and the state as a universitas—an "enterprise association" run by "teleocratic" managers in pursuit of a "substantive common purpose." And in the most beautiful lyric flight in part 3, Oakeshott celebrates "enjoying individuality" with unmatched pages on Augustine, Montaigne, and Cervantes ("a reading of the human condition as an adventure in personal self-disclosure."

The new Oakeshott volume entitled Morality and Politics in Modern Europe: The Harvard Lectures, is a kind of early version of On Human Conduct, part 3. Beginning with the claim that "the history of modern European morals displays to us two distinct and opposed moral dispositions or moralities: the morality of individualism and the morality of collectivism" (p. 27), Oakeshott goes

on to illustrate his sympathy for the first and his aversion to the second with fine accounts of Montaigne, Montesquieu, Kant, and (unexpectedly) Bentham and with more jaundiced readings of Owen, Fourier, and Marx. (For Óakeshott, J. S. Mill is an equivocal figure—a supposed liberal individualist who is finally unmasked as a devotee of perfectionist uniformity: "Mill's plea for diversity is methodological, not substantial: diversity must be allowed only because we cannot yet be certain about what is true and what is false.") If, in the Harvard lectures, Oakeshott's reading of Montaigne is fine but no match for the lovely pages in On Human Conduct, Kant and Bentham, by contrast, are viewed more favorably than anywhere else in the Oakeshottian corpus: in Kant's Rechtslehre and other political writings, there is "to be found a genuinely philosophical version of the political theory of individualism." His "task" was to give "the experience of individuality" a "metaphysical and ethical context"; and he "performed it more thoroughly, perhaps, than any other writer." And in the Harvard lectures, Bentham is treated not with the amused contempt of Oakeshott's 1932 Scrutiny essay ("As a thinker ... he is negligible") but as a respectable minor contributor to the morality of individualism: "Bentham did not merely wish each man to make his own choices for himself about his own happiness: he believed that the persons he had to do with were, in fact, persons of this kind.'

Fine as the 1958 Harvard lectures are (they also contain splendid pages on Montesquieu, Locke, Smith, and Burke and will stand as a monument to their devoted editor, the late Shirley Letwin), they are really a pendant to *On Human Conduct*. Only Kant and Bentham are seen to better advantage in the earlier work.

It is more difficult to characterize the second "new" Oakeshott volume, Religion, Politics, and the Moral Life, since this book pulls together very disparate essays stretching from 1925 to 1955 and includes everything from early pieces on Christianity to reviews of Morgenthau and Walter Lippmann. The Christianity studies from the middle and late twenties are fascinating (with extraordinary "asides" on Pascal and Augustine). Perhaps the finest is "The Importance of the Historical Element in Christianity" (1928), which insists that "a religion, in a manner to which a philosophy is not subject, may be proved lacking by being proved inadequate to the [practical] demands of the ordinary life of ordinary people. . . . Religion exists to satisfy no craving for knowledge apart from the knowledge which comes with the mere strength and courage to take life as it is." (Those eloquent lines foreshadow some of Oakeshott's last thoughts on religion, in a letter written shortly before he died to the present reviewer: "I have gone back to 'theology'—or rather, to reflection upon religion. And I would like, more than anything else, to extend those brief pages in On Human Conduct into an essay . . . on religion, and particularly on the Christian religion. This ambition came to me, partly, from my re-reading all that St. Augustine wrote-St. Augustine and Montaigne, the two most remarkable men who have ever lived. What I would like to write is a new version, a post-Montaigne, post-Pascal, post-Blake version of Anselm's Cur deus homo—in which (amongst very much else) 'salvation', being 'saved', is recognised as [having] nothing whatever to do with the future." Those sentences, from 1986, have roots going back to the twenties, as the "new" volume makes very clear.

Among the later pieces in Religion, Politics, and the

Moral Life, three may be singled out as especially notable. The most radical is surely the Scrutiny essay, "The Claims of Politics"—which serves to recall that Oakeshott always believed in a moderated version of Nietzsche's dictum that "every great age of culture is an age of political decline"; for it was in September 1939, when everyone was rushing to philosophical, as well as to actual, barricades, that Oakeshott had the nerve to say:

Politics is a highly specialized and abstracted form of communal activity; it is conducted on the surface of the life of a society and except on rare occasions makes remarkably small impression below that surface. . . . It is a function mainly of protection.

... A limitation of view, which appears so clear and practical, but which amounts to little more than a mental fog, is inseparable from political activity. A mind fixed and callous to all subtle distinctions, emotional and intellectual habits become bogus from repetition and lack of examination, unreal loyalties, delusive aims, false significances are what political action involves. . . .

But in every society there are, I believe, some for whom political activity would be a perversion of their genius, a disloyalty to themselves, not because they have little or no part in the promotion of the communal interests of their society, but because their part is one which it is essential that a society shall have performed and which it is difficult if not impossible to combine with political activity. And among them, I believe, are those whose genius and interest lies in literature, in art and in philosophy.

That skepticism about politics' worth also comes out, in a lighter and funnier way, in "The Customer Is Never Wrong" (1955), in which Oakeshott insists (against Walter Lippmann) that politics "are concerned with the cultivation of what from time to time are accepted as the peaceable decencies of conduct among men who do not suffer from the Puritan–Jacobin illusion that in practical affairs there is an attainable condition of things called 'truth' or 'perfection'." By contrast, "liberal democracy" is a "skeptical politics in which 'truth' appears not as the opposite of 'error' but merely as the opposite of 'lies' and in which utterance is largely free because it is recognized not as argument but as conversation." And he adds that "if we want instruction about the theory of liberal-democratic freedom of speech it is not to Milton or to Mill that we should go, but to Montaigne and Hume."

Perhaps the most extraordinary essay in this second "new" volume, however, is the one called "The Concept of a Philosophy of Politics" (c. 1946), which is a reworking of the magisterial 1938 article, "The Concept of a Philosophical Jurisprudence." In the postwar reworking, Oakeshott omits the earlier commentary on assorted jurisprudes of the early twentieth century (above all Pound), and preserves his clearest statement of what philosophy can hope to achieve:

Philosophical thought and knowledge is simply thought and knowledge without reservation or presupposition. . . . It should be thought of as something which happens at the end, when the concepts of science, or common-sense, or practical life are subjected to the revolutionary and dissolving criticism of being related to a universal context.

The starting place in philosophy is not some remote region of experience known only to the philosopher. . . . A philosophy of law, for example, does not begin with already defined and accepted abstract ideas such as the ideas of Right, Duty and Obligation. . . . It begins with any concept of law a man, however uninstructed, may happen to find in his head. In philosophy, therefore, there is no such thing as a transition from mere ignorance to complete knowledge; the

process is always one of coming to know more fully and more clearly what is in some sense already known.

And in a closely related (unpublished) piece from circa 1946-50 called simply "Political Philosophy," Oakeshott urges that "the attitude of radical reflective subversiveness" ("without hindrance or arrest") comes out most notably in Plato's Republic, Hobbes's Leviathan, Spinoza's Ethics, and Hegel's Philosophy of Right—those more-thanideological works that "mediate between politics and eternity." (In connection with the "radical subversiveness" of Hobbes, it is a pity that room could not be found, in this second "new" volume, to republish Oakeshott's wonderful 1935 Hobbes essay from Scrutiny: "Radicalism, extravagance, the intrepid following out of a theory conceived in the grand manner and the absence of any sign of alarm, dismay or compromise are not qualities often to be found in English thinkers; but they flourish in Hobbes almost unchecked. . . . Hobbes appears never to have been even tempted to make his conclusions more moderate."

It remains only to say that Timothy Fuller, by retrieving these long lost or never-published works of Oakeshott, continues to place us deeply in his debt. More than any other scholar, Fuller has helped to preserve Oakeshott's poetic voice "in the conversation of mankind."

University of Wisconsin, Madison

PATRICK RILEY

Pluralism: Against the Demand for Consensus. By Nicholas Rescher. New York: Oxford University Press, 1993. 208p. \$29.95.

The subtitle of Nicholas Rescher's new book explains it all. It is less concerned with defending pluralism than with attacking "the demand for consensus." Rescher, at the start, defines consensus as "uniformity of belief and evaluation" (p. 1)—a fiercer conception of it than many of us would have in mind—and goes on to suggest that consensus has been the aim of many philosophers and social theorists from Aristotle to C. S. Pierce and Jürgen Habermas.

They were mistaken, in his view, (1) because the pursuit of truth does not necessarily produce consensus and it is truth, not agreement, that the philosopher should be concerned with and (2) because, since "value dissensus inheres in the human condition" and "value disagreement is . . . ineliminable," it is simply "unrealistic and unreasonable" to try to achieve consensus in such matters (pp. 131–32, 134). In fact (not surprisingly), Rescher sees the pursuit of consensus as politically sinister, "the last stand in an ethos of democracy of a pre-democratic dirigisme" (p. 3). In the final chapter, he tells us that the aim of political consensus derives "from a fundamentally socialistic commitment to the co-ordination and alignment of individual action into the uniform social order of 'rationalized' central planning" (p. 187).

This being the work of a contemporary academic

This being the work of a contemporary academic philosopher, the book comes garnished with tables and a good deal of the "If P, then . . ." type of formulation that seems to be obligatory these days, but actually adds little or nothing to the main text. A more serious defect is the almost complete absence of any sense of history or sociology. If this were a work of pure philosophy, that might not matter. But Rescher is arguing that societies

do not need consensus and ought not to aim at it, and the first of these contentions is an empirical proposition (at least in part) and needs to be supported by a consideration of relevant evidence. Instead of evidence, we get bald assertion. "A viable society can dispense with consensus (the USA is a pretty good example)," he tells us in his breezy way (p. 167). To this outsider it sometimes seems as if the United States, with its frightening levels of urban violence, rigid ghettoization, ethnic conflicts, and sporadic rioting, is a society where the dissensus Rescher celebrates is in danger of generating wholesale fragmentation.

Rescher says that what is needed is not greater consensus but "a mechanism for the limitation of conflicts" (p. 182). But since some of these conflicts spring precisely out of the absence of consensus and since they often produce violence, destruction, and death, what mechanisms are there (beyond the law and law enforcement agencies) that can limit such conflicts? There are currently, in Western societies, groups that are so convinced of their religious duty to try and murder a supposedly blasphemous novelist that he has to live in hiding and under constant police protection. Is that acceptable dissensus? Would it not be better if there was a societywide consensus that regarded murder as intolerable, however scandalous the victim's crime might be?

The real questions in this area are, Does any society need *some* common values in order to function at all? And Are there limits to the range of beliefs and commitments (dissensus) that a society can accommodate without disintegrating? The tendency of Rescher's arguments seems to be toward answering *no* to both questions. But such is the lack of detail and examples in his discussion that it is impossible to be sure.

He seems to envisage a society of coolly rational individuals who find it easy to agree to differ. Thus, he tells us blithely that "only for the narrow-mindedly intolerant will disagreement issue in conflict or diversity of opinion in social friction" (p. 182). Really? How does this apply to racism, for example, or sexism? The trouble is that there are, by Rescher's standards, a great many narrow-mindedly intolerant people about, and dealing with the conflicts they give rise to is a far more intractable matter than he seems to appreciate.

Of course, he is right to argue that modern societies are inherently pluralistic and require a high degree of mutual tolerance. But he does not seriously consider whether there are necessarily limits to the extent of dissensus that any harmonious and peaceful society can, in practice, contain. A mere celebration of the virtues and advantages of disagreement is not enough.

University of Sheffield

ANTHONY ARBLASTER

The Foundations of American Citizenship: Liberalism, the Constitution, and Civic Virtue. By Richard C. Sinopoli. New York: Oxford University Press, 1992. 215p. \$32.50.

At the beginning of this challenging study, Richard Sinopoli declares that its theme is a problem posed by Rousseau—how individuals are to balance the competing demands of civic duties with private desires. He never entirely loses sight of that question, but it soon develops that most of the work is addressed to a far more specific matter, namely, whether, in the debates

over the ratification of the American constitution, proponents and opponents were guided by the neoclassical tradition of civic humanism (that man is a political being who finds his highest fulfillment through active participation in public life as a citizen of a republic) or by classical liberalism, either in its "possessive individualist" version or in a more moderate form. Interwoven in that analysis, in turn, is an examination of the philosophical and psychological sources of allegience and obedience, as seen by Federalists and anti-Federalists and the writers who influenced them.

That is an undertaking of vast proportions; and in considerable measure, Sinopoli handles it well. He prudently rejects the more extreme readings of both civic humanism (J. G. A. Pocock, Gordon Wood) and liberalism (C. B. Macpherson) on the ground that they are not much in evidence in the writings of Publius and his leading opponents. Instead, he formulates a reasonable "weak version of the republican thesis" (p. 11) and a version of liberalism that is compatible with a sense of community (as in fact it was in Locke and in the concept as it was understood in America). Then comes the heart of the work in the form of careful, cautious, and thorough analyses of Locke, the Scottish Enlightenment (Hutcheson, Reid, Hume, and Smith), the Federalists, and the anti-Federalists. The result is to restore Lockean liberalism and compact theory to the central place in the American founding. Along the way, Sinopoli clarifies, by subtle reasoning, a number of points that previous scholars had muddled.

But his performance is far from flawless. First, there are some major errors of omission. There is nothing here of the English country-party ideology (and only passing reference to the rediscoverer of that ideology, Bernard Bailyn), without which the anti-Federalists are incomprehensible. The analysis of Hume is satisfactory as far as it goes, but there is no mention of Hume's insistence in Essays: Moral, Political, and Literary (1741) that the "stated forms and methods" governing administration are crucially important as a "check on the natural depravity of mankind"-a conception that Hamilton took to heart and made the centerpiece of his statecraft. (Sinopoli does touch upon Hamilton's concern with administration, but he misreads it entirely; indeed, his interpretation of Hamilton throughout is clichéd and uninformed by Hamilton scholarship during the past three decades.)

Errors of commission abound. Sinopoli quotes Madison accurately to the effect that the purpose of government was to secure liberty and happiness but quite misleadingly defines happiness as "the prosperity brought about by industry" (p. 19). Shortly thereafter, he says that "the dividing line between persons with rights and obligations in the liberal policy and 'nonpersons'" was usually "property qualifications" (p. 22). Has he not heard of race, sex, and nonage? He describes Hamilton's reference to "first principles" in Federalist 31 as "Lockean" and "Reidian," whereas in reality Hamilton is describing the enthymeme in Aristotelian rhetoric (p. 70). Sinopoli equates Madison's defense of property rights with "liberal capitalist competition," whereas Madison's actual conception of property rights was fundamentally precapitalist (esp. p. 91). He cites Madison's opposition, in the federal convention, to a proposal that the states pay their senators, on the ground that it "would make the Senate like Congress" and then goes on to explain, "that is, like the House,"

when Madison was obviously referring to the Congress under the Articles of Confederation.

Finally, there is an act of commission that is utterly outrageous. Instead of the traditional use of the masculine as the neutral pronoun, Sinopoli (or his publisher) regularly alternates between masculine and feminine. The usage is, in most instances, merely silly. But sometimes it is an unconscionable distortion of history—as when, in discussing Pocock's definition of classical civic humanism, Sinopoli writes that "any individual with insight into her own well-being would seek the development, or perfection, of her most essentially human faculties" (p. 10). At the very heart of republicanism, in the ancient world as well as in eighteenth-century America, was the understanding that participation in public affairs was for men only. To employ the political correctness gimmick in that context is as inappropriate as in a discussion of abortion (such as is found a few pages later) to speak of a woman's right to control his own body.

University of Alabama

FORREST McDonald

Psychoanalytic-Marxism: Groundwork. By Eugene Victor Wolfenstein. New York: Guilford, 1993. 468p. \$40.00 cloth, \$19.95 paper.

This book attempts to evaluate the history of the conflicting relations between psychoanalysis and Marxism, as well as to offer the theoretical bases for a synthesis between the two. After an initial chapter describing the problem to be studied, Wolfenstein proceeds, successively, to establish a genealogy of the relations between both bodies of theory, to lay the grounds of his own vision, and to draw from the latter a set of conclusions for both the understanding of contemporary social struggles and a new emancipatory vision. To accomplish the first of these tasks he starts by contrasting the visions of Marx and Freud. This is followed by a discussion of the various attempts at a synthesis by Wilhelm Reich, Eric Fromm, and Herbert Marcuse, as well as of the contemporary theoretical tendencies that claim to go beyond Freudian Marxism. The second part (which, on top of Marx and Freud, thoroughly discusses Hegelian philosophy) explores the concepts of consciousness and alienation and gives a central role to the category of recognition. This centrality is the result of the pluralization of emancipatory struggles in contemporary society, which requires going beyond a vision of the interaction between groups conceived in purely antagonistic terms: This development is a consequence of the pluralization of emancipatory politics. Classical Marxism, as a theory of class warfare, can bypass the issue. . . . But the world we seek to win cannot be all female, all male, all black, all white, all African, all Asian, all European, etc. At the existential level some particularity is not only unavoidable but vital and vitalizing. Hence the importance of the problem of recognition" (p. 169). Finally, in the last part, Wolfenstein confronts his theoretical perspective with contemporary struggles around class, gender, and race and postulates a project of human emancipation grounded in seven principles: going beyond the systems of alienated labor, making human interests inclusive, removing impediments to emotional growth and wellbeing, limiting desire to possible human experience, aiming at mutual recognition, replacing hierarchical social organizations by participatory ones, and preserving natural–environmental conditions.

There is no doubt that the problems addressed by Wolfenstein are central ones in contemporary societies. It has also to be said that his recognition of the plural and fragmentary nature of contemporary social actors is a merit of his book, as well as his attempt to go beyond a purely antagonistic notion of social interaction (as was the case with the classical notion of universal class). But the shortcomings of Wolfenstein's theoretical enterprise are also only too visible. Perhaps the root of most of them is the entirely unilateral and arbitrary selection of the authors that he has decided to discuss. There would certainly be no objection to a purely analytic exercise, entirely devoted to the internal critique of some classical texts (Marx's and Freud's, in this case). But Wolfenstein does not confine himself to this task. He makes, instead, detailed references to a variety of intellectual traditions derived from those basic theoretical matrices. He does so, however, on the basis of such an arbitrary selection that one cannot avoid the conclusion that his intellectual enterprise is uneasily located in a no man's land between intellectual history and analytical presentation.

Psychoanalytical theory, for one, as Wolfenstein well knows, has advanced a great deal since Freud's days. Now, given Wolfenstein's primary interest in alienation, identification, and the dialectics of desire, one would have expected to find in his book a serious discussion of Lacanian theory, which is centered precisely in those notions and has been extended, over the last decades, to the study of the sociopolitical field. But the only reference to Lacan that we find in the book (apart from two other passing mentions) is the following: "Lacan is far and away the most fallocentric and narcissistic of psychoanalystic theorists. One must defeat him in order to win the struggle for psychoanalysis. But this is not the whole story, not even, perhaps, its principal narrative line. Why, especially outside France, all the attention to the narcissistic Lacan? Why the emulation of his language and style? Why the willing acceptance of his terms for the debate about gender? Is this critique, or is it compliance?" (p. 140). Needless to say, there is no attempt to answer these half-rhetorical questions. The same cavalier treatment is reserved for deconstruction

The same problems are to be found in what concerns Marxist categories. The author passes too quickly from Marx analyses to his own critique and reformulations, paying little attention to the various attempts by twentieth-century Marxist authors to engage in similar endeavors. Concepts such as alienation or false consciousness, for instance, have been the subject of intense debates in the 1960s and 1970s, with some Marxist authors making them the cornerstone of their approaches while others repudiated them. Wolfenstein, however, takes the fruitfulness of these categories as unproblematic and makes no reference to those debates. The result is that even his more promising ideas appear rootless and without a precise direction.

I do not want my assessment of this book to appear as entirely negative. The author shows a considerable knowledge of the texts of Hegel, Marx, and Freud, and many of his intuitions are sound, original, and well worth pursuing. But I think that the shortcomings of the book clearly outweigh its merits.

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ERNESTO LACLAU

AMERICAN POLITICS

Uncertain Warriors: Lyndon Johnson and His Vietnam Advisers. By David M. Barrett. Lawrence: University Press of Kansas, 1993. 279p. \$35.00.

Whatever Possessed the President? Academic Experts and Presidential Policy, 1960-1988. By Robert C. Wood. Amherst: University of Massachusetts Press, 1993. 208p. \$40.00 cloth, \$15.95 paper.

A main frustration in doing presidential scholarship is the difficulty of penetrating the black box of White House decision making. Even when there is available a raft of documentation in the form of reports and memoranda, including decision memoranda on which a president indicates a choice, there is usually the sense that important pieces of the puzzle are obscured. Consequently, studies that make the black box even a bit less mysterious are welcomed. *Uncertain Warriors*, by David M. Barrett, and *Whatever Possessed the President?* by Robert C. Wood in somewhat different ways are polished, rich, and useful additions to the literature on the presidency in this regard.

Both focus on advisory structures and processes. Barrett's book presents three case studies of Lyndon B. Johnson's Vietnam decision making. They are the decisions to escalate in 1965; the debate over options in 1967; and the choices made in the aftermath of the Tet Offensive in 1968, including Johnson's decision not to seek reelection. Wood is concerned with the role of academic advisers, especially social scientists, in presidential politics and decision making from the Kennedy through the Reagan years.

The core purposes animating the two books are similar in some respects but different in others. Both, of course, seek to illuminate the shadowy world of presidential decision making. More specifically, Barrett is interested in verifying or confounding the conventional wisdom about the nature and operations of Johnson advisory system that contributed to the fateful results that were the nation's Vietnam experience. Was it, in fact, "narrow, cohesive, and overwhelmingly hawkish?" (p. 169) Were aides afraid to offer candid advice? Was Johnson close minded about the war and prone to orchestrate agreement among his advisers in support of his predetermined positions? Was it stable, or did it change over time? Was, then, the advisory system simply an extension of Johnson's complex and often difficult personality?

Wood charts the ups and downs of presidential advisers with academic credentials through six presidencies. In addition, woven through the book is a brief. Essentially it is that academic social science, with its "reliance on data, the disciplined use of inference, the articulated distinction between fact and value," (p. 168) can make important contributions to public policy.

A strength shared by the two books lies in clearly stated conceptual frameworks in which analyses are set. Barrett's baseline for his assessment is presidential rationality which is defined as "responding to the environment by developing an advisory system to help determine and reach policy goals, seeking diverse views and information, and connecting means to ends" (p. 9). He also specifies a relationship between the political

environment and the characteristics of advisory systems. Wood's core concept is "talk," political talk about attaining and using power. This is the basis for the questions at the heart of his analysis: "Where do the ideas come from ... Who offers ideas on politics, through what form of talk, and at what time in the political process" (p. 3)? Presidential candidates and presidents mainly talk to three elites: other politicians, professional administrators, and policy experts. There are also outsiders involved in presidential talk, including political philosophers and image makers. Three types of talk, rhetoric, ideology, and knowledge, compete in the presidential arena during campaigns, transitions, and in governing. Decisions arise out of "the ever-present struggle between the ideas of the president and those of the three elites" (p. 6). Wood participated personally in presidential talk for many years starting in John F. Kennedy's presidential campaign, followed by service as chairman of two of Johnson's Great Society task forces, then as under secretary and secretary of the Department of Housing and Urban Development.

Based upon a meticulous examination of the Johnson papers, the papers of other participants, such as Senator Richard Russell, oral histories, memoirs, interviews, and previous studies, Barrett makes a convincing case that Johnson's Vietnam advisory system was rational. Overall and in the particulars, the reality he depicts is far from the pathological horror often described. It was fairly open system, though disguised by Johnson's insistence on confidentiality, and populated by persons in and out of government with diverse views. Johnson listened to and seriously considered the varied points of view and assessments that came to him through a variety of channels.

Moving beyond understanding Johnson's system and how it worked, important in and of itself, of broader interest is Barretts' effort to understand how the system evolved over time. Basically, he suggests, advisory systems are shaped by presidential preferences, but also by the political environment. He theorizes that they will be orderly and collegial when the president's goals and the means to achieve them are clear. They will be less orderly and more competitive when goals and the means to realize them are controversial and unpopular. When goals are clear, but there is uncertainty as to means, they will be a hybrid. He finds the advisory system to be of the third type in 1965, but slightly more competitive than collegial. In the hybrid form of 1967, the collegial outweighed the competitive element in the system. The following year, 1968, although collegiality was still evident, there was a distinctly higher level of competition among Johnson's advisers. His personal style did not change, but his advisory system did.

Barrett concludes his book with consideration of Johnson's political style and the relationship between rationality in advisory processes and the quality of decisions. He concludes that the particulars of Johnson's Vietnam decision were shaped less by the nature of his approach to advice taking than by his world view and the political circumstances of the time.

Wood treats the reader to a lively and interesting history of academic advice based upon his own experi-

ences and systematic investigation of its place in other presidencies. Much of his description of academic advice in post-Johnson presidencies is fresh material. Not surprisingly, he finds that the high water mark reached by academic social scientists and their knowledge talk was during the days of Johnson's Great Society. In subsequent presidencies, although there were academics present, they were most evident in campaigns and transitions, less so in governance. In the Nixon and Carter presidencies, lawyers became increasingly important in shaping policy. Over time, knowledge talk unfortunately has lost ground to rhetoric and ideological talk, Wood believes.

He seems to be somewhat ambivalent about the Reagan presidency, however. On the one hand, for Wood, Reagan was "a man of beliefs, not ideas" (p. 139), and ideological talk was a prominent part of his presidency. The ill-conceived supply-side economics and the "reckless rush to deregulation" (p. 160) were products of ideology, not knowledge. Yet in other areas, such as the New Federalism proposals, Wood sees circumstances that cause him to conclude, "The pattern of advice and counsel from the social sciences not only persisted in the Reagan administration; it was expanded and, in its implementation, perfected," (p. 160). As Wood observes, the line separating ideology and knowledge is blurred, and it remains so in present understandings of the Reagan presidency.

Another troublesome part raised but not fully resolved in Wood's book is, in general, the loss of currency of academic social science and the present preeminence of ideological talk over knowledge talk. In his last chapter, there is a provocative essay that touches upon a variety of possibilities, including the troubled state of universities and social science disciplines that make them inhospitable places for scholars who wish to relate knowledge to policy, the rise of think tanks, an increased analytical capacity within government, and a political environment marked by a resurgent "fervor for capitalism" (p. 174) and by a politics driven by ideological contention. In the end, however, there is no confident explication of the determinants of the quantity and quality of knowledge talk informed by social science in the presidential arena.

In summary, all students of the presidency will be interested in these books. Each makes important contributions to how the personal preferences of presidents mix with institutional characteristics and contextual political forces in the uses of presidential power.

University of Tennessee, Knoxville DAVID M. WELBORN

Agendas and Instability in American Politics. By Frank R. Baumgartner and Bryan D. Jones. Chicago: University of Chicago Press, 1993. 289p. \$47.50 cloth, \$14.95 paper.

Those who follow public policymaking in the United States long have suspected that the dynamics they observed never seemed to match up to the laws embodied in the discipline's dominant models. Incrementalism reigns, but dramatic and deep changes occur rather often. Policy subsystems are inherently stable, shaped by differential intensity among interests and buttressed by institutional biases; but they erode—or collapse entirely. And of course, the system tends always toward

general equilibrium, though many have observed apparent and frequent disequilibria. In short, scholars ask, Which conditions are the norm and which are the exceptions?

Baumgartner and Jones have produced the book that students of public policy have been awaiting and that more than a few will wish they had written. In this rich work, the authors show how long periods of policy incrementalism and spasms of change are part of the same equation of issue definition and institutional bias; how policy subsystems both maintain stability but also can succumb to powerful countervailing forces; and finally, how the search for some general equilibrium probably is fruitless. What exists, instead, is a "punctuated equilibrium" of subsystem stability and change. Amid seeming stability, there is continual change. In fact, they note, for a political system long regarded as structurally and politically conservative, "one is struck by the vast policy changes that have occurred in the United States" (p. 235).

The authors come to these conclusions honestly. First, this book is based firmly on, and is heir to, a long line of scholarship going back at least as far as Schattschneider (1960), who, it seems, garners more respect with each passing generation. Its theoretical base is built upon the literatures on problem definition and agenda formation, policy subsystems, and (in a very nuanced application), social choice theory. These traditions are woven together in subtle and sophisticated ways, and the first several chapters alone would make this work a worthwhile read for any core graduate seminar on public policy processes.

Second, the authors back up theory with a methodology that is both commonsense and (even more important) readily reproducible. They start initially from a base of solid longitudinal case studies on issues like nuclear power, pesticides, smoking, and urban policy (among others). These cases, taken individually, show long periods of policy subsystem stability interspersed by sharp spasms of change. Taken together, these cases suggest the impact of broader systemic forces on policy subsystems, since so many of these subsystems encounter trouble for roughly the same reasons at about the same times. The authors build from these cases with empirical analyses of media sources, interest group mobilization, governmental attention and action, and policy outputs (with methods and data sources explained more fully in a useful appendix). The result is a truly comparative, empirically supported analysis of several issues across time—a rare commodity indeed.

No one of the authors' findings may surprise those who follow this literature closely. Issue definition and control over the agenda for debate are critical to policy stability or change. Institutions and institutional design can buttress the policy status quo or can offer opportunities to new voices. Policy entrepreneurs exploit their opportunities to define issues and gain access to sympathetic institutional venues and seek to keep others from doing likewise. The ways in which competing elites and interests seek to involve the "apathetic" in a conflict powerfully affects its outcome. Schattschneider, for one, said pretty much the same things over 30 years ago. What is important is how Baumgartner and Jones back up what have become truisms with solid analysis and how they extend the literature into new theoretical directions. The perceptive discussion about the interactions between policy "images" and institutional "venues" alone should keep more than one doctoral student busy for a while.

Perhaps the single most illuminating theoretical finding in this work is what the authors call the dual mobilization thesis. The mobilization of new voices and previously excluded interests either can come during a wave of enthusiasm for a particular policy (the cycle of "alarmed discovery" and "euphoria" about problem solution explored by Anthony Downs) or out of opponents' criticism of, and attack against, the policy status quo (as suggested by Schattschneider's notion about the "socialization" of conflict). But each type of mobilization produces a far different outcome. A "Downsian" mobilization is marked by a positive policy "image" (e.g., "peaceful" uses of nuclear power) and by the creation of institutions likely to support subsystem hegemony (e.g., the Atomic Energy Commission). A "Schattschneider" mobilization, by contrast, involves negative policy images (e.g., the health dangers of nuclear power) and the greater policy role played by institutions (e.g., congressional oversight committees) less likely to offer unalloyed support for the status quo. There are, in politics, two very different kinds of mobilization; and which one dominates at any given point explains convincingly whether a subsystem is or is not stable—hence, punctuated equilibrium, or stability and change.

There is so much offered in this book that any one review may not do it justice, so read it yourself. It is one of the more important books in years, a powerful and well-supported rejoinder to narrowly mechanistic and deterministic policy models. It reminds us that ideas, institutions, and (yes) politics all matter. Its lessons to students of public policy are several: look long (into history), look broadly (beyond the narrow confines of a single institution), look deeply into issues, and compare across issues and across time. It is at once a grand synthesis of the past and a pathbreaking work against which future studies will be measured. In these ways, it is a worthy heir to a long tradition.

Agendas and Instability in American Politics is well written, particularly for a work where the hands and souls of both authors are evident. Any quibbles about the book (a couple of the later chapters fit in a little awkwardly) are minor. This book is recommended especially for graduate courses in American and comparative politics, public policy, and interest groups, among other topics; but it also is accessible for upper-level undergraduate offerings.

Northeastern University

CHRISTOPHER J. BOSSO

Careers in City Politics: The Case for Urban Democracy. By Timothy Bledsoe. Pittsburgh: University of Pittsburgh Press, 1993. 231p. \$39.95.

Ambition and Beyond: Career Paths of American Politicians. Edited by Shirley Williams and Edward L. Lascher. Berkeley: Institute of Governmental Studies Press, 1993. 260p. \$21.95 paper.

The literature on political careers, writes Timothy Prinz in Ambition and Beyond, encompasses the fields of "political recruitment, political elites, personality and politics, political biography, legislative studies" (p. 11). But from the books under review, it is clear that as "disparate and far-flung" as this literature is (ibid.), one theoretical approach dominates. Joseph Schlesinger's

ambition theory looms large both in Williams and Lascher's collection of essays and in Timothy Bledsoe's study of city politicians. These books offer us an opportunity to see how Schlesinger has influenced our understanding of American political careers.

Ambition theory states that officeholders' ambition for higher office shapes their behavior. Most politicians hold progressive ambition (the desire to move to higher office), rather than static ambition (to remain in their present office) or discrete ambition (to leave office). Schlesinger's work suggests three broad areas of inquiry into differences: ambition, opportunities, and the behavior of politicians in one institution whose sights are set on moving to another. But the "key component," writes Prinz, is the structure of opportunities, or "the seats available and the hierarchy of positions for advancement," which "give shape and definition to the political career" (p. 27).

Although they do not follow a common format, the Williams and Lascher essays fit together well. The first section includes an introduction by Shirley Williams and the literature review and prospectus by Prinz. Williams offers comparisons between political careers in the United States and Britain. Prinz lays out the three stages of political careers discussed in the essays: recruitment, advancement, and retirement. The book includes sections on congressional careers (Linda Fowler and John Hibbing), subnational legislative careers (Peverill Squire and Edward Lascher), and careers for women and minorities (Susan Carroll and Fernando Guerra). The essays are drawn together by Lascher's introduction to each section and his concluding essay summarizing the critiques of ambition theory offered by the other authors.

While some of the research here can be found elsewhere, it is kept fresh by the authors' efforts to use their findings to speak to ambition theory. Hibbing, for example, uses his research on the internal congressional career to show how ambition theory actually misses a great deal of ambitious behavior. Ambition theory tends either to assume that all politicians have progressive ambition or to infer ambition from behavior, that is, that those who run for higher office have progressive ambition. This is limiting, however, because "progressive ambition can just as easily be displayed by running for Party Whip as by running for the Senate" (p. 131). Focusing on a predetermined hierarchy of offices and office seeking as an outlet for progressive ambitions misses how ambition may be satisfied by position seeking within an institution. The critical question, Hibbing argues, is "why some members vent progressive ambitions by employing the many ladders within the House, while others do so by attempting to jump to a different institution" (p. 131).

Linda Fowler's essay on recruitment demonstrates the need to appreciate how ambitions interact with opportunities and institutional characteristics. A puzzling aspect of recruitment in an era of candidate-centered campaigns, she writes, is "why individuals who could be the strongest candidates... never think about running for Congress at all" (p. 89). Restricting ourselves to those who want to move on means we will miss many of the ways politicians satisfy their ambitions. For example, ambition may be more satisfactorily pursued in some local and state governments than in others. Fernando Guerra finds that in the Los Angeles County hierarchy of elective offices, many local offices rank above a seat in the House. We might easily misconstrue the relationship

between ambition and career choices by politicians from this area without recognizing both how the hierarchy of offices differs from other areas and the special appeal of retaining a local office. Peverill Squire shows that state legislatures can be set up to offer satisfactory political careers in their own right and that legislators in many states are willing to stay there.

The emphasis in these essays on differences in opportunities across states, localities, and institutions has its payoffs. Guerra, Fowler, and Carroll each demonstrate the value of this emphasis to understanding the career paths of women and minorities. But with the exception of Lascher's contribution on job satisfaction in local governments, we learn little about differences in political ambition and even less on how ambition shapes behavior.

By drawing both on Barber and Schlesinger, Bledsoe offers us greater insights into these two aspects of ambition theory. From a panel survey of over nine hundred city council members interviewed in 1982 and 1987, Bledsoe looks at who seek city council positions and why, how city council careers unfold, and the future offices sought by city council members. Well integrated into the literature on careers and city politics, the analysis is empirically rich, using demographic data, motivations, and council characteristics to explain city political careers. Motivations are measured along four dimensions, the most important of which is from politico to apolitical. *Politicos* are progressively ambitious people who enjoy politics and call themselves politicians.

Bledsoe's research is premised on the belief that city politics need revitalizing. His goal is "to shed light on the means to attract skilled politicians back into city politics" (p. 8). Presenting a counterargument to termlimitation activists, he calls for city governments made up of ambitious, experienced politicians willing to serve long terms. Testing the heart of ambition theory ("the consequences of some people having aspirations for higher office"), he finds that upwardly mobile city councillors had "an increased constituency awareness, an increased constituent attentiveness, and an increased level of energy" (pp. 148, 170). While his data offer a way of looking at how ambition shapes behavior without assuming ambition from subsequent office-seeking behavior, he measures aspirations based on whether council members sought higher office. But at other points he does find interesting differences among motivational types, differences that support the need to bring more career oriented politicians into city politics. To those who would oppose careerist legislators, he responds that "the most that might be feared from lengthy legislative service is the development of political skill

and governmental knowledge" (p. 181).

Bledsoe is concerned that city politics are not good places to launch a career for higher office and that most offer unsatisfying careers for their members. In sharp contrast to Hibbing's findings for members of Congress, Bledsoe finds that most people do not change much in how they approach their jobs or constituencies during their time on councils. Further, data he uses from Schlesinger show that city governments were never, in this century, good paths to higher office. Nonetheless, the "solution to political revitalization of cities" is to reform councils so they attract career politicians (p. 175). These include longer terms, the removal of limits on successive terms, and higher salaries. "If city politics can

attract strong political talent, then that talent," he asserts, "will in turn excite the people" (ibid.).

Not all of Bledsoe's conclusions are convincing. First, it is not clear what Bledsoe means by "revitalization." The problems with city politics are not concretely identified with reference to any earlier period when politics were somehow better. Further, city councils have rarely been a means to higher office, so it would seem that far more than tinkering would be required to change this pattern. Second, he argues that politicos are better councillors and that we should try to attract more of them. Yet from his data, they do not appear to change much once in office, so it is not clear why we should expect them to develop political skills and knowledge. Finally, he may not give sufficient weight to how the types of issues in city politics attract particular types of people. While he notes Paul Peterson's argument about how city interests constrain policymakers, he argues that this "simply puts a premium on creative and talented political leadership" (p. 17). Yet his own data show a preponderance of business people and preoccupation with the city's economic vitality. Whatever the reforms, it may never be possible to attract the broad spectrum of progressively ambitious people that he seeks in city politics. Indeed, this, more than any other factor, may explain why city councils have never been routes to higher office.

Pennsylvania State University MICHAEL B. BERKMAN

Follow the Leader: Opinion Polls and the Modern Presidents. By Paul Brace and Barbara Hinckley. New York: Basic Books, 1992. 243p. \$25.00 cloth.

Media Polls in American Politics. Edited by Thomas E. Mann and Gary R. Orren. Washington: Brookings Institution, 1992. 172p. S28.95 cloth, \$10.95 paper.

Is the recent, media-driven proliferation of public opinion polls a blessing or a curse on American democratic politics? Thomas E. Mann and Gary R. Orren, editors of Media Polls in American Politics, and Paul Brace and Barbara Hinckley, authors of Follow the Leader: Opinion Polls and the Modern Presidents, provide similar answers to this question: media polls can be both blessing and curse, but the present situation often favors the latter. After carefully detailing various problems associated with, or caused by, such polls, both books offer encouraging and sometimes helpful advice on how we (academics, pollsters, journalists, public officials, and citizens alike) can turn the tide and make public opinion polls more valuable and useful to democracy. Media polls are not going to go away, so we might as well make the best of them. The first step, the authors suggest, is for everyone to become more informed and thus more realistic about what the polls mean, what forces influence the polls, and what impact the polls have on American politics.

The Mann and Orren volume brings together polling experts from both academic and news media organizations to discuss the use and misuse of media polls. The editors frame the discussion around two questions: (1) What are the strengths and weaknesses of media polls? and (2) What impact do media polls have on politics? The ensuing debate, which (not surprisingly) finds the academics on the offensive and the media pollsters on the defensive, focuses mainly on the first

question. The second question is usually addressed only indirectly. The underlying and occasionally recognized argument in each chapter is that polls foster, rather than subvert, democracy when they accurately and honestly reflect, rather than create or distort, public opinion.

Although relegated to the fourth chapter, Henry E. Brady and Gary R. Orren's essay, "Polling Pitfalls" sets the agenda for the debate. Most other chapters can be read as responses to the powerful critique they offer. Mixing survey research with journalism, Brady and Orren argue, is like trying to mix oil and water. Each has an "ethos" or set of priorities that sets the at cross purposes (p. 56). Journalists' primary mission is to produce novel, interesting, and easy-to-understand stories as quickly and as cheaply as possible. Survey researchers and statisticians, on the other hand, are a very conservative bunch. They must be certain of the validity and reliability of their measures before they will venture even the most qualified conclusion. The result of this "inherent mismatch," the authors assert, is that media pollsters and journalists are not only more likely than academics to commit methodological and conceptual errors in their surveys, they are also more likely to ignore them (ibid.).

Brady and Orren identify and skillfully explain three types of errors found in survey research and especially in media polling: (1) sampling errors, the most troubling of which is low response rates, (2) measurement errors produced by bad questions or "bad arrangement of good questions," and (3) specification errors (p. 59). The last are not the specification errors familiar to econometricians, however. Brady and Orren's specification errors refer to faulty assumptions about the nature of public opinion that affect the kinds of questions asked, how the questions are asked, and how the answers are interpreted. The most common and troublesome of these assumptions is that people have a single, unambiguous, unambivalent and stable opinion on any given public policy issue or public figure. Overall, this discussion of the errors that plague all sorts of survey research provides a very useful methodological guide for survey researchers and users, whether they be journalists, readers, scholars, or students.

The media-based contributors to *Media Polls* are all too aware of the time and monetary constraints that fierce competition places on journalists. Yet in their view, the consequences of these constraints are much more benign than Brady and Orren would have us think. Kathleen A. Frankovic (CBS News), for example, argues that journalists have responded to the demands placed upon them by becoming innovators in survey research techniques. In particular, she attributes the development of exit polling, computer and telephone interviewing technology, tracking polls, panel surveys, and focus groups to media polling organizations. Partisan though she may be, Frankovic provides a useful discussion of advantages and limitations of these polling innovations.

Many of the methodological issues that Brady and Orren consider serious problems, Michael R. Kagay (New York Times) considers "legitimate variations in techniques," which fall within "the range of acceptable polling methods" (p. 104). Differences in efforts devoted to increasing response rates, for example, are "subtle differences between equally well conducted polls," not differences in methodological validity (pp. 105–6). Kagay also points out that the competitive forces and constraints to which the news media are subject have

fostered the proliferation of national polls. In the pluralistic tradition, he welcomes this development, for it allows us to examine a preponderance of public opinion data on any given topic and frees us from having to rely on the results of just one or two polls.

E. J. Dionne, Jr. (Washington Post), on the other hand, acknowledges many of the criticisms against the proliferation of polls. He agrees that instead of helping citizens make informed decisions and fostering democratic deliberation, journalists who rely on polls encourage passivity among their citizen—readers by portraying the political system in an overly simplistic and deterministic fashion. Nevertheless, he claims, these problems are not the fault of media polls themselves. Rather, they are the result of the decline of political parties in the United States and the simultaneous rise of nonpartisan journalism. In effect, the news media have had to assume the responsibility for identifying viable candidates for public office. The polls simply allow the media to fulfill this role in as neutral and apolitical a way as possible.

Most contributors to Media Polls in American Politics offer suggestions for making polls more conducive to democratic politics. These recommendations, which are well summarized by the editors, range from the very practical and doable request that journalists fully disclose information about their sampling techniques, response rates, and survey methods to the quite idealistic notion that media pollsters should "familiarize themselves with theories of opinion formation and methods of multivariate analysis" (p. 16). Other recommendations call for journalists to replace their current superficial or nonexistent analysis of polling data with more sophisticated, in-depth analysis. These recommendations, while valid, are somewhat limited. First, they address only media personnel. Do academics, politicians, and ordinary citizens have no responsibility or recourse here? Second, the possibility that the media will take these recommendations seriously seems remote precisely because of the various structural constraints that Brady and Orren argue make survey research and journalism incompatible.

While Mann and Orren approach their topic from the point of view of polling experts, Brace and Hinckley address the democratic implications of the proliferation of opinion polls from the perspective of political institutions. Moreover, Follow the Leader provides a more focused and in-depth approach to the larger question by concentrating on one particular political institution (the modern presidency [Truman to Bush]) and one particular aspect of polls (presidential approval ratings). To what extent, Brace and Hinckley ask, do these monthly reports on the presidents' popularity affect presidents' behavior in office, and to what extent do presidents' activities affect their subsequent approval ratings? And what does this say about the strength of democratic linkage between American presidents and citizens?

Brace and Hinckley's careful analysis does give us reason to worry. Concerns about maintaining or boosting presidents' popularity have played a significant role in White House decision making. Presidents do, indeed, appear to undertake certain activities in reaction to recent trends in their approval ratings. Specifically, presidents' major domestic policy addresses and foreign travel are "curiously timed to accord with month-tomonth changes in approval ratings" (p. 55). Many of the modern presidents' activities in foreign affairs also ap-

pear to be "curiously timed." The use of military force abroad, foreign policy addresses, and international "rally points" (dramatic, or dramatized, events that include—but are not limited to—the use of force) are more likely to occur in the immediate aftermath of negative, dramatic domestic events (e.g., White House scandals) or amid worsening economic conditions. The authors suggest that these foreign policy activities are preemptive strikes taken to prevent a drop in the presidents' approval ratings that would otherwise follow such negative events.

The problem with all this, Brace and Hinckley explain, is not that presidents are concerned about public opinion but that they are concerned about their popularity at the expense of good public policy and real public opinion. Decisions and actions based on what is popular are not necessarily in the best interest of the nation; they may even conflict with what the public actually wants, for presidential popularity as measured in the polls does not tend to reflect the public's policy preferences. Public relations presidents, of which Reagan and Bush are prime examples, are particularly adept at manipulating events to make themselves appear to be concerned about, and in line with, public preferences, when they are, in fact, not. Public opinion in this scenario is not being used by presidents as a guideline for policy decisions, it is being used only for the benefit of the presidents themselves.

Not all modern presidents have been so concerned about being liked. As Brace and Hinckley's analysis demonstrates, Truman, Johnson and Ford were more willing than others to place their convictions, or at least their policy goals, before their popularity. Not surprisingly, these three received the lowest average approval ratings. It is also no coincidence that these unpopular presidents are the very same ones who were not elected to the presidency. Clearly, presidential campaigns and elections—crucial elements of the democratic process itself—foster the public relations mindset.

At the same time that Follow the Leader "underscores the worry that the polls turn democracy upside down" (p. 16), it also provides plenty of reassurance that there are limits to what presidents can do about their standings in the polls. In fact, Brace and Hinckley find that most of the activities presidents undertake to boost their approval ratings are futile: they have no discernible effects on subsequent polls—much less, positive effects. Even the use of force will not raise a president's popularity unless the White House puts forth considerable effort to dramatize the event and make it a "rally point." The authors also show that presidential approval is largely a function of economic circumstances and dramatic events that are, for the most part, beyond the control of any individual president. In addition, presidents are set up for failure. They cannot possibly maintain the high approval ratings with which they enter office, for those scores are based on unrealistic expectations that American culture, citizens, and academics place on the presidency. Modern presidents concerned about their popularity may find that the polls control them more than they control the polls. In either case, however, democracy is not well served.

Brace and Hinckley's solution to the problems of the public relations presidency is twofold. First, we should lower our expectations regarding the level of support an incumbent president should receive. A more realistic expectation would be that presidents maintain the same margin of support that elected them in the first place. The White House could then take more policy risks and focus less time and effort on manipulating public information. Second, everyone needs to read this book. Were this to come to pass, presidents would stop wasting their time trying to maintain or increase their popularity, citizens and the media would not hold presidents accountable for events and conditions beyond their control, and (most importantly) Americans would no longer be so easily duped by clever and manipulative presidents and White House staff.

Luckily, most everyone can read this book and get something out of it. The analysis is methodologically sophisticated but presented in a very user-friendly manner. For the typical undergraduate or casual reader with little or no knowledge of (much less, interest in) statistics, Brace and Hinckley simply and effectively explain their analysis and present their results in English, in the main text. For the typical graduate student or professor, they explain the statistical procedures and present the results in much greater detail in the appendices. Overall, Follow the Leader is a methodological delight. The analysis (either version) is extremely careful and straightforward. The ways in which Brace and Hinckley operationalize concepts and collect data, especially with regard to their vast array of indicators of presidential activities and dramatic events, are remarkably creative and ingenious.

In contrast to the accessibility of Follow the Leader, Mann and Orren's Media Polls in American Politics is an "insider's" book. Both the general subject matter and the oftentimes technical discussions make it appear to be written for other conscientious journalists and media critics. For the reasons mentioned earlier, the Brady and Orren chapter is exceptional is this respect. Both books, however, address a very important and pertinent set of issues surrounding the proliferation of public opinion polls. Because these issues address the viability of modern-day democratic politics and the linkage between citizens and elected officials, they deserve the attention of said citizens and elected officials, as well as that of scholars and pollsters.

Emory University

BETH REINGOLD

Power Failure: New York City Politics and Policy Since 1960. By Charles Brecher and Raymond D. Horton. New York: Oxford University Press, 1993. 393p. \$35.00.

A Phoenix in the Ashes: The Rise and Fall of the Koch Coalition in New York City Politics. By John Hull Mollenkopf. Princeton: Princeton University Press, 1992. 285p. \$35.00.

These two fine books provide rich, original detail about New York City and contribute to broader issues of urban politics and administration. The Era of Urban Austerity began with the 1975 New York fiscal crisis and continues today. Why and how political leaders then ended some three decades of government growth has sparked much debate. Four answers—two economic and two political—are commonly advanced: (1) capitalism strangled America's most generous local welfare state; (2) decline in jobs and population, exacerbated by growth in the Sunbelt and abroad, devastated the economic base and in turn suppressed government spending; (3) Washington, especially under Presidents Ford,

Reagan, and Bush, cut grants, the lifeblood of New York's social welfare programs; (4) finally, there is the regime theory of Clarence Stone and others, which stresses the constraining force of business elites. The interest in these two books is that they review carefully these common views but disagree with all of them. Both books build extensively documented analyses stressing specifically political decisions. They thus extend the subtradition of work stressing political forces, from Ken Auletta and Ester Fuchs, among others.

Mollenkopf's Phoenix in the Ashes considers the Koch era. It speaks to the general interpretative issues by showing how dramatically different policies emerged under the same mayor. When Koch first took office in the wake of New York's fiscal crisis, he imposed major cutbacks that his predecessor Abe Beame had considered "impossible." But by 1980, Koch led a major turnaround: New York's spending started growing and continued through the 1980s, despite Republicans in the White House. What Mollenkopf contributes is a detailed interpretation of how and why such policy shifts occurred. His answer stresses changes in the leading groups comprising the Koch coalition. Koch initially ran on a strong antiunion, antibureaucracy platform and appealed to "average citizens," stressing lower taxes and increased productivity. But over the years, Koch moved away from his traditional liberal-left political base in Greenwich Village and upper Manhattan to build a new coalition incorporating Latinos and white Catholics outside Manhattan. With the old Tammany-DeSapio Democratic organization no longer a serious threat, he incorporated many regular Democrats, who, along with certain developers and finance firms, contributed generously to his campaign funds. Most controversial, however, was his increasing exclusion of low-income African-Americans, which eventually led Koch to be labeled a racist and David Dinkins to replace him. Thus, while Mollenkopf recognizes demographic and economic constraints, "astute political entrepreneurs" occupy the driver's seat. There is much discussion of campaign strategies, governing, and specifics of leadership. In many such instances, Mollenkopf thoughtfully raises broader questions of interpretation and assesses them with brief illustrations from other major cities (e.g., Why and how did Boston, Chicago, and Los Angeles go one route, while Houston and Atlanta took another?). These quick comparisons generally bolster the case that Koch as a leader made specific decisions that led New York to pursue its own unique route, largely flowing from the shifts in the groups he brought into his coalition. Mollenkopf still finds room for serious attention to, and empirical detail on, shifts in New York's economy in the postindustrial era—and many details on ethnic voting patterns.

Brecher and Horton argue against the same four common interpretations of urban politics as does Mollenkopf. Their answer is similar to his: political leaders are key. Yet despite this common answer, the two books are quite distinct. Brecher and Horton's *Power Failure* is "a guidebook" to New York City politics and government over three decades. It provides a longer historical purview, more detailed budgetary and fiscal analysis, attention to specifics on service delivery and government productivity, and many suggestions of specific reforms. These are natural outgrowths of the two-decade-long involvement by the two authors in monitoring and assessing New York City government,

generating a shelf of publications that have made them leading policy analysts of the city. Their goal here is to update the classic on New York's politics, Wallace Sayre and Herbert Kaufman's Governing New York City (1960). They consider that New York City politics is less open and competitive, less pluralistic, than do Sayre and Kaufman. But they offer an essentially pluralist interpretation in that no single elite dominates decisions. Like Dahl, they stress issue-area differences: the teacher's union may dominate much of education, and the Policeman's Benevolent Association could defeat proposals for a civilian review board by civil rights groups and leading politicians, but land use is more dominated by real estate owners and developers (pp. 8-9). Clearly, the average citizen or minor-group leader may count little. Indeed, they stress that bureaucratic leaders in these various issue areas have surrounded themselves with pressure group allies that make political access by others sorely difficult. Many of their reform suggestions concern ways of seriously introducing more general criteria like value for tax money, since attention to such criteria often run counter to entrenched interests. This is clearly a work by two veterans of political wars; dozens of their past battles are distilled here. The level of detail sometimes overwhelms the reader of Power Failure; but if 393 pages are weighty, this is still just about half the length of Sayre and Kaufman.

Mollenkopf's focus on Koch and his political coalitions provide an organizing theme that makes his book narrower but more readable. He makes a useful conceptual distinction between campaigning coalitions and governing coalitions, each of which has its partially distinct dynamics. Koch railed against the unions as a first-time candidate but, once in office, grew closer to them. Brecher and Horton's review of five administrations from 1960 to the present leads them to add much more detail. They provide many specifics, from the quality of refuse collection to detailed breakdowns of outlays on psychiatric inpatient care. Such detail about trees—and leaves—occasionally obscures the forest. Still, for students of New York city government, such detail makes this the new definitive "handbook."

In assessing interrelations between economic and political forces, Mollenkopf chooses a wide variety of specific decisions that he briefly summarizes and interprets. There are no detailed "cases," however, as in Dahl or Banfield. Mollenkopf's conclusion is that "political considerations had a far greater impact on the actions of the Koch coalition than any supposed contextual imperative to promote private investment" (p. 161). Through skillful use of extensive, historical data, Mollenkopf traces the sources of Koch's popularity, the strength of his governing coalition, and the roots of his demise. Still, Mollenkopf has some difficulty explaining the demise of the Koch coalition. It is unclear why Koch's efforts to maintain tensions within his opposition suddenly failed. The role of local economic conditions in the Koch coalition also remains obscure. How he specifically chose a prodevelopment agenda once in office and 'growth politics" was distinct in New York City could be more sharply addressed.

For Brecher and Horton, the task is to demonstrate whether shifting political alliances, a changing local economy, or fluctuations in the level of intergovernmental aid are responsible for policy variations. Comparing changes in budget outlays with changes in these three independent variables, they conclude that local political

actors were the most important factor in three of their four time periods. The problem here is that they provide little or no discussion of specific political actors and decisions that they undertook to influence policy.

Differences between the two books stem from different goals. Brecher and Horton are fascinated with the Big Apple. While their analysis has wider implications, their main goal is to provide an encyclopedic guide to New York City politics and government, which they do with considerable detail, ingenuity, and success. For reformers, however, Brecher and Horton lament that on the two criteria of responsiveness and efficiency, New York fails again and again. From an electoral process that is neither competitive nor participatory to a budgetary process and a service delivery system that suffer widespread inefficiencies, Brecher and Horton highlight the power failure of New York's government. This contribution is diminished by a dearth of attention to specific political decisions. Even though a policy option may be more efficient, it may have negative distributional repercussions or other politically important implications. Who supports which criteria of reform are not as central questions as they might be. Consequently, the reasons for lack of support for their reform suggestions are often vague. They seldom mention race; but it is central for Mollenkopf. He is far more concerned with the political costs and benefits of a given policy; and for him, a policy is successful if it generates greater political support. Obviously, one must read both books.

University of Chicago

TERRY NICHOLS CLARK

The Phantom Respondents: Opinion Surveys and Political Representation. By John Brehm. Ann Arbor: University of Michigan Press, 1993. 266p. \$39.50.

In this slim volume (fully 50 pages of the already modest length comprise technical appendices), John Brehm presents a broad and detailed empirical analysis of the effects of nonresponse on survey results and traces out the profound implications of that analysis.

Brehm first explores the notion of representativeness in both its statistical and political senses. (In this connection, I miss a reference to the four papers by William Kruskal and Frederick Mosteller published in the International Statistical Review in the late 1970s that thoroughly explore the definitions and usage of the term representative.) He argues that polls pervade political life, influencing the allocation of funds, coloring political discourse, affecting political campaigns by both shaping emphasis on issues and influencing contributions (and perhaps even turnout), and supplying data on which academic theories of politics are tested. But more important, he suggests that surveys present a "unique opportunity . . . to compensate for inequities elsewhere in politics' as they bring the voting booth to respondents' front doors or telephones and hence can potentially increase the representativeness of the political system (p. 18). To the extent that there is nonresponse, representativeness in both senses is compromised. Brehm's aims for the book are twofold: he develops and tests a model for survey nonresponse, and he assesses how nonresponse affects scientific research.

But first he analyzes who is missing in the General Social Survey (GSS) and National Election Studies (NES) from 1978 to 1988, interestingly enough using the Current Population Survey, rather than the census, as the gold standard. The finding-that blacks, women, the elderly, the poor, and the less educated are overrepresented in academic surveys and that men, youths, whites, and the wealthy are underrepresented-is initially cheering in the context of surveys providing political access for groups typically underrepresented in more conventional political arenas. But Brehm points out in a later chapter that it is likely to be atypical members of the conventionally underrepresented groups who make up the overrepresentation in surveys-individuals who are more like those in the usually well represented groups.

Brehm's next step is to use data collected by NES, GSS, and the Detroit Area Study on reluctant respondents and nonrespondents to test his model of survey participation. He uses probit analysis to model contactability and capability of being interviewed—the first two stages of the participation process. But the emphasis is on modeling respondent compliance once contact has been made, postulated to be based on four relationships for the potential respondent: relationship to strangers (Will I talk to an interviewer?), relationship to the interviewer (Will I comply with this particular interviewer's request?), relationship to the interview (Does the idea of being interviewed appeal to me from my past experience or current circumstances?) and relationship to self-image (Am I interested in helping out?). After examining each of these relationships in turn, Brehm incorporates them into a structural model of compliance, pointing out that he has, at best, weak indicators of the underlying concepts and that using those indicators requires believing that the reasons given for participating or declining to participate are, indeed, the real reasons for the behavior. (He points out, for example, that "I don't have the time" may be merely a polite excuse. In addition, cognitive psychologists have taught us that individuals do not necessarily have privileged access to the reasons they do the things they do.) Nevertheless, Brehm finds a good fit for his model across several data sets, with the relationship to the interview being the dominant factor determining compliance. Here and later, Brehm argues that the message for survey researchers is to make their interviews more interesting and inviting to respondents in order to bolster response rates.

Brehm goes on to show how nonresponse can bias univariate and multivariate results of surveys and to carry out a Monte Carlo simulation of selection bias. I find this section to be the least carefully explained and the most difficult to understand of all the material in the book. But he makes a strong and telling argument against weighting as the solution for nonresponse. First, members of an underrepresented group who do participate in the survey are likely to be atypical of the group as a whole; unduly weighting their responses will only compound the bias. Further, weighting by likelihood of participation is shown, in the simulations, to decrease the measured strength of relationships. Instead, Brehm advocates correcting for nonresponse, using strategies of modeling the selection process as proposed by Heckman and Achen. He proposes three models of the sample selection process and applies them to four broad classes of models that political scientists often estimate with survey data. Indeed, taking advantage of the fact that political scientists so often use data from NES, he reanalyses earlier studies on political participation, vote choice, candidate evaluation, and policy position, incorporating correction for nonresponse. He finds that the coefficients for most of the models change only slightly, leaving measures of relative importance approximately the same. But coefficients sometimes double upon correction, making for far different substantive conclusions, especially on issues of turnout and information. I find this section fascinating, but am somewhat concerned with the uncritical acceptance, without any comment, of the restrictive and sometimes dubious assumptions necessary for the selection models.

Finally, Brehm simulates what would happen if response rates were to drop considerably from their current levels by comparing results from NES as he successively drops out those who were the most difficult to contact or to convince to participate. His major finding is that if response rates should fall below about 30%, nothing, not even the modeling of nonresponse, could rescue the results of surveys. He closes with the plea to survey organizations to make their surveys interesting to respondents and to collect and disseminate the collateral information on respondents and nonrespondents that would make modeling of selection possible.

Brehm's work is careful and comprehensive. Hence, the lack of reference to *Incomplete Data in Sample Surveys* (1983), the three-volume report of the Panel on Incomplete Data of the Committee on National Statistics of the National Research Council surprised me. Brehm's volume provides a great deal of important analysis for survey researchers and political scientists to think about and to act upon.

State University of New York, Stony Brook Judith M. Tanur

American Political Cultures. By Richard J. Ellis. New York: Oxford University Press, 1993. 251p. \$45.00.

Do Americans possess a single political culture? Do they share the same basic beliefs about such vital political ideas as liberty, equality, property, and democracy? According to Richard Ellis, there is a tradition of thinkers, most notably Louis Hartz, who have answered these questions in the affirmative. Their various accounts, he tells us, have tended to focus on the pervasive "individualistic character of the American people" (p. 3). In contrast to this singular and rather ambiguous characterization, Ellis stresses the plurality of political cultures and the multiple meanings of individualism in America.

Drawing from the anthropologist, Mary Douglas, Ellis begins by distinguishing between two dimensions of individuation. Individual autonomy may be constrained either through what Douglas calls group or grid. "Group restrictions on individual autonomy can be measured by determining whether rules of admission to a group are strong or weak, and whether the elite support a group gives to its members is complete or partial" (p. 3). Grid restrictions, on the other hand, concern social regulations or external prescriptions. "At the high end of grid, interaction among individuals is strictly regulated and all members know their station" (p. 3).

Using a simple matrix, Ellis adduces four types of social life: competitive individualism, egalitarianism or communitarianism, atomized subordination or fatalism,

and hierarchical collectivism. The following table charts his fourfold typology:

	Low Group	High Group
High Grid	Atomization	Hierarchical
Low Grid	or Fatalism	Collectivism Egalitarianism or
Low Gila	Competitive Individualism	Communitarianism

Employing this typology, Ellis analyzes the history of the American experience. Against the view that American political culture is predicated on competitive individualism alone, he emphasizes the egalitarian or communitarian form of political culture. "Egalitarianism differs from individualism," he tells us, "in its effort to integrate the individual into a caring collectivity... Competition is revered by individualists, solidarity is privileged by egalitarians... Participation is the guiding star for egalitarians; self-regulation is the ideal of individualists" (p. 5). As examples of egalitarian or communitarian ways of life, Ellis analyzes the orientations to liberty, equality, property, and democracy proclaimed by proponents of such diverse movements as Puritanism, classical republicanism, and abolitionism.

Although much of Ellis' book is dedicated to examining the rival positions held by competitive individualists and communitarians, he dedicates a chapter each to the political cultures of hierarchy, fatalism, and what he calls hermitude. In his chapter on hierarchy, he binds together Virginia planters and New England Federalists. Employing his typology, Ellis suggests that the former would score to the right on the grid axis while the latter would score to the right on the group axis. In other words, Virginia planters tended to be more stratified and rank-conscious than the New Englanders while New England Federalists tended to be more conscious of the collectivity than their hierarchical counterparts in Virginia.

In his chapter on the various political and cultural orientations and responses to slavery in America, Ellis elaborates the idea of fatalism. "Fatalism," he writes, "is a learned response to a social environment in which there is only a tenuous connection between preferences and outcomes" (p. 135). According to Ellis, such a social environment is depicted by such scholars as Kenneth Stampp (*The Peculiar Institution* 1956) and Stanley Elkins (*Slavery* 1959), recounted in the Uncle Remus stories of Br'er Rabbit, and retold in the slave narratives of William Wells Brown and Frederick Douglass.

Finally, going beyond his fourfold typology, Ellis examines the life of the hermit, exemplified by Thoreau at Walden Pond, who journeys beyond (or at least outside) the horizons of both group integration and social regulation.

In his concluding remarks, Ellis reviews the two major theses of his work. First, he claims that his stress on rival political cultures contradicts the consensus theory of political culture in America that points to competitive individualism as the sole vision of the good life. Given that the scholarship on American political culture over the past 25 years has stressed the debates between such rivals conceptions as republicanism and liberalism, Ellis' contradiction of the consensus theory seems somewhat banal. Indeed, he even admits that this aspect of his book "will seem passe" (p. 152). In fact, a good portion

of his work is nothing more than a surface remapping of the already well articulated terrain marked between communitarians and individualists by such notable scholars as Hannah Arendt, Sheldon Wolin, John Homer (Jack) Schaar, Benjamin Barber, and Wilson Carey McWilliams. Well before Ellis, these thinkers clearly demonstrated the force of the communitarian challenge against the pervasive individualism of American life.

Ellis' second thesis concerns his use of a general theory and typology to stake out a middle ground between those who want to universalize a single notion of culture and those who believe that "every distinct time and place and race is endowed with a unique culture" (p. 156). Once again, there is nothing extraordinary about Ellis' thesis. Almost every thoughtful observer of political culture has staked out this same theoretical ground. Indeed, over a century and a half ago, Alexis de Tocqueville balanced a sensitivity to historical and social context with a general theory regarding the prospects of democracy. In Tocqueville's seminal work, each of Ellis' rival political cultures can be easily located. Tocqueville stressed communitarianism in his analysis of the New England township system; he analyzed competitive individualism in his remarks on "self-interest rightly understood"; he warned against social atomization and fatalism in his discussions of individualism and modern despotism; and he associated hierarchical views with both the burgeoning manufacturing aristocracy in New England and the established plantation economy in the South.

To say that there is little new in Ellis' book is not to say that it is without merit. His typology links diverse political traditions, historical movements, and cultural patterns of action together in often very insightful ways. In the end, it provides a useful and clear map of a road well traveled by those who have tried to come to terms with the complexity of American political life.

Kenyon College

MICHAEL BRINT

Bargaining with the State. By Richard A. Epstein. Princeton: Princeton University Press, 1993. 322p. \$35.00.

As most students of American politics and public policy will readily admit, the power of the federal government is constrained only in the most minimal sense by the Constitution. The emergence of a dense network of government programs providing benefits to citizens has created new means of exerting governmental power. The federal government often provides contracts, grants, licenses, and tax exemptions that increase the welfare of its citizens. However, access to these benefits may depend on the willingness of citizens to abide by specific rules and regulations. This, in turn, creates a means by which the state may exercise powers beyond established constitutional limits, particularly if citizens have but a limited capacity to bargain. Can the government make acceptance of public funding for the arts contingent on the subject matter addressed by the artist? Does the provision of a tax exemption to a religious organization allow the state to intervene in its religious practices should they prove discriminatory. Can the government provide employment only if individuals agree to waive their rights against unreasonable search-and-seizure? Questions of this type are at the heart of Richard A. Epstein's Bargaining with the State, in which he seeks to "explore the extent to which, as a matter of political and constitutional theory, it is proper to impose limits on the power of the state to bargain with its citizens" (p. 3).

Bargaining with the State begins with a comprehensive examination of governmental coercion and unconstitutional conditions. In part 1, Epstein seeks to explore the "bargaining risks" associated with government action and to determine what kinds of conditions the state can force on its citizens as a requirement for access to particular benefits. Epstein begins with the doctrine of unconstitutional conditions, that is, that "even if a state has absolute discretion to grant or deny any individual a privilege or benefit, it cannot grant the privilege subject to conditions that improperly 'coerce', 'pressure', or 'induce' the waiver of that person's constitutional rights" (p. 5). However, his goal is to refine this doctrine to determine how one might distinguish between legitimate and illegitimate demands.

Epstein begins by accepting that the proposition that some forms of property are best held in common and that the state has a legitimate role to play in managing particular resources. The best situation is to provide for that initial assignment of rights which allows subsequent adjustments to be made with the lowest level of friction and drag" (p. 39). While a system of private property rights and freedom of contract works well for most situations, one must exert great care when addressing the use of public property because of the monopoly power of the state. With respect to public policy, the key questions are two: "First, what techniques help insure the maximization of the social surplus from forced exchanges initiated by the government? Second, what rules determine each person's share of that surplus?" (p. 19). On the first question, Epstein merely suggests that although each situation must be examined individually, the answer will be tied to the need to minimize transaction costs. It is the second question that receives the greatest attention. Epstein argues that at bare minimum, a just compensation requirement is necessary, assuring that no one is worse off following the exercise of state power. However, the Pareto condition, when taken by itself, "is underinclusive because it leads to the conclusion that the surplus generated . . . may be allocated through political action in any way whatsoever" (p. 93). Epstein suggests that the best rule is to require a maximum improvement in social welfare and a pro rata division of the gain from government action, allowing subsequent private transactions, rather than public policies to redistribute incomes. The concern is that the state may often force individuals or groups to accept situations that are a Pareto improvement over the original state but suboptimal because they are tied to efforts to redistribute and administer social gains. Epstein thus concludes that we should adopt the best achievable state of affairs to judge policy, rather than the status quo ante, if our goal is to maximize the cooperative surplus from government action. As one might expect, one finds little support for redistributional schemes in this framework even if there is a surprising level of support for positive government

Epstein's key contribution is developed in part 1 of Bargaining with the State. It is careful and relatively

balanced in its examination of collective action problems and the problems associated with the state's exercise of monopoly power. The remainder of the book is devoted to an examination of a wide range of contexts in which the bargaining risks are present and questions of unconstitutional conditions emerge. This exploration leads Epstein through incorporation, discriminatory taxation, federalism, public roads and highways, land use restrictions, licenses and permits, employment contracts, tax exemptions, unemployment compensation, welfare benefits, and educational benefits. While Epstein's analysis of some of these topics (e.g., access to interstate highways, the provision of licenses) is only mildly interesting, the analysis of other topics (e.g., Medicaid funding of abortions, the provision of public monies for the arts, and tax exemptions for religious institutions) is truly fascinating. Despite the quality of these chapters, the book would benefit from a greater interplay between the theoretical concerns raised in part 1 and the policies and court decisions examined in the remainder of the book. The theoretical framework is so rich in its implications, yet much of it remains muted or absent from the 11 chapters that follow. Indeed, one can read part 1 and the remainder of the book as largely separate enterprises. A concluding chapter critically reviewing the theoretical discussion of part 1 in light of the subsequent cases would have been a partial solution to the lack of integration. Alas, this is not done. The book ends abruptly, with no more than a few paragraphs devoted to bringing the reader full circle.

In the final chapter, Epstein reminds the reader that "the power to contract and to grant, when lodged in the hands of government, may well prove to be as dangerous as the power to take and to regulate." Although analysts rarely subject government contracting to extensive scrutiny, Epstein is most certainly correct when he notes that "government and statecraft are always a high-risk enterprise, and bargaining by the state has to be watched as closely and with the same level of concern and suspicion, as taking, regulation, and taxation—the traditional forms of government power" (p. 312). In casting new light on this nebulous world of contracts, grants, subsidies, and tax exemptions, Epstein's book expands his audience's understanding of some of the tensions inherent in the modern welfare state. Despite the limitations noted, the early chapters of Bargaining with the State provide more insights than one commonly finds in a single volume. As such, this book should be read widely by policy analysts and students of the American political and constitutional system. Its insights are important, even if they remain somewhat underdeveloped.

Wesleyan University

MARC EISNER

Candidates, Congress, and the American Democracy. By Linda L. Fowler. Ann Arbor: University of Michigan Press, 1994. 240p. \$42.50 cloth, \$16.95 paper.

The stated goal of the University of Michigan Press's Analytical Perspectives on Politics Series is to publish works of a synthetic nature that provide "clear and discriminating descriptions of the current state of the art and a strong-minded prescription and structure for future work in the field." The latest contribution to the

series is Linda Fowler's study of candidates for Congress. I stress the raison d'être of the series to help prospective readers understand what this book does and does not provide. It is not intended to offer original research if that means new data and/or new analyses of old data. It is intended to provide both a creative synthesis of existing research and a strong-minded prescription for future research. In terms of synthesis, I would assign the book a grade of A+, nor would I have to agonize in doing so. In terms of prescription, I would probably assign a B- or C+.

The book reflects Fowler's impressive command of a large literature, as well as her thoughtfulness and insight. Readers would be hard pressed to spend time with the book without learning something. Its organization is driven by Fowler's belief that "the same fundamental question about who ends up on the congressional ballot" is at the core of a wide variety of debates within the discipline (p. 12). As such, she is not much taken with Downsian, party-based views of American politics, preferring instead to stress the candidate-centered nature of the modern era. She is not much taken with rational choice assumptions about goal-seeking behavior, preferring instead to stress the idiosyncrasies of individual candidates. Nor is she much taken with voter-driven models of the polity, preferring instead to see voters responding to, rather than creating, options presented by elite candidates. From Fowler's perspective, candidates are in the middle of not just elections but also representation and institutional change; and chapters on these three topics anchor the book.

As a synthesis, Fowler's work succeeds wonderfully, and this is not surprising since I can think of no one more qualified to survey the literature on congressional candidacy than Linda Fowler. This is not just a literature review but (as the series objective promises) a "creative and sophisticated" synthesis that actually suggests new ways of looking at the literature and new implications of that literature. I found many provocative insights in this engaging, broad-based review. She points out that the challenger quality literature arose as an attempt to explain how national economic conditions and other aggregate forces affect vote outcomes through strategic choices of candidates but that recently this literature has looked more toward local conditions than toward national forces as causes of candidate decisions. She points out that many studies of ambition are, in fact, studies of the opportunity structure masquerading as studies of ambition. And she points out that while institutional change could be due to the desires of members or to changes in the larger environment, neither of these explanations works well empirically.

But offering strong-minded prescriptions for future work is a much more difficult task than synthesis and it is this difficulty, rather than any failing on Fowler's part, that in all probability accounts for the lower mark on this component of the book. The prescriptions offered are sometimes difficult to find. The primary general pleas seem to be for more attention to challengers, as opposed to incumbents (reasonable, but not too specific), and for the explicit realization that congressional candidacy can be conceptualized as both an independent and a dependent variable (already done, though perhaps only implicitly).

A few of the more specific prescriptions may not be directly on target. For example, Fowler believes that one

consequence of the discipline's alleged preoccupation with incumbents "is an inability to identify the pool of prospective candidates" (p. vi). I fully agree that this inability is at the heart of dissatisfaction with the study of congressional candidates (it keeps us from doing more by way of comparing candidates and noncandidates), but the reason the pool has not been satisfactorily demarcated is that it is very hard to do-even without a preoccupation on incumbents. Similarly, Fowler does a splendid job of discussing the methodological problems inherent when candidacy is both an independent and a dependent variable, and a few vague references are made to two-stage least squares estimation procedures; but the real problem is obtaining a satisfactory instrumental variable that would then permit estimation. To date, none appears to be available. I applaud Fowler's ability to identify the problem areas so clearly and her proddings that we realize the complexity of the situation, but I worry that realization alone is insufficient for research to advance very far. The difficulty, in my opinion, is not a bias in the research agenda so much as a set of seemingly intractable problems.

Before closing, I should note that recent events could, in the eyes of some observers, leave the larger premise of the book open to question. The surprising resurgence of party voting in Congress, the policy constraints imposed by severe budgetary pressures, and the somewhat formulaic nature of modern congressional service could be seen as conspiring to reduce the importance of individual members of Congress, once in Congress, even as individual candidates for congressional office become more central to election outcomes. Why is it that the massive class of 1992, despite all the expectations that it would dramatically alter the face of the institution, was a dud? Efforts at institutional reform in the 103d Congress came to very little and were not led by the scores of self-proclaimed antipoliticians elected in 1992. One explanation could be that the process of institutionalization diminishes the autonomy and uniqueness of individual members because the socialization process and other forces turn them into company men and women. This is institutionalization in the public administration (rather than the Polsby) sense, so it is seldom employed by Congress scholars; but it represents a valid way of looking at the issue. When it is applied, term limits become a logical step in the evolution of the institution. If individual members do not matter that much because the institution has negated most of their uniqueness, just rotate interchangeable

Of course, if the truth be known, I do think individual members matter; but it could be that in addition to working a little harder at offering prescriptions, Fowler could defend the importance of individuals as members and not just as candidates. I am not sure this importance is quite as obvious as she believes. But these are mere interpretational quibbles and are generated by the stimulating nature of the book. They are by no means serious problems that should keep people from buying, reading, and assigning the book. It constitutes an erudite summation and critique of the literature on congressional candidacy—a sterling contribution worthy of much attention.

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JOHN R. HIBBING

The Constitution Besieged: The Rise and Demise of Lochner Era Police Powers Jurisprudence. By Howard Gillman. Durham: Duke University Press, 1993. 317p. \$34.95 cloth.

Conscience and the Constitution: History, Theory, and Law of the Reconstruction Amendments. By David A. J. Richards. Princeton: Princeton University Press, 1993. 295p. \$35.00 cloth.

The role of the judiciary and legal ideology in the development of political institutions is a central concern of the public law scholarship; and much of the research is interdisciplinary in combining law, history, philosophy, and political science. That concern is addressed in the books by Howard Gillman and David A. J. Richards, though in different ways and for very different purposes. Both works are also interdisciplinary in their approaches.

The Constitution Besieged reexamines the rise, in the late nineteenth century, of the Supreme Court's jurisprudence protecting economic rights under the Fourteenth Amendment's due process clause, as well as its eventual demise in 1937 amid the battle over Franklin D. Roosevelt's "Court-packing" plan and the Court's "switch-in-time-that-saved-nine." That period became known as "the Lochner era" because of its identification with the jurisprudence of a "liberty of contract" espoused in Lochner v. New York (1905), which struck down a state law setting limits on the number of hours bakers could work. Gillman, a political scientist, ably advances a revisionist interpretation of that era's jurisprudence.

The standard interpretation of the Lochner era takes its cue from one of the dissenting opinions in that case. Dissenting Justice Oliver Wendell Holmes brought the insights of American legal realism to bear on the majority's ruling in Lochner. His opinion embodies the principal liberal and progressive criticisms of the Lochner era. The majority was making, rather than interpreting, the law in its creation and enforcement of a substantive, yet unenumerated, "liberty of contract." The Court secondguessed legislatures and Congress, thereby becoming a 'super legislature." In Lochner and other cases leading up to the 1937 constitutional crisis, the Court ostensibly imposed its own conservative laissez-faire economic philosophy on the country. Substantive due process analysis and "judicial activism" were discredited by Justice Holmes. And they remain discredited in circles of liberal legalism, at least insofar as they apply to the judiciary's defense of economic interests.

Largely overlooked was another dissenting opinion in Lochner, filed by Justice John Marshall Harlan and joined by Justices Edward White and William Day. That dissent represents a rival theory of the Lochner era's jurisprudence. Justice Harlan considered the "liberty of contract" to be a vital constitutional guarantee, rooted in the Reconstruction Amendments and the antislavery movement's emphasis on self-ownership. However, as he reminded his colleagues, economic liberty remained subject to legislation aimed at promoting public health, safety, and the general welfare. Lochner's bare majority, in his view, mistakenly construed New York's law as a protectionist measure for bakery workers, rather than as a legitimate piece of health and safety legislation promoting the general welfare. And Justice Harlan marshaled empirical studies showing that working long hours in bakeries may indeed be injurious to workers health. In short, the majority in Lochner and its ilk got carried away in standing against protectionist legislation favoring one occupation or group over another.

Lochner's era and jurisprudence thus invites two rival interpretations. On the Holmesian view, Lochner represents the Court's purblind imposition of an economic philosophy that ran against the growing forces of progressivism. Alternatively, the Lochner era was a time of great class conflict and political struggles during which the Court tried to draw (not always successfully) a principled distinction between legitimate economic legislation promoting health, safety, and the general welfare and legislation that was invalid because it was deemed to advance the special interests of particular groups or classes.

Gillman makes a persuasive argument for the revisionist theory of the *Lochner* era. He does so, first, by examining the origins of police-powers jurisprudence in the early nineteenth century and placing it within the context of James Madison's analysis of factions, as well as the judiciary's enforcement of a standard of legislative neutrality during the Jacksonian period. Gillman then commendably links that analysis with postbellum Supreme Court rulings to establish a continuation of that tradition and legal ideology. Finally, the cases leading up to and following *Lochner*, along with liberal legalism's opposition to them, are analyzed within the framework of political and legal ideological conflict over the "social construction of reality" (p. 16). The bottom line, in Gillman's words, is

"a story not of the sudden corruption of the law and the judicial function, but rather of how the judiciary's struggle to maintain the coherence and integrity of a constitutional ideology averse to class politics was complicated and ultimately derailed by the maturation of capitalist forms of production. . . The Crisis in American constitutionalism that we associate with the *Lochner* era was triggered by the judiciary's stubborn attachment to what historical participants perceived to be an increasingly anachronistic jurisprudence, one that had lost its moorings in the storm of industrialization." (pp. 10–11)

The revisionist thesis advanced in *The Constitution Besieged* is not original. Historians, such as Charles McCurdy, David Gold, and William Forbath, among others, have taken a revisionist view of the *Lochner* era's justices, jurisprudence, and judiciary. Yet until Gillman's work, no scholar has developed that thesis in such a sustained and systematic fashion. In doing so, Gillman makes an important contribution to the literature. Nevertheless, readers may not be entirely convinced that Holmes's and liberal legalism's critique of *Lochner* was completely wrongheaded or that Gillman takes fully into account the extent to which a conservative political ideology played a role in the Court's rulings. In other words, Justices Holmes and Harlan each focused on different facets of the same social reality.

By contrast, Conscience and the Constitution is a very different sort of book. Richards, a law professor, aims to construct a rights-based jurisprudence that basically supports the liberal rulings of the Warren and early Burger Courts and would take constitutional law even further in the direction that Ronald Dworkin would push it. Lochner's specter still hangs over substantive due process, however. And as a liberal legal theorist, Richards must find an alternative constitutional basis for his political theory. He thus reclaims the Fourteenth Amendment's privileges-or-immunities clause, which

the Court essentially wrote out of the Constitution in the *Slaughter-House Cases* in 1873.

Richards grounds the protection of fundamental rights on the privileges-or-immunities clause by arguing that the antebellum radical abolitionists' advocacy of a Lockean respect for inalienable human rights imbues the Reconstruction Amendments. His reading of the history behind the privileges-or-immunities clause ostensibly demonstrates that the radical abolitionists' "once implausible interpretative attitude toward the Constitution of 1787 [became] the only plausible attitude toward the Constitution" (p. 146). The Fourteenth Amendment not only embodies a commitment to the protection of fundamental rights and the "dissenting conscience," according to Richards but also holds out "an interpretative challenge to each generation's creative powers of universal moral reason and imagination" (p. 147).

Of course, Richards is not the first to take the privileges-or-immunities clause seriously. Laurence Tribe, Sanford Levinson, and Clarence Thomas, among others, have sought to revive that guarantee. But Richards may be the first to advocate such a bold extension of that clause, along with the Fourteenth Amendment's equal protection clause. To be sure, Justice Hugo L. Black argued that the privileges-or-immunities clause was better suited than the due process clause for serving as the basis for applying the Bill of Rights to the states. Richards agrees. Still, he goes beyond that to argue that the clause is so open-ended in the protection it affords that it "must be interpreted with sensitivity to each generation's most reasonable understanding of its circumstances" (p. 231).

On Richards's analysis of the Fourteenth Amendment, the equal protection clause would both permit affirmative-action programs for African Americans and require them for women and homosexuals because they have historically suffered "unjust degradation" (p. 198). Poverty would become a "suspect classification," contrary to the Court's rulings. Human rights to "minimal subsistence, shelter, health care, [and] education" (p. 245) would also find a constitutional home in the privileges-or-immunities clause.

Richards preaches to his choir and will fail to persuade the unconverted. Little attention is paid to readings of the legislative history of the Reconstruction Amendments that run contrary to his argument. Indeed, competing interpretations and evidence advanced by the likes of Raoul Berger, Charles Fairman, and Earl Maltz are largely ignored or dismissed out-of-hand as "implausible" (p. 215).

Moreover, Richards claims to be innovative by adopting an interdisciplinary approach that weaves together law, history, and political philosophy. Historians, he says, "pay relatively little attention to the deeper questions of moral, political, and constitutional theory," while contemporary political and legal theorists are taken to task for being "fundamentally antagonistic to the central normative arguments implicit in history" (pp. 8–9). Alas, Richards's interdisciplinary approach is neither new nor persuasively executed. Richards's book is basically little more than a brief, a law-office history cobbled together in support of his political theory. Too much is erected and depends on too little. Richards's poorly crafted edifice falls under its own weight.

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DAVID M. O'BRIEN

Labor Visions and State Power: The Origins of Business Unionism in the United States. By Victoria C. Hattam. Princeton University Press, 1993. 226p. \$35.00

Victoria Hattam has written a fascinating book of great importance to all who are interested in the broader questions about the nature and peculiarity of American politics. Carefully researched and closely argued, the book sets out to explain why U.S. labor, in contrast to its European counterparts, adopted narrow business unions rather than broad class organizations. The answer to this question is central for Hattam to understanding what is distinctive about American politics since "The particular path of working class formation in the United States has set the broad contours of American politics" (p. 3).

Building on the insights of other students of American political development and political history, Hattam identifies the unusual power and dominance of the judiciary in the United States. This situation is in contrast to that in England and Western Europe. Hattam presents an especially rich analysis of the evolution of conspiracy law and judicial interpretations of it in the 19th century in this country. She shows that unions in the late 19th century were quite successful in getting some state legislatures, and even at times the federal government, to exempt them and guarantee their rights. State and federal courts, on the other hand, repeatedly undermined and "reinterpreted" these laws, often going against legislative intent, to the great detriment of unions. It was this experience, underscoring for labor unions the fruitlessness of electoral and lobbying activity-even when successful-that turned the American Federation of Labor, according to Hattam, away from politics and towards business unionism.

Hattam's argument, however, is not an account of autonomous state structures. Rather, it was the transformation of the labor movement in the post-Civil War period, and its interaction with the judiciary that led it to change its orientation. Economic development undermined the autonomy of crafts leading to a more proletarian workforce. Labor organizations began to organize on an employee basis, rejecting the earlier producerism which included workers, tradesmen, small businessmen, and farmers as part of the same grouping.

19th century period that Hattam wants to highlight. In fact, the late 19th century craft unions were becoming virulently racist (towards Blacks and Chinese especially) and exclusionary, and the Knights of Labor (Hattam's prototype producer organization) was successfully organizing female workers and racially integrating organizations even in the Deep South. Some of this activity of the Knights is documented graphically in works cited in the book (including Peter Rachleff's Black Labor in Richmond [1984] and Leon Fink's Workingmen's Democracy [1983]).

This misleading characterization of the Knights of Labor points to an even deeper problem with this important work. The rejection of alternative theories is much too glib. Hattam rejects arguments that identify questions of race and ethnicity and the structure of American politics (e.g., two party system, winner take all, lack of proportional representation). She argues that "Neither theory provides a convincing account of the nature and timing of the shift in AFL strategy at the turn of the century" (p. 26). Such an argument, especially with respect to race and ethnicity, is inaccurate. The end of the century was precisely such a turning point for race in American politics, a turning point that had important consequences in law, social organization, politics, and ideology. For labor and other social movements it was critical. The 1890s marked the defeat and destruction of both the integrated Populist movement in the South and the important beachheads of integrated labor organization in that region of the country.

According to E. E. Schattschneider, Northern Republicans, themselves fearful of both agrarian radicalism and northern labor militancy, made a Faustian bargain with Southern Democrats, establishing the System of '96, which held sway stably and unchallenged in American politics for over thirty years. As Schattschneider argues:

... The 1896 party cleavage resulted from the tremendous reaction of conservatives of both parties to the Populist movement, a radical agrarian agitation that alarmed people of substance all over the country....

The new alignment became possible when the southern conservative Democrats decided that they were willing to abandon their ambitions to win power nationally in return for undisputed control of the South. The Solid South was one of the foundation stones of the Republican system because it weakened the Democratic party disastrought and victorally

sary to protect the privileges of white, male, skilled workers. The alternatives at the time to the AFL were not merely producerist groups, but the later Knights, the Industrial Workers of the World, and within the AFL itself, the United Mineworkers of America, whose interracial, industrial organization was to eventually take it and its allies out of the AFL entirely.

The differences between England and the United States—which Hattam discusses in detail—were not just in the state structures and judiciary, although these differences were, to be sure, there. The differences also had to do with the greater breadth of the English labor movement, a movement in the United States that was undermined by the racist, anti-immigrant, male chauvinist, anti-industrial orientation of many of its constituents and of virtually all of its components and national leadership.

In short, Labor Visions is a rich book for all who are interested in the big questions of American politics. Its information and argument will undoubtedly form a key component of any full answers.

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MICHAEL GOLDFIELD

Latinos and the U.S. Political System: Two-tiered Pluralism. By Rodney E. Hero. Philadelphia: Temple University Press, 1992. 233p. \$49.95.

Hero presents us with a serious and timely book on Latino politics and the shortcomings of both previous scholarly work on the subject and the discipline's attitude toward studies of the politics of communities of color (also see Avalos 1989). This is a well-written and well-researched work. Hero's effort, which, he informs us, is one to enable an understanding of "Latino politics within the larger United States political context and United States politics from the vantage point of the Latino political condition" (p. xi), advances us in that direction. In this pursuit, the larger purpose of the work is both to demonstrate weaknesses in previous approaches and to suggest a new approach that takes these weaknesses into consideration.

Hero argues that "Latinos are in a disadvantaged position politically, socially and economically in the United States" (p. 11). This statement is an early indication of an institutionalized disjuncture in society that he defines as "two-tiered pluralism." In introducing us to this "new perspective" seeking to advance an understanding of Latino politics, Hero indicates that "Latino politics, as well as that of other minority groups, is best understood through a perspective of two-tiered pluralism... Because [of] historical, socioeconomic, and other factors, minority individuals and groups have largely been relegated to a lower social and political tier or arena" (p. 29). This theory, therefore, is being primed to explain how and why Latinos and other minorities have been relegated to a lesser place in the United States political system and, by implication, in economic and social life.

Much of the strength of this book resides in the author's critical summaries of "several major theories"—pluralism, coalition bias—elite theory, and internal colonialism—as applied to the study of the "Chicano political experience." He finds all of them, in varying degrees, wanting, although elements of each are absorbed into his own approach. He dismisses much of pluralist the-

ory, most specifically the argument that multiple access points exist for all interest groups seeking to be influential, while suggesting that coalitional bias (a modification of elite theory) has value in its stress on the importance of social stratification beyond simplistic elites and masses formulas to better understand group power relationships. Hero also assesses internal colonialism as a perspective that has assisted in the understanding of minority politics, although viewed by many as a paradigm (and relic) of the 1970s, through the theory's focus on racism, cultural genocide, and external administration, which are largely or entirely omitted in other theories.

The author develops an argument for the "unique" quality of Latino politics—and the necessity for scholars to understand this quality-into a predominant theme of the book. Part of his dissatisfaction with pluralism, especially the early variety, is that it does not contend with group uniqueness, which suggests to him the "inadequacy of conventional pluralist interpretations of Latino politics and the need for alternative frameworks" (p. 56). Intermittently throughout his book, Hero contends that a historical, as well as a social, background is necessary in order to place and understand the "Latino political situation" in perspective. Noting that there are "important differences" among Latino groups—Mexican Americans, Puerto Ricans, and Cubans—the author is also interested in pointing out similarities. Nevertheless, he also suggests that "formative (historical) events" have occurred in the political development of each group that have explanatory value in understanding each group's political situation and politics. Hero discusses a variety of issues regarding this Latino uniqueness theme, including perceptions of being discriminated against, group self-identity and self-labeling, partisan orientations, participation variables, policy concerns, interest groups, ideology, ethnicity, and class.

Although consistent with his stated structural purpose of the book, Hero's sections on Latino politics in national and state governance (chap. 5–6) are, nevertheless, largely "forgettable." He presents us with structurally awkward chapters that represent somewhat of a contrast and intrusion to his impressive early and closing chapters. Perhaps a stronger commitment to exploring and assessing Latino grassroots and community politics, political organizing, and coalition efforts might have been structurally more appropriate for an understanding of Latino politics. His conclusion, drawn at both national and state levels—that Latinos lack political clout—continues to be rather obvious to, and widely shared by, those studying Latino politics.

The assessment of the elections of Federicc Pena in the 1980s (chap. 7) is of interest and should have applicability to Latino and African-American candidates for major local elective positions, although it might have been structurally better subsumed under Latinos and urban politics (chap. 8). In fact, outside of its being a primary personal field of investigative research for the author, it is not entirely clear why this chapter stands structurally alone. Nevertheless, within the context of Latinos and urban politics, Pena's 1983 election as mayor was, as the author claims, significant for at least three reasons: (1) Pena's election marked the first time a Latino was elected to a powerful position in a city whose Latino population was relatively small (about 18% of the total population and 10% of the voting population); (2) Pena became the first Latino elected as mayor in a

"major U.S. city"; (3) Pena's victory was fashioned by an electoral coalition that bears similarities to the coalition formed around Tom Bradley in Los Angeles and Henry Cisneros in San Antonio. Hero's conclusions, drawn from Pena's two election victories regarding candidates of color, are that candidates must (1) get almost unanimous support from minority voters, (2) make "extraordinary" efforts to appeal to nonminority voters, and (3) form coalitions (p. 129). This supports findings of Browning, Marshall, and Tabb's Protest Is Not Enough (1984), among others.

Hero's chapter 10, "The Study of Latino Politics: Questions and Issues," claims to offer the first substantial critique of the much acclaimed and broadly used Browning, Marshall, and Tabb theory of political incorporation. While probably true, Hero's critique also serves as a bridge to link with his theory of two-tiered pluralism in the final chapter. The Browning, Marshall, and Tabb contribution to the study of minority politics, has been, according to Hero, adopted and heralded by the discipline in its full mainstream. As an example of "mainstream political science research into minority politics," it exemplifies a particular behavioral approach not previously subject to "extensive critical scrutiny." While acknowledging that Protest Is Not Enough "is sympathetic to the minority pursuit of social and political equality." Hero argues that there are problems with the model's approach that typify "problems of commission" (p. 177). Hero's two primary concerns are, How appropriate is the approach in the study of minority politics generally and Latino politics specifically? and How adequate is the approach in such study? On the first point, Hero is concerned that the Browning, Marshall, and Tabb model does not incorporate historical or political perspectives of Latinos into their analyses. According to Hero, being wedded to a pluralist approach, which he labels as "ahistorical," takes the understanding of historical uniqueness qualities away from the field of study and investigation. Additionally, Hero contends that Browning, Marshall, and Tabb largely dismiss the views of other scholars who have studied Latino politics, thereby leaving a void in terms of potentially enriching merging and diverging views with which to react. In terms of adequacy of the approach, Hero argues that Browning, Marshall, and Tabb focus on but one type of power relationship (decisional), whereas others, such as Stone, have argued that there are at least four. Hero also questions the adequacy of the almost exclusively allocational (albeit in "highly redistributive form") assessment of policy responsiveness. In other words, do increases in minority local public sector employment, commission appointments, and contracts, as well as the presence of a police review board ensure the political incorporation of a community and, implicitly, equality? Are these measures of sufficient consequence to ensure incorporation? Or is there a danger that in spreading the spoils of local influence through such channels to minorities, Erie's concern of a "proletarianization of minorities" becomes reality? What do Browning, Marshall, and Tabb mean by "political equality"? Is "political equality" a beginning, an end, or both? Does it necessarily follow that such equality would be achieved through incorporation, as implied by Browning, Marshall, and Tabb? These are significant questions posed by Hero, who argues that the allocational nature of these measures of policy responsiveness do not have much "consequence" for equality of redistribution (p. 183).

Finally, Hero hints at the cooptation nature of the Browning, Marshall, and Tabb model of incorporation, using Eisinger's concern that even where policymaking bodies become more racially and ethnically diverse, preservation of the political and economic institutional status quo remains both the goal and the norm.

Hero's concluding chapter lays out his alternative approach of "two-tiered pluralism." Thus, it is an important finale, although not very well-written (it is frequently redundant and somewhat confusing). However, perhaps much of this is intentional, as the author seems to be walking us through a "working out" of his theory. Hero defines two-tiered pluralism as "a situation in which there is formal legal equality on the one hand, and simultaneously, actual practice that undercuts equality for most members of minority groups, even if some individuals register significant achievements" (pp. 189-90). In agreement with points raised by Eisinger, Jackson, and Regalado, the theory assumes that there is only "a marginal inclusion of minorities in . . . political process[es]" (p. 190). The subordinate status of minorities in political life is emphasized where pluralism "exists in form but not fully in fact for some groups" (ibid.). Hero concludes that because two-tiered pluralism reflects, for Latinos and African-Americans, an institutionalized subordinate status in political and economic life, the theory represents an important theoretical and political dilemma for American liberal democracy.

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Civic Environmentalism: Alternatives to Regulation in States and Communities. By DeWitt John. Washington: Congressional Quarterly, 1994. 347p. \$39.95 cloth, \$21.95 paper.

Environmental Protection at the State Level: Politics and Progress in Controlling Pollution. By Evan J. Ringquist. Armonk: Sharpe, 1993. 243p. \$55.00 cloth, \$25.00 paper.

Until recently, most studies of environmental policy have focused on federal legislation, its implementation by the Environmental Protection Agency and other federal agencies, and the general deficiencies of "command-and-control" regulation. We now have a spate of new studies demonstrating that exclusive preoccupation with federal policy misses many of the most innovative and important developments in the field of environmental politics and administration. Indeed, both of the works under review argue that policy leadership on the environment, as in many other areas, has been shifting from Washington to the states since the mid-1970s. They provide data showing that this trend accelerated during the 1980s as a result of Reagan's "new federalism" and severe budget cutbacks, which ironically led to increased funding and adoption of new, more stringent environmental programs in many of the states. Both books suggest that "we are entering a new era of environmental policy" (Civic Environmentalism, p. 1) in which "many states have been far more active in controlling pollution than has the federal government" (Environmental Protection, p. 4).

What is most valuable about these works is the empirical evidence they are able to muster for this perspective, using entirely different methodologies. DeWitt

John's Civic Environmentalism rests on three lengthy, detailed case studies in which states have become the principal actors: reduction of agricultural chemical use to protect groundwater in Iowa, protection and restoration of the Florida Everglades, and negotiation of new energy conservation policies by electric utilities in Colorado. By contrast, Evan Ringquist's Environmental Protection at the State Level employs rigorous statistical analysis to explain variations in state policy outputs and outcomes. These works thus complement each other nicely and together add richly to our understanding of state policymaking and of emerging patterns of intergovernmental relations that include much bottom-up innovation.

John organizes his book around the concept of "civic environmentalism," which he claims as a label for a cluster of five traits that he argues constitute a "new paradigm" of environmental policymaking: (1) bottomup, rather than top-down; (2) directed toward a new generation of policy problems (ecosystem protection, pollution prevention, cross-media and nonpoint pollution, and sustainable economic development); (2) using a new set of policy tools (mostly nonregulatory and noncoercive, relying on collaboration, negotiation, education, information-sharing, technical services, public investment, and economic incentives); (3) stressing interagency and multilevel cooperation and coalition building; (4) avoiding confrontation; and (5) seeking to create new federal-state-local partnerships in addressing specific problems.

John elaborates and evaluates the strengths and limitations of this model at some length but is clearly advocating it as a desirable alternative to conventional federal regulation (though he is careful to state that it can only complement, not fully replace, the latter). He also has two useful chapters discussing the background of federal policies and budgets and presenting an overview of state environmental programs. But the heart of the book is the three case studies mentioned. These are detailed descriptive accounts (perhaps more detailed than need be on some points) illustrating many of the processes he attributes to his conceptual paradigm. The main disappointment is that his preoccupation with processes and actors does not leave room for as much evaluation of the success of the policy outcomes as one would like (clearly none of the three problems had been resolved at the time of writing).

Ringquist, on the other hand, sets out to explain both state policy efforts and their relative success. In doing so, he follows in the long tradition pioneered by Thomas Dye and others that attempts to explain variations in cross-state policy performance by regressing various output indicators against standard economic, demographic, and political institutional variables. But this is not just another such correlational study applied to state environmental policy. Ringquist takes a quantum leap forward by critically evaluating and refining traditional methodologies for tapping and integrating different kinds of variables that explain variations in state policy efforts and then by comparing these output levels with their actual outcomes in reducing air and water pollution. In other words, he asks not only why some states are much more active in addressing environmental problems than others but also whether state environmental policies matter. Do they work?

This adds up to an "integrated theory of state policy" that stands as an important contribution to the literature

in its own right (set out in chapter 4). Ringquist also provides a very succinct and useful summary of federal pollution control policies and state responsibilities under the existing regulatory framework. He then devotes separate chapters to explaining variations in state air pollution programs, air-quality outcomes, water pollution programs, and water-quality outcomes.

In constructing his output models, Ringquist draws on the best of comparative research on economic determinants (Dye, Sharkansky, Hofferbert, Lowry), political variables (Plotnick, Winters, Erickson, Wright, McIver), and group theory and regulatory policy (Lowi, Wilson, Meier, Wood). He critiques each of the traditional models and then integrates them into a comprehensive approach that is operationalized as both a single regression equation and, most significantly, as a causal model using path analysis to compare the weight of each of the most significant variables on policy outputs. These models are then run using standard measures of the strength of state air and water pollution regulatory programs as the dependent variables.

The results are quite interesting. Regressions show socioeconomic variables, political variables, and group influence variables all to be significant predictors of state air pollution programs. None of the traditional models is initially superior to the others. But when integrated in a causal model, the single best predictor of the strength of air-quality programs turns out to be state economic wealth, followed by legislative professionalism and the strength of polluting industries (all of which correlate positively). While the results tend to reject the "agency capture" thesis, they also indicate that factors such as opinion liberalism and the strength of environmental groups matter little. When it comes to water pollution regulation, on the other hand, per capita income and legislative professionalism turn out to be far less significant predictors than two interest group variables: mining industry strength (which correlates negatively) and agricultural industry strength (which correlates positively). State opinion liberalism is slightly more important when it comes to water policy than for air, but environmental group strength remains a weak determinant.

Finally, Ringquist attempts to determine whether relative policy strength (the dependent variable in the output model) translates into effective pollution control outcomes. The dependent variables now are (1) coefficients for both emissions and ambient concentrations for air pollutants and (2) pollution concentration levels only for water, in each case measured across the time period 1973–75 to 1986–88. Such data are notoriously suspect, but Ringquist probably does as well as possible in constructing his outcome measures.

The results are even more interesting than those for policy outputs. For air pollution, both regression and causal models show that state regulatory policy strength is a powerful determinant of emission reductions and air-quality improvement. Industrial variables have an offsetting impact. For example, fossil fuel consumption is the single most important determinant of nitrous oxide emissions, as one might expect. But overall, state air-quality efforts seem to pay off. The results for water pollution are far less reassuring. There is no significant correlation between policy strength and improvements in water quality. In fact, for some pollutants, regulatory strength correlates negatively. But Ringquist argues that strong state policies are a response to poor water quality, rather than vice versa. This confirms that traditional

point-source regulation is inadequate for controlling water pollution. Indeed, Ringquist concludes that neither state nor federal regulatory policies will succeed without further strengthening and without more fundamental changes in society.

Both of the works under review add greatly to our understanding of the role states now play in environmental policy. Both are well written and would make excellent texts in upper-level undergraduate and graduate courses in environmental politics, public policy, or state government. My own feeling is that Ringquist has probably taken aggregate analysis about as far as is worth going and that more detailed case studies like John's will add more to our understanding in the future.

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Norman J. Vig

Civil Rights and the Idea of Freedom. By Richard H. King. New York: Oxford University Press, 1992. 269p. \$35.00.

This book treats the concern with freedom that animated the Civil Rights movement. By way of advancing an Arendtian interpretation of the Civil Rights movement, Richard King announces early on that civil rights struggle led movement activists to forge genuinely interesting conceptions of what freedom is. An Arendtian approach stresses that politics, in its richest and most meaningful form, is about public justification and explanation of one's political actions, sometimes in dangerous circumstances, before other people—one's comrades and opponents and others who might join in one side or another. The Civil Rights movement becomes, in King's view, a central episode in the development of democratic thought.

King, a reader in American studies at the University of Nottingham, has personal experience of the movement and studied oral histories located at Howard University. His knowledge of African–American literature and political thought back to the antebellum period is also rather good.

King's most basic claim is that everywhere in the Civil Rights movement people were talking, singing, writing poetry, and thinking about freedom (not equality). Buildings or offices that were movement headquarters were called "freedom houses," not "equality houses."

oped—in the thought of Rev. Vincent Harding, the African–American theologian and historian who did so much to develop the now-famous Eyes on the Prize video histories of civil rights and Northern urban struggle. But the point bears repetition, here in this study and elsewhere.

As for the SNCC, the author offers valuable discussions of a key innovation of the SNCC, the "freedom schools," and of the theory about how to build new political identities and personalities that went into the SNCC's political program. The *freedom schools* conceptualized by the great SNCC leader Ella Baker and the related program of *citizenship schools* devised at the Highlander Folk School have been overlooked by movement theorists or democratic theorists with an empirical and institutional bent. As far as I know, the best place to begin studying these interesting institutions in this book.

The discussion of these innovations sets the stage, as well, for an account of how it became possible for some of the SNCC's leaders to adopt a related program—black power-and to become increasingly interested in the connections between violent militance and freedom, in contrast to Dr. King's overarching emphasis on patient nonviolence. The author continues with a close analysis of the origins and character of Frantz Fanon's theory of political violence and with a suggestive treatment and critique of his influence on such SNCC leaders as (then) Stokely Carmichael and on Black Panther leaders, such as Huev Newton. The book ends with rather thoughtful speculation about how the full Arendtian possibilities in the SNCC for establishing durable institutions of continuously liberating popular participation were critically undermined by television's political temptations. The role Arendt assigned to public action and speech could not be realized, King seems to suggest, when several SNCC and Panther leaders chose sound-bite slogans and hot political images of protest over the hard work of ongoing engaged debate and institution building. Although the author does not draw this inference, he may have identified one reason for why, in contrast, Dr. King's speeches, styles, and concepts are still read, memorized, and used: King, his colleagues, and his followers did the kind of difficult Arendtian politics that would assure them lasting influence.

In all, this is an interesting and, in places, fascinating book. Still, it is worth noting certain problems. The topic deserves better organization and plainer writing than concept of the "mirror stage" and their relationship to the evolution of Frantz Fanon's thought. These are doubtless important matters; and by themselves, the discussions are illuminating. But they are distant from the task at hand.

Finally, though there is a cursory treatment of the Exodus story, the book does not explore whether there is a relationship between African-American religious thought and tradition, on the one hand and, on the other, the possibility and evolution of the innovations he investigates. Taking up the issue might have permitted treatment not only of mainstream religious traditions but also of Garveyism (the largest mass movement among African-Americans in this century) and its relationship, if any, to the democratic heyday of the Civil Rights movement.

Still, it is a measure of this book's contribution that it genuinely stimulates reflection about its important claim that the democratic revolution caused by the Civil Rights movements was about more than the innovative protest tactics that have rightly attracted analysts of collective action and social movements. The movement was also profoundly innovative at the level of political thought.

Swarthmore College

RICHARD M. VALELLY

The Speaker and the Budget: Leadership in the Post-Reform House of Representatives. By Daniel J. Palazzolo. Pittsburgh: University of Pittsburgh Press, 1992. 262p. \$39.95.

How has budget process reform changed the role of the Speaker? How important are individual characteristics in understanding a Speaker's leadership style? These two questions form the heart of Daniel Palazzolo's Speaker and the Budget, the most extensive analysis to date of the role of the Speaker in the new budget process.

The budget reform act of 1974 left vague the Speaker's role and many other aspects of the new budget process. Consequently, Speakers have had to adapt. Palazzolo argues that Speakers since budget reform have taken on a shifting set of roles. These roles include the nurturer, who seeks to calm fears about the change in established congressional procedure; the process manager, who attempts to keep budget making timely and prompt; and the opposition party leader, who articulates, protects, and advances the policy and political interests of the majority party. A Speaker is likely to be performing more than one of these tasks, and the tasks will sometimes clash. The role assumed at any particular time depends on policy, political, and institutional conditions and the individual characteristics of the Speaker. Among the more important of the latter are his level of budgetary policy expertise, conception of leadership, and policy preferences. Palazzolo departs from literature arguing that Speakers are either creatures of contextual influences or individual masters of congressional destiny. Instead, he shows that while conditions will largely determine what role is most appropriate at a given point, individual characteristics will influence the manner in which that role is conducted. The author acknowledges that the Speaker's ability to exercise policy leadership is usually constrained (not least by the demands of the membership), but he also suggests how Speakers can overcome this constraint.

The bulk of the book applies this theoretical framework in great detail to the postreform period through 1988. Carl Albert performed a largely nurturing role, educating members of Congress about the new process, reassuring committee chairs that the new system was not designed to challenge their authority but rather to produce an alternative to the president's budget. As Tip O'Neill took over the Speaker's responsibilities, policy controversies, especially within the Democratic party, were escalating. O'Neill became a process manager; and as with Albert, his individual characteristics and open, participatory view of leadership fit the new demands well. Once Ronald Reagan assumed the presidency, O'Neill increasingly emphasized his role as opposition party leader. In both roles, Palazzolo shows that O'Neill enjoyed limited success. Finally, Jim Wright as an opposition party leader inclined toward an activist leadership style because of conditions, his own characteristics, and the brief tolerance of House members.

Palazzolo includes individual characteristics in his framework, but he is careful not to be overwhelmed by them. His emphasis on context is particularly useful as a counterweight to other studies that perhaps exaggerate the influence of postreform leadership. Similarly, Palazzolo draws a nuanced picture of the relationship between the membership and leadership. Palazzolo shows nicely how leaders have managed to be both well ahead of, and well behind, members over remarkably short periods. Finally, the book provides substantial detail, is well written, and uses interviews intelligently. This is one book that does not ask the reader to endure innumerable excepts from the author's interview notes.

A few problems weaken the force of the argument. First, the author does not deal directly enough with the classic problems of research of this kind: What do you do when the n is 1? Where are the counterfactuals? Are conditions ever truly "similar" for different Speakers at different times? Although they are to some degree at the heart of the entire book, these problems deserve a more focused consideration, along with any implications they may pose for the author's findings. Second, the book ignores that by the mid-1980s, the budget had weakened as a macroeconomic management tool. The agenda was to reduce the deficit and allow monetary policy to drive the economy. Did this momentous transformationcertainly a new condition-have an impact on the budget process? A third problem is the awkward treatment of activist policy leadership. To Palazzolo, this leadership is apparently neither a role nor an individual characteristic but an undefined combination of both. Unless one wants to assume that only certain individuals can respond to crises with dramatic and activist policy leadership, policy activism is best treated as an additional role. Finally, while the book does not overstate the power of the Speaker's individual characteristics, the author too willingly accepts that Ronald Reagan's personal persuasiveness, charisma, and congressional liaison continually turned the tables in the budget fights of the early 1980s.

Shortcomings aside, the argument in *The Speaker and the Budget* is sophisticated and often subtle. The book would be of interest to budget scholars and students of Congress generally.

University of Wisconsin, Madison

JOHN J. COLEMAN

Mobilization, Participation, and Democracy in America. By Steven J. Rosenstone and John Mark Hansen. New York: Macmillan, 1993. 333p. \$15.00.

This book considers familiar questions: Who participates? Why has participation declined? It uses familiar data. Yet, in its rather unassuming way, it offers a dramatic reframing of the way American politics researchers think about involvement in politics. At least one strand of research on social movements has traditionally relied heavily on the notion that mobilization (intentional activities by elites designed to gain support or generate resources from target groups) is the central "cause" of individual or group activity. This research has seen the demographic and attitudinal attributes of individuals primarily as contextual factors that may inhibit or encourage the kinds of social relations that lead to social and political action. As one example among many studies in this tradition, McCourt's Grass Roots Politics (1977) described how women in workingclass Chicago neighborhoods were drawn into neighborhood organizations by group leaders and how aspects of these women's lives (e.g., whether or not they had children or their husbands were supportive mediated the mobilization relationships.

In contrast, behavioral political science research has seen participation as being a set of essentially individual-level decisions shaped largely by such incividual attributes as education, income, civic values, and political efficacy. Much of this difference in perspective stems from the different research methods used by sociologists interested in social movements and political scientists interested in participation: while the former have often relied on participant observation and interviews with group members and leaders, the latter have investigated participation primarily by means of survey research.

Steven Rosenstone and Mark Hansen's new book uses the traditional methods and data of behavioral political science (National Election Studies [NES] and Roper surveys) to support a new and distinctive theory of participation that *does* allow "the strategic choices of politicians, political parties, interest groups and activists" to play a central role. Their argument is that "people participate in politics not so much because of who they are but because of the political choices and incentives they are offered" (p. 5).

They argue for the "centrality of strategic mobilization," though they extend and build upon a tradition that focuses upon "resources, interests, identifications, and beliefs of individual citizens" (p. 5). This twopronged argument-accepting much of the logic and evidence of traditional "attribute-based" research but trying to build in a focus on mobilization—is laid out in more detail in the second chapter. Using NES data, the authors examine group differences and trends in various types of participation since 1952. Using Roper surveys conducted between 1973 and 1990, they trace the trends in contacting (writing to a senator or representative), attending local meetings, and signing petitions. They find seasonal cycles in governmental participation but conclude that neither the short-term fluctuations in participation nor the long-terms trends can be adequately explained by changes in demography or attitudes.

Rosenstone and Hansen inventively employ a variety of data to illustrate the importance of issues and elites in generating mass-level political activity. For example, they use NES data on whether or not the respondent

was contacted by a party and also Congressional Quarterly data on the competitiveness of particular elections. For example, the presence of a contested gubernatorial campaign (in presidential election years) means that citizens are 5% more likely to vote. Those citizens in the midst of a competitive House race are more likely to contribute money to parties or candidates, as well as to vote. Another example is the authors' use of McAdam's 1982 tabulation (in Political Process and the Development of Black Insurgency, 1930-1970) of civil rights activities, over time and by region, to examine the extent to which these social movement activities exert a mobilizing (or countermobilizing) effect. In these analyses and in their analysis of the correlates of the contact by parties variable in the NES surveys, they find that political mobilizers target their efforts strategically-in particular, concentrating on people who have resources, strong party affiliations, or who are "centrally placed in social networks" (p. 210). Thus, they conclude that people in associations are more likely to be mobilized, rather than just to participate—a subtle but important distinction that brings elites into the picture.

Toward the end of the book, Rosenstone and Hansen attempt to solve the "puzzle" of declining voter participation. They create a "simulation," in effect, which attaches to the respondents in the NES surveys of the sixties the attributes of present-day voters (more education, less efficacy, less partisanship, more skepticism about candidates, less integration into communities, etc.). (I should note that even fairly complicated analytical methods are explained coherently and concisely.) The resulting decomposition of the decline in turnout indicates that two factors (change in voter registration laws and increased education) should have increased turnout by 4.6% and that less than half of the decline can be traced to Americans' declining efficacy, weakened social involvement, increasing youth, and decline in partisanship. The major part of the decline they attribute to declining electoral mobilization: parties "abandoned the labor-intensive canvassing methods of the 60s" (p. 218), electoral competition declined, and demands on campaign resources intensified (i.e., the number of contested presidential primaries doubled).

Though I disagree with some of the assumptions here (e.g., the analysis in chapter 3 essentially assumes that the relationship between race and participation has remained constant since 1952) and feel that the book was unduly repetitive (e.g., the argument about why education increases participation is repeated far too many times), I believe that Rosenstone and Hansen's argument and analysis will—or should—introduce a new and fruitful way of conceptualizing and investigating political participation.

Syracuse University

Kristi Andersen

Does Redistricting Make a Difference? Partisan Representation and Electoral Behavior. By Mark E. Rush. Baltimore: Johns Hopkins University Press, 1993. 172p. \$32.50.

Does redistricting make a difference? Not according to Mark E. Rush in this insightful look into partisan gerrymandering and its impact on fair representation. Indeed, his central thesis is that a partisan gerrymander is "in fact virtually impossible to prove; furthermore, its

impact is unsubstantiated by political science research" (p. ix). Noting that a group's opportunity to be represented is dependent on "durable, identifiable groups of voters," the author points to the paucity of evidence that the parties-in-the-electorate are, in fact, durable or identifiable. Arguing that the variability of partisan voting behavior has been overlooked by the judiciary, as well as political scientists, he contends that a gerrymander cannot have an impact regarding representation of a group if voters of that group are likely to change their partisan behavior in subsequent elections.

Rush's work is well organized and follows a logical progression. Beginning with theoretical considerations revolving around defining a gerrymander, the courts' rather confused rulings, and the contributions made by political scientists, he then shifts to an empirical analysis intended to demonstrate that partisan voting is not constant but rather in a state of flux. Rush does an excellent job of portraying the Supreme Court as a body that has been inconsistent and contradictory. Noting that the Court has been unable to delineate what is meant by a political group and fair representational opportunity, he condemns the case law as "laden with unclear terminology, confused concepts, and conflicting precedents, as well as incorrect assumptions about voting behavior, political partisanship, and representation" (p. 11). Tracing Supreme Court decisions from Colegrove v. Green through Davis v. Bandemer, Rush offers a clear and concise account of the Court's difficulties in moving from an explicit individual right to vote to an implicit group (esp., partisan group) right to vote. His prescription: the courts should stay out of this political thicket.

As a political scientist interested in gerrymandering, I was enlightened, provoked, and forced to reexamine my own assumptions by Rush's review of "political science model" (chap. 3). Drawing from what might be considered the classic works on American electoral behavior, this chapter attacks our conceptions of party identification and party voting. Focusing on the debate among the authors of The American Voter, Key, Burnham, and Nie, Verba, and Petrocik, voters are portrayed as sometimes partisan, sometimes not; maybe rational, maybe not; driven by issues, driven by candidate appeal; and so forth. It is precisely this failure of political science research to provide a conclusive, fixed profile of all voters that undergirds Rush's central thesis. Quite simply, a number of factors may influence voters and changing the electoral environment may cause changes in partisan behavior. Indeed, based on the research findings, even constituencies that show no change in their partisan voting patterns following redistricting cannot be assumed to be an identifiable, cohesive voting bloc. They may have been affected by "(1) the strong partisan ties on the part of voters, (2) the similarities of the candidates and issues in the new and old districts, or (3) a low level of sophistication and interest on the part of the affected voters" (p. 54).

While I agree that a number of factors influence an individual's voting behavior, there are several problems with this approach. First, noting that some (perhaps many) voters switch does not preclude the existence of identifiable partisan voting blocs within a district who do not. These groups could well be the target of a gerrymander. Second, Rush ignores the possibility that one strategy of the majority party in drawing new maps might well be to move new voters into a previously stable minority-party district to create an element of

unpredictability, thus typing up party and candidate resources. Finally, this perspective presents political scientists with somewhat of a no-win situation. Stated in the extreme, if the party split stays the same following an election, it is because of the similarity of candidates and issues; if the party split changes, it shows there are no durable partisan voting patterns. I am left wondering how durable, consistent partisanship could be demonstrated. Unfortunately, Rush does not tell us. He should examine the possibility that a partisan gerrymander may be a matter of degree, not dependent on stable voting patterns either statewide or even districtwide.

These difficulties are not intended to refute Rush's theoetical considerations. Rather, they point out that he does not carry those considerations far enough. His basic premise that durable partisan voting patterns are central to an analysis of gerrymandering is still intact and provides a nice lead-in to the empirical section of this book.

To examine the consistency of partisan voting behavior under similar and differing circumstances, Rush utilizes election results for Congress, governor, and the state senate in Connecticut and Massachusetts from 1972 through 1986. His initial analysis concerns the suitability of statewide measures of partisanship. Through the use of regression analysis (with towns as his unit), he demonstrates that party registration figures for both states are remarkably consistent over the entire period. The previous year's party registration levels predict the current year's with an R-squared over .90 in every case. However, election data indicate a much more volatile electorate. The Democratic percentage of the vote in the previous election does not serve as a particularly good or consistent predictor of the Democratic vote in the current year for the offices studied. For example, Connecticut's congressional election outcomes, "indicate that marked changes occurred not only in the statewide propensity to vote Democratic (shown by the broad fluctuations in the betas) but also in the uniformity of the state's yearly partisan swings, as shown by the wide range of the value of the R-squares" (p. 82). His general conclusion here is that states can be quite heterogeneous and that partisan behavior is not necessarily consistent over time or between levels of office. Consequently, statewide measures of partisanship are not suitable for demonstrating a gerrymander.

Recognizing the inadequacy of explaining the current year's vote with the previous year's, Rush next incorporates variables tapping the impact of incumbency, contested elections, and redistricting. His findings go to the heart of his critique of the assumptions made by studies of partisan gerrymandering. Assuming that a partisan voting bloc can be divided to prevent a majority from forming or that a particular legislator can be defeated by merely shifting voters ultimately depends on the likelihood that those voters will "maintain their partisan profile" (p. 103). Rush's analysis refutes this likelihood: "In fact, all redistricted towns display one common characteristic: regardless of their previous partisan behavior, they all experienced increases in partisan vote levels for the party or incumbent controlling their new district" (p. 103). Not only did the partisan vote increase, but the increase was often greater than that which occurred in towns remaining in districts held by that party's incumbents.

These data reveal an important finding. When the political environment changes, voting behavior changes,

whether it is due to incumbent retirement, redistricting, issues, or weak partisan attachments. To the extent that this fluctuation in voting behavior is meaningful, Rush's thesis that a gerrymander cannot occur is upheld. To demonstrate a gerrymander that is discriminatory necessitates identifying blocs of partisan voters who are moved strategically to the detriment of the party out of power. If those voting blocs are defined on the basis of election results but their members do not vote out of partisan considerations (or if they do, not consistently), then the underlying assumptions needed to demonstrate a partisan gerrymander are unfounded, as are claims of unfair representation by the "out party." This is precisely what Rush maintains and his data show. However, there are several shortcomings with his analysis that need to be examined.

A focal point initially addressed by Rush is that a statewide pattern of partisan voting (i.e., a base vote) is not a good indicator of the behavior of subdivisions within a state. In part, this contention is supported by the results of gubernatorial elections. Yet the race for governor is an election involving high visibility, issues that are more likely to catch the voters' interest, and a higher propensity for candidates to deemphasize the party (since they are faced with an electorate composed of a large proportion of "independents" who have access to other-than-party cues). Can we reasonably expect election results to reveal a clear, consistent, partisan voting pattern? I would prefer an analysis examining election data for a less visible statewide office where a greater reliance on the party as a voting cue could be assumed. It is this type of office that is generally used by both individuals drawing district maps and students of the impact of redistricting. If the findings of such an analysis were similar to those reported by Rush, I would have more confidence in their validity.

A second drawback with this book's empirical section has to do with the unit of analysis. Rush uses election data for towns in both Connecticut and Massachusetts throughout the period under study. A better approach would rely on much smaller units, such as precincts. There is no effort to distinguish between small or large towns, nor is there any indication of the degree to which some towns may dominate a legislative district while others are relatively insignificant to the district's electoral makeup. It may well be that some areas in a town are stable bastions of party support, with others exhibiting frequent voter fluctuation. Utilizing towns as the unit of analysis not only masks this pattern but also presupposes that when drawing district maps, the cartographer creates districts through a reliance on aggregate votes from towns—an unlikely supposition. This difficulty primarily affects the author's use of bar graphs to show variable patterns of town voting over time. However, a similar observation applies to his regression analysis wherein the party vote in legislative districts is to be explained. If the vote in some areas of a district exhibits a historically partisan stability but is highly volatile in other areas, this pattern will remain hidden if the vote is aggregated by district.

A final criticism of Rush's empirical analysis has to do with his apparent view that partisan gerrymandering is an all-or-nothing endeavor. There is no attempt to differentiate districts that might be most amenable to a shift in voting blocs from districts that would not. Nor does he take into account the possibility that a "successful" gerrymander might require only the strategic shift

of a relatively small number of voters in a few districts. For example, even in his analysis of the Connecticut state senate elections of 1982 (a redistricting year), he provides no a priori reasoning as to what we should expect in the towns which were involved in new districts. Were the new districts dramatic alterations of the old? Did the voting patterns in the new districts reveal a consistent, across-the-board change in partisan voter behavior throughout the town? These and related questions cannot be adequately addressed from the approach employed in this study. Does this invalidate the work by Rush? By no means.

Despite the aforementioned criticisms, *Does Redistricting Make a Difference*? is worth reading. It provides evidence that the assumptions made by legal experts and political scientists regarding the possibility of, and the consequences from, partisan gerrymandering may be faulty. It raises serious theoretical questions about concepts such as "fair representation" and "politically relevant groups." Finally, a careful review of this work yields fresh insight as to how to demonstrate the existence of a gerrymander. Can a partisan gerrymander make a difference? I think so, but I agree with Rush that we have failed to illustrate how.

Ball State University

GARY L. CRAWLEY

Prejudice, Politics, and the American Dilemma. Edited by Paul M. Sniderman, Philip E. Tetlock, and Edward G. Carmines. Stanford: Stanford University Press, 1993. 339p. \$45.00.

Race is the keyword that connects the twelve chapters brought together in this volume. Not much connected otherwise, most have something to offer taken individually. A strength of the volume is its cross-disciplinary character, particularly important for this area of research.

The chapters by psychologists attempt the greatest generalization (as is typical when human nature is the primary disciplinary focus). Rothbart and John provide an excellent summary of three important findings from experimental research on stereotypes: (1) even random labeling of individuals as members of groups (the "minimal group paradigm") tends to bias memories and behavior; (2) stereotyping, though a normal cognitive process needed to simplify the world, often leads to confounding of description with evaluation; and (3) positive contact with an outgroup member may not reduce stereotypes, for the effect may be to distinguish the individual from the negatively viewed group, rather than change the image of the group. The authors also present longitudinal data on stereotypes by students in a large university and note that the absence of a positive characterization (e.g., black students are not described as "scientific") can be meaningful, given the reluctance of students today to voice negative stereotypes in questionnaire settings.

An even more ambitious attempt to generalize is provided by Sidanius and Pratto under the heading of social dominance theory. Racial and other forms of oppression occur, they hold, because all societies develop group-based hierarchies for evolutionary reasons (e.g., allocation of scarce resources). The argument is most convincing when the authors cite the dominance of one ethnic group by another in many societies through-

out history. Their own research evidence seems less persuasive: it involves mainly individual difference variables, yet their theory is primarily at a macro level. In addition, because of the authors' highly general aims, the theory has little to say about variation over time within a country like the United States.

The least relevant psychological chapter is Gough and Bradley's report of research using classifications of individuals by their spouses and acquaintances in terms of words like "cynical" and "intolerant." The aim is to avoid the limited validity that can be ascribed to self-report of ethnic attitudes. Although an interesting idea, the authors relate their results only to a variety of psychological scales developed by the senior author, not to ethnic attitudes or behavior. Thus, one problem of validity is replaced by another.

With the exception of Rothbart and John's data on college students, none of the chapters by psychologists throws light on the present state of racial attitudes in the United States. This is the province primarily of the political science and sociology contributors. Thus, Dowden and Robinson present information showing that 18- to 25-year-old Americans no longer provide a major source of increased liberalization of white racial attitudes, as was found in earlier research. However, the youngest white cohorts remain more liberal than older cohorts on most issues despite the twelve Reagan–Bush years, which suggests a half-full, rather than half-empty, interpretation of the same data.

Three chapters by varying combinations of the volume editors and associates focus directly on political conflicts within the United States. The basic argument is that in addition to earlier issues of equal treatment versus personal prejudice, there are now other issues that invoke political disagreements of a nonracial character, over the implementation of a social welfare agenda and various forms of affirmative action. Opponents of what are often seen as problack positions in these controversies are regarded by the authors as motivated primarily by political beliefs that transcend race, not by covert prejudice. For example, Carmines argues that the same appeal to individualistic values that helped the civil rights struggle three decades ago is now used to oppose affirmative action.

There is merit in the premise of these chapters: as Lipset and others have argued, it is misleading to assume that all issues involving race can be reduced to a conflict between bigotry and egalitarianism. Yet even if Americans do hold abstract ideological beliefs that resist preferential treatment for blacks, one notes that preferential treatment for one's own interests (e.g., continued high social security benefits for wealthy retirees) manage nevertheless to be justified. Individualistic values are more "flexible" than the editors credit, depending on who is the recipient of some special benefit.

A different attempt to distinguish among issues within the broad racial debate is presented by Kluegel and Bobo. They employ factor analysis to discern patterning of attitude items in the National Election Study, the General Social Survey, and ABC/Washington Post polls. The chapter demonstrates both the virtues and limitations of such an approach; for though it provides a wealth of information on a massive amount of data, the outcome is clearly contingent on the scattering of items available to the authors. One of the more interesting later results is that while age and education show moderately large associations with traditional measures

of prejudice, associations of the same variables with beliefs about the prevalence of discrimination are much smaller.

The remaining four chapters in the book concern African–Americans directly and do so with varied questions in mind and with quite different methods. The most relevant in terms of the title of the book is "Ethnic Stereotyping," a secondary analysis of black stereotypes about other minorities. Sigelman, Shockey, and Sigelman find that in comparison to whites, blacks do not differ significantly in their stereotypes of Hispanics but do hold more negative stereotypes of Jews. There are also racial differences in the demographic correlates of stereotyping. For example, age is positively correlated with stereotyping for whites but not for blacks.

Hochschild contributes a broad review of the distinctive economic, social, and psychological problems faced by middle-class African-Americans in a white-dominated society. The chapter offers a mixture of anecdote, evidence, and speculation, which is useful but sometimes unclear as to what weight should be given to particular conclusions. Stanley's careful analysis of supporters and opponents of Jesse Jackson as a presidential candidate acknowledges that it is impossible to know the attitudinal sources of white opposition to Jackson (e.g., how much reflected racial prejudice). The chapter is of value, but would fit better into a book on voting behavior.

Last (but definitely not least) is Hauser's investigation of why college entry among African–Americans declined sharply from the mid-1970s to the mid-1980s. In addition to learning the most sensible answer to this question, readers will benefit from a knowledgeable critique of some popularly voiced claims (e.g., that as many black men of college age are in prison as are attending college) and will come to appreciate the limitations and pitfalls of government data for this and other important issues.

University of Michigan

HOWARD SCHUMAN

How Women Legislate. By Sue Thomas. New York: Oxford University Press, 1994. 205p. \$35.00 cloth, \$14.95 paper.

Sue Thomas has written a book that all institutions will want for their libraries and many political scientists will want to acquire for their course work and personal collections. Thoroughly researched and deftly presented, How Women Legislate provides incontrovertible evidence that women in politics do make a difference. While these differences may be less transformational than some of the earliest feminists envisioned, they are probably much more substantial and impressive than those unfamiliar with the literature of women and politics have assumed. What makes this book uniquely valuable is that it takes "what women want" out of the realm of conjecture and grounds it solidly in the goals which women state legislators express for themselves. Thomas then goes on methodically to demonstrate how these aspirations do (and do not) differ from those of their male colleagues and then further documents the impressive extent to which these aims are being realized.

Thomas begins this study with as succinct a summary as one is likely to encounter of the longstanding and perplexing differences, even among contemporary feminists, as to whether women would be better served by an equality strategy or by a strategy that acknowledges and protects women's special capacities and roles. From this necessary grounding, Thomas neatly segues into a central question that has frequently been posed but rarely answered: Will women confront or adapt to the status quo? Will they contest the existing "rules of the game" or go along to get along?

Drawing primarily on her own interviews, questionnaires, and observations but also on a very comprehensive search through the relevant literature, Thomas contrasts the somewhat reticent early women state legislators with their much more self-confident, ambitious, and policy-producing contemporary counterparts: "In the 1970's, women state legislators, for all their acknowledged hard work and preparation, did not engage in active participation in the everyday world of legislatures. ... In contrast, women of the 1980's, while acknowledging similar obstacles, clearly plowed through them and moved on to contribute in all areas of legislative life" (p. 83). Interestingly, the later women legislators have developed increasingly, rather than decreasingly, distinctive policy priorities from those of their male colleagues.

One of the book's most intriguing and important aspects is the evidence of just how rapidly women lawmakers have strengthened their self-regard and increased their policy successes in a relatively short period of time. Drawing on the findings of both psychology and sociology, Thomas convincingly explains these differences primarily through the power of proportionality. Interlegislative comparisons make it very clear that as women move from token minority status to significant (albeit still minor) presence, there will be reduced pressures for conformity and increased possibilities of and support for distinctive behavior. Thus, as women have become more numerous, they also become increasingly assertive about pressing their longstanding interests in legislation of special consequence for children, women, and families. Obviously women are still a long way from dominating or even being consistently successful in influencing the state public policy agenda, but Thomas supplies sufficient historic background to make this relatively sudden explosion of empowerment (sudden in contrast to the 72-year struggle for federal suffrage) not just notable but astonishing.

Thomas next examines why, given the increased self-confidence and policy effectiveness of today's women legislators, there has been little tangible change in the legislative process, a goal that many women (but relatively few men) legislators articulate. If the institutional reforms (as compared to the policy accomplishments) to date seem somewhat disappointing, Thomas attributes that disappointment more to the unrealistic expectations of those who predicted and expected radical change than to the timidity of the women officials themselves. She also effectively employs some quick case studies of congressional reform to demonstrate the formidable obstacles facing institutional revisionists.

Stylistically, some of the author's points are reiterated more than is necessary in this relatively compact volume. Substantively, the failure to even mention the Term Limits movement, which will clearly have profound impact on state legislative institutions generally and, in particular, on women's chances both for election to, and leadership within, these institutions is disappointing.

However, Thomas has, by answering some of the central but heretofore largely unanswered questions about women in politics, moved the entire discussion beyond where it has been churning to important new ground. Since her book includes the questionnaire and interview instrument used in her research—and a very comprehensive bibliography—those interested in tracking the next stage of developments will find that Thomas has provided the tools and the directions for them to do so.

University of Arkansas

DIANE D. BLAIR

COMPARATIVE POLITICS

U.S.-Mexico Relations: Labor Market Interdependence. Edited by Jorge A. Bustamante, Clark W. Reynolds, and Raul A. Hinojosa Ojeda. Stanford: Stanford University Press, 1992. 495p. \$52.50.

This edited volume is the fifth in the series issued by the United States-Mexico Relations Project, a consortium of American, Canadian, and Mexican scholars organized by Clark Reynolds at Stanford. In 19 chapters, 25 researchers explore aspects of what most believe is a "binational" labor market linking the United States and Mexico. The linkages between Mexican and U.S. labor markets have been strengthening for most of the twentieth century. During the 1980s, it was possible correctly to call the linkages between particular U.S. industries and Mexican workers a binational labor market, as some fruit and vegetable farms, hotels, and restaurants, and janitorial firms became dependent on Mexican workers in the same manner that Wall Street depends on commuters from New Jersey and Connecticut. This 1992 book includes the papers presented at a conference held in 1986. The contributions thus reflect the concerns of the mid-1980s: authors' references to contemporary events are full of U.S. labor shortages, a (then) proposed immigration reform and control act, and a Mexico that was just taking its first steps to switch from inwardlooking to outward-oriented economic policies.

Readers should turn to this book primarily for its perspectives on how U.S.-Mexican labor interdependence was seen in the mid-1980s. Some of the themes remain important: the Mexican drumbeat that illegal immigration is due to more U.S. demand-pull rather than the story of Mexican supply-push, the evolution of many Mexican communities so that they are dependent on the U.S. labor market and remittances earned there, and the undermining by desperate Mexican immigrants of wages and standards in U.S. labor markets. Other contributions reflect ephemeral fashions, including assertions that the United States will, by 1990, surely "need" to import Mexican workers to fill vacant jobs.

Few people will read this five-hundred page book from cover to cover. For this reason, I highlight the points raised by the various contributors to guide readers to chapters of interest. The editors grouped the papers into four sections: a conceptual framework, supply-and-demand for migrant workers, migration's impacts on Mexican and American communities, and sectoral dynamics. In the book's last chapter, Clark Reynolds explores whether—and how fast—wages will converge in what is likely to be a more integrated North American labor market.

The editors are well-known researchers with a point of view. Their introduction emphasizes the importance of Mexican-born workers in the U.S. labor force (Mexican immigrants account for more than 10% of U.S. labor force growth); the key role of remittances in the Mexican economy (exporting people generated as much foreign exchange as exporting oil in the mid-1980s); and the apparent neat match between a youthful Mexico and an aging United States. (According to the editors, the United States "will be facing a serious shortfall in labor supply . . . into the next century" [p. 2].)

In order to better understand labor market linkages between the United States and Mexico, the contributors were asked to explain who Mexican migrant workers in the United States are and what effects they have on the United States and Mexico. Contributors were charged with evaluating the benefits and costs of labor migration for groups such as employers, workers, and consumers. Finally, contributors were asked to assess the likely effects of NAFTA on these labor market linkages. Contributors were also asked to evaluate IRCA, the U.S. effort in 1986 that legalized almost three million aliens and attempted to reduce future illegal immigration.

The first three chapters argue that restrictionist U.S. immigration policies reflect fallacious or elite beliefs about the effects of illegal immigration. Jorge Bustamante, for example, contrasts to "U.S. public opinion," which wants illegal immigration stopped, the "economic reality" that such immigration is beneficial (p. 23). Bustamante outlines four likely scenarios and their migration consequences, from rapid U.S. economic growth and Mexican political stability (which, according to figures cited but not included in the book, would lead to more female and urban migrants) to a U.S. recession and instability in Mexico (which, according to a fairly detailed elaboration of U.S. responses, would lead to a "border wall and moat") (pp. 33-34, 39).

Heller's contribution is a lengthy summary of progressive and corporatist U.S. history, which, in his view, made immigration restrictions a necessary adjunct to industrialization, the creation of a working class, and unions. Garcia y Griego argues that a few officials at the apex of Mexico and the United States have been able to make immigration decisions relatively unfettered by societal pressures (p. 76). In a fascinating series of seven cases, Garcia y Griego shows how the U.S. government twice initiated Bracero programs to bring Mexican workers legally to the United States: once in 1917 and again in 1942 (p. 85). In his discussion of these cases, I think Garcia y Griego gives too much credence to "fears" that without Mexican workers, the United States war effort might have been in jeopardy. Many agricultural subsidies, including foreign workers, were initiated during wartime "emergencies" and then maintained in peacetime, from Bracero programs to mohair subsidies (to make soldiers' coats).

Six papers and one comment make "Supply and Demand" the longest section of the book. Hinojosa Ojeda and McCleery describe a CGE model they constructed to stimulate the effects of various types of North American economic integration. Three types of economic integration are simulated-protectionism, neoliberal opening, and managed interdependence—with the authors clearly favoring managed interdependence, rather than the neoliberal NAFTA. According to their neoliberal model, changes in capital and labor (migration) policies have greater effects on production than do changes in trade policy, reinforcing the argument of those who said that NAFTA was more about investment

Wayne Cornelius, perhaps the best-known North American migration researcher, summarizes data from a number of Mexican communities during the 1980s to demonstrate that Mexican sojourners or temporary

workers are increasingly U.S. settlers. Hayes-Bautista and his colleagues explore California's demographic future, projecting a median age of 39 in 2030, a minority population of 62%, and a Latino population of 40%.

Hayes-Bautista and his colleagues emphasize that working-aged Latinos will be expected to contribute to the health care and social security costs of older white workers, and they may be unwilling or unable to assume this "burden of support" if they do not have the education needed to earn high incomes. According to the authors, 57% of Latino adults in California have less than a high school education, versus 22% of non-Latinos (p. 204). This chapter, too, succumbs to the tendency in this book to sketch out "scenarios" involving young Latino workers versus elderly white voters.

Bortz notes that Mexico has always had high inflation, low wages, low productivity, and a smaller share of its population in the workforce than did the United States. Alba joins most of the other contributors to assert that U.S. demand for Mexican workers is what created a binational labor market between the two countries. Reyes notes that even optimistic assumptions about growth and job creation in Mexico leave a deficit in the year 2000 of 5 to 10 million jobs (p. 266).

Chavez examines the effects of Mexican immigrant workers in San Diego in a chapter that includes several reminders of how dated some of the material is. He quotes a mid-1980s study's assertion that in 1990, migrants from other states and countries would be needed to fill 60% of the jobs created in southern California (p. 275), although the story of the early 1990s is job losses and the emigration of middle-class Americans from the area.

Alarcon contributed an interesting case study demonstrating that once labor migration takes root, it sets in motion processes that make it "self-perpetuating." Gonzalez-Arechiga argues that among Mexicans living in border areas, border commuters (with U.S. immigrant visas) are much better off than those who seek work without visas in the United States—precisely the intent of U.S. immigration policies such as IRCA. The mid-1980s is reflected in Muller's paper, which opens with newspaper accounts of "labor shortages" and goes on to describe how the United States can, with immigration, offset "insufficient growth of native-born workers to meet projected employment needs" (p. 369).

meet projected employment needs" (p. 369).
"Sectoral Dynamics" includes four loosely connected chapters, dealing with the shift of U.S. auto parts and assembly operations to Mexico, the substitution of non-union Hispanic janitors for unionized blacks in Los Angeles, a description of California agriculture's depending on Mexican workers while criticizing Mexico for trying to step up exports on the basis of low wages, and a concluding essay posing the important question, Will free trade lead to a convergence that pulls U.S. wages down toward Mexican levels or Mexican wages up toward U.S. levels? Reynolds promises to answer the question in a forthcoming book.

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PHILIP L. MARTIN

Political Culture and Democracy in Developing Countries. Edited by Larry Diamond. Boulder: Lynne Rienner, 1993. 455p. \$49.95.

After a quick start in the sixties, progress in the study of political culture seems to have lagged. Most of the new work on democratization has ignored the construct altogether, focusing on institution building and elite maneuvering instead.

But now that attention has begun to shift from the emergence to the durability of democracies, political culture has come back into play. The dismissal of values as deterministic preconditions now seems misguided. The rejection of political culture as a welter of extraneous preferences that get in the way of template-like theorizing is reminiscent of the scholastic fanaticism of Giles of Viterbo, declaiming in the face of the Reformation that "men must be changed by religion, not religion by men."

The volume on political culture and democracy edited by Larry Diamond therefore comes at an auspicious time. It gathers 13 original articles on the workings of political culture in over 20 countries. The collection contains some excellent pieces. One of the most insightful, by Gehad Auda, analyzes Islamic movements and resource mobilization in Egypt. Auda chronicles the decay of the symbolic capital, the legitimacy of the secular regime, and the growth of the social capital of religious fundamentalism through the takeover of existing institutions and the creation of alternative political networks. Christine Sadowski provides a cogent analysis of the growth of civil society in postcommunist settings. And the introduction and conclusion provided by Larry Diamond are perceptive reviews of the literature.

The rest of the book, however, does not match the standards set by these essays. Too much of the volume rehashes ideas from the sixties with methods from the fifties. Indeed, one of the reasons why the editor's orienting and wrapup essays work as well as they do is that he makes only sporadic reference to what comes in between. This is not unusual in compilations devoted to big-picture topics. A similar deficiency afflicted the series on democratization put together some years ago by O'Donnell, Schmitter, and Whitehead. The fact that many of the case studies function mainly as casual grist or white noise to a larger framework does not necessarily detract from the interpretation, but it does raise doubts about the wisdom of packaging it in book format. One senses that an editorial admonition to keep things jargon-free was translated by some of the contributors as an injunction to dumb it down. The resulting air is that of a textbook or of an uninspired briefing for distracted foreign service officers. All the things that critics find wrong with commentary on political culture—(ad-hocery, discursiveness, casual apercus, etc.) are present here.

Yet the flaws of *Political Culture and Democracy* need not be identified as inherent to research on political culture. To some extent, studies in political culture are simply no longer the flavor of the month, having fallen out of fashion with one or another faction in the discipline. Toward the end of the eighties, Samuel Popkin argued against the misuse of culture, only to wind up insisting on the importance of "ethos"—the same thing under a different name. Culture bashing of this sort keeps academics off the street, but it does not add to the body of knowledge.

In addition, some difficulties may be distinctive to a stage of development in the field—at least insofar as it bound up with comparative politics, rather than intrinsic to the approach itself. While survey research, the instrument of choice in the breakthrough days of political culture, has become routine almost everywhere, the goals of cross-national collaboration have probably become more ambitious. It is no trick to pull off one-shot

surveys in hitherto inaccessible places, but the career trajectories of many foreign scholars and the vagaries of social science funding agencies do not encourage a commitment to data gathering over the long haul. Without comparable time series, it is difficult to sort out blips in public opinion from transformations in political culture.

Nowadays, the field operates according to a two-track model that can be understood partly as a pair of adaptations to disparate political circumstances and partly as indicative of deliberate choice. Innovative work in comparative political culture (e.g., Merelman on the United States and Canada, Putnam on Italy, and Scott on Southeast Asia) comes out of a semiartisanal, eclectic tradition of research that draws on tools from rational choice theory, cultural anthropology, and even the occasional survey. By contrast, with the exception of some of the work of Inglehart and his collaborators, the results of large-scale team projects are mixed. The payoffs that archival data sets and a big-science style eventually produced in the study of American mass politics have yet to appear on the horizon of comparative politics.

The chief problems, however, are conceptual. For the most part, Americanists who study mass politics traffic not in political culture but in political psychology and in an environment where academics worry not so much about the stability as the quality of democracy. The paramount questions still revolve around individual consciousness, and the dependent variables are usually precise and microlevel. The volume edited by Ferejohn and Kuklinski, *Information and Democratic Processes* (1990), exemplifies this approach: theoretically focused, methodologically sophisticated, with lots of measures polished over long periods of time.

By contrast, analysts of political culture generally favor a more Durkheimian view, and many of them work in erratic contexts where lack of funding is not the main thing preventing replication. Collective norms and aggregate cleavages are the units of inference, and the dependent variables tend to be macrocosmic (e.g., regime stability) even when the empirical materials are drawn from individuals. One may criticize political psychology, for its implacable individualism, as "typically American," as well as for a certain imaginative fatigue. All the same, the sheer nebulousness and noncumulative treatment of political culture is daunting.

In effect, political culture has become a baggy monster. The proliferation of idiosyncratic understandings of the phenomenon evident in the volume at hand may reflect dissatisfaction with the narrowness of earlier formulations. But the seminal work of Almond and Verba at least had a consistent theory and a rigorous methodology. The country studies collected here have neither.

Nevertheless, with a bit of pushing and hauling, some coherence can be made to emerge from a consideration of thirty-odd years of work in the field. A heuristic conceptualization of political culture might profitably come in three parts. First are sets of hegemonic values, the preeminent norms of one society or another (e.g., "individualism" in the United States). Next are dissident or counterestablishment orientations. No majoritarian norms are permanent; and even when such norms are massively "characteristic," no society is so homogenous as to obliterate ideological countercurrents. Societies, in other words, have cleavages, latent or otherwise. This focus has important consequences. Emphasis shifts from the sorting of individuals (participant, subject, paro-

chial, ideologues, near-ideologues, etc.) to the multidimensional classification of issues and cleavages. Third, polities have—or claim to have—what Axelrod has called metanorms—overarching rules for adjudicating conflicts between hegemonic myths and countervalues.

This capsule rendition of political culture is incomplete. To begin with, we need to know what cleavages are about. There seem to be a limited number of axial themes: protocols of authority versus values of autonomy, the priority given to accumulation as compared to standards of equity, and "everything else," including preconceptions about the boundaries of the political and the private sphere.

Another problem is that in practice, this theoretical approach presupposes verbalization that is literacy-based. The demonstration that mass publics do not know what they are talking about much of the time fails to deal with this bias. The further challenge, then, is to encompass predominantly oral societies and subcultures, to come to grips with the elusive pervasiveness of the electronic media in mass societies, and to interpret iconic environments and semiotic landscapes that are expressive in ways that political scientists are not trained to examine. This McLuhanesque condition requires the cultivation of a wider range of methods and sensibilities than most academic training imparts.

Such an agenda regarding political culture may lapse into a story about how difficult it is to tell a story—that is, toward a linguistic turn and a methodological obsessiveness of supreme interest to a few academics and incomprehensible to the vernacular world it tries to interpret. This is the perennial danger of the hermeneutic circle, that of projecting unverifiable fancies onto demotic cues. But once it is granted that the steady monitoring of mass politics that has been a hallmark of social science in industrial societies is not practicable in the same way in more tumultuous settings, alternative and possibly complementary strategies must be taken seriously.

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PETER McDonough

Democracy and Development. By Axel Hadenius. New York: Cambridge University Press, 1992. 218p. \$44.95.

Axel Hadenius' Democracy and Development is a worthy successor to Robert Dahl's Polyarchy (1971) in its effort to quantitatively evaluate the extent and the determinants of democracy across nations. In particular, this book focuses on the "varying level of democracy at a certain point in time (1988)" in 132 Third World countries (p. 2). OECD and East European countries are excluded.

Democracy and Development is really three books in one. The first quarter of the book is an extended, thoughtful essay on the varying criteria that philosophers and political scientists have posited for democracy and the difficulties involved in operationalizing those concepts. The second quarter of the book develops a multifaceted "snapshot" index of democracy as practiced in individual countries at a particular point in time. Hadenius finds a U-shaped pattern, most countries having relatively low or high scores, with few in the middle. The second half of the book reviews the arguments and evidence about the effects of a number of possible influences on the level of democracy, including economic development (notably measures of socioeconomic

modernization, economic dependence, and inequality), culture, and institutional background (limited to the role of the military and colonial history). Regression analysis is used to test these variables against the author's index. Hadenius argues that trade with the United States, percentage of the population that is Protestant, and capitalism have a strong positive association—and economic development (as measured by literacy) a weaker positive association—with democracy, while commodity concentration, ethnic and linguistic fragmentation, and military expenditures are negatively associated with it. The final chapter of the volume includes an excellent discussion of problems of attributing causation in quantitative cross-national analyses of democratization.

Readers will find each of these sections valuable. I especially recommend the second part of this book, not just for students of comparative political development but for use in courses in political science methodology. Democracy and Development is much like David Mayhew's Divided We Govern in the care and exceptional clarity with which the reader is guided through the author's thought process, using an informal, almost musingaloud style as the author attempts to construct a quantitative index to measure a dependent variable that many have talked about but few have been able to measure in a plausible way. As in Mayhew's book, readers can see all the gears turning in this exercise. This section of Democracy and Development also provides a wealth of information on individual countries. And while readers may quibble with details of Hadenius' rankings (e.g., awarding Cyprus a perfect 10 democracy score despite the forced division of that island seems a bit of a stretch), they will, justifiably, be widely used by

Most of the limitations of the book flow from its analytical method. Reduction of the many facets of democracy (most notably participation and contestation rights) to a single metric conceals as well as reveals. As Hadenius acknowledges, the focus on a "snapshot" view of the existence of democracy at a particular point in time ignores questions of democratic stability. The use of states as units of analysis means that disproportionate weight in the regression analysis is given to island microstates. And the analysis risks understating the importance of variables for which quantitative indicators are either not available or contain a lot of measurement error and variables that have a nonlinear relationship with the presence of democracy.

The author generally sticks to the measurable and to the aggregate data. The analysis would have been strengthened by a more detailed presentation of data on individual hypothesized determinants of democracy, including the use of scatterplots to identify potential nonlinear relationships and outlying cases that might disrupt—or illuminate—the statistical analysis. Why some countries and countries in particular regions might be persistent outliers from the regression line is not explored. Why are democracy scores so much higher in Micronesia than in the Middle East and North Africa, for example? Hadenius tends to discount regional patterns (p. 145), but this may be because their effect is being undercut by other variables in his model.

Students of political institutions might wish to see more attention given to specific institutional arrangements, such as presidentialism versus parliamentarism, electoral rules, and federalism. The author's failure to say anything about the emerging democracies and dictatorships of the former Soviet bloc (including the successor states to the USSR) is another curious omission. While their exclusion from the author's data base is understandable, some speculation about what the author's findings suggest about prospects for democracy in those countries would have been welcome.

Such omissions should not be surprising in a relatively brief book, however. While *Democracy and Development* is unlikely to be seen as the definitive answer to debates about the social and political determinants of democracy, it should reinvigorate many old debates and spark new ones. Hadenius concludes his volume with stimulating suggestions for future research. His ideas, data and analytical techniques will—and should—be widely utilized and revised by others to retest and refine, refute, or broaden his conclusions.

Brookings Institution

R. KENT WEAVER

The End of Communist Power. By Leslie Holmes. New York: Oxford University Press, 1993. 358p. \$55.00.

This volume provides a comprehensive analysis of political corruption in Leninist systems, with special attention given to the fundamental role corruption played in those systems' delegitimization, disintegration, and ultimate collapse. The study is cross-national, in principle bridging all of the two dozen countries that adopted a Leninist system, though with more detailed attention given to the Soviet Union and China. While sensitive to the cultural and developmental differences among these Leninist states, the author is persuasive in identifying numerous cross-national commonalities in norms of corruption. His analysis reveals a complex relationship between level of development and corruption. For instance, the type and pace of economic growth and level of reliance on coercion are critical to manifestations of, and regime responses to, official corruption.

The volume's title is somewhat misleading in that the author does not assess all of the political, economic, and societal factors that contributed to the collapse of Leninist systems. Rather, he focuses on the role of official corruption and the crisis of system legitimacy in assessing the end of communist power. Even with this focus, Holmes is biting off quite a bit, but he is effective in demonstrating the centrality of corruption and legitimacy crisis to the processes that overwhelmed most communist regimes in the late 1980s and early 1990s.

A major strength of this volume is its theoretical treatment of both legitimacy and corruption. These nebulous and imprecise phenomena are often defined in very subjective ways. As a consequence, much work done in this area has been interpretive and anecdotal, rather than systematic. Holmes grounds his empirical study of the Soviet and Chinese cases in a theoretical framework based primarily on legitimation and the concept of crisis. He builds on the literature while providing a more rigorous and useful definition of corruption—a definition advantaged by encompassing both the "hard" and "soft" dimensions of objective and perceptual elements. His taxonomy of 20 forms of corruption spans a wide array of actions (e.g., political patronage, embezzlement, and deliberate dereliction of duties), with each form explored in a broad overview of Leninist countries.

Holmes is clearly one of the few people who have the

requisite knowledge base to provide a comparative assessment of developments in most Leninist systems. He is quite effective in illuminating the roots and logic of the anticorruption campaigns that emerged in the USSR, China, and elsewhere in the 1970s and 1980s. Occasionally, his country surveys are rather anecdotal. But the studies of the USSR (1966-87) and China (1966-86) are systematic and well crafted. Holmes makes good use of published reports of official corruption to explore a wide range of questions. The author's cautious conclusions confirm, and add to, our often limited knowledge, demonstrating for both the USSR and China that (1) there was increased reporting of corruption from the late 1970s into the 1980s, (2) an increased level of officials identified with corrupt activities, (3) top-level leadership changes were correlated with reports of corruption, and (4) there was an eventual move to go after high-level Party officials in the Gorbachev and Deng periods. Overall, while this study does not permit Holmes to rank-order, in importance, the factors leading to official corruption, it does allow for an illumination of the systemic and regime conditions conducive to official corruption in the Leninist setting. Some speculation is unavoidable, but Holmes's discussion of the costs and benefits of corruption yields especially insightful conclusions.

We are only now beginning to see the publication of serious studies dealing with the crises of mature Leninist systems, and this volume must be counted among those which effectively illuminate the contradictions that overwhelmed advanced communist states. Holmes demonstrates that Leninist system corruption and anticorruption campaigns were in part a product of a transitional period, in which regimes were moving away from traditional models and ideological formulations toward capitalist system structures and norms. A profound identity crisis of irreconcilable political and economic cleavages overwhelmed regimes increasingly committed to root-and-branch reform. The regimes' own reporting of official corruption became vet another symptom of a general tendency to emphasize performance and what Holmes identifies as the legal-rationale mode. Yet this proved incompatible with the Marxist-Leninist model. As this study indicates, the complex interplay of positive and negative consequences of regimes publicly fighting various forms of corruption (e.g., patronage, second economy) only undercut system and regime legitimacy. Indeed, the political system was weakened by these developments. Institutional cleavages and a general confusion overwhelmed the various party and state agencies charged with addressing problems of corruption.

One of the most interesting features of this volume is Holmes's seven-stage model of political transition to postcommunism. In the process of presenting his model, Holmes offers some fresh and provocative thoughts on communism and postcommunism and their relationship to our understanding of modernism and postmodernism. He argues that in certain respects, the Leninist system represented an extreme form of modernity, while in other respects, it was premodern or "extramodern." At the most basic level, Leninist systems had many modern values (often developed to an extreme level) but few modern forms. The legitimacy crisis, to which he gives considerable attention, was a natural outcome. Holmes concludes that postcommunist states exhibit many features associated with early modernity (e.g., authoritarian and autocratic political tendencies, more distinct class structure with group cleav-

ages), suggesting that these societies will continue to struggle with political and socioeconomic dilemmas long characteristic of the "modern" West.

This volume was authored in the late Soviet period, but the analysis and conclusions nicely anticipate post-Soviet developments now underway in Russia and Eastern Europe. The theoretical discussion suggests why corruption will be of continuing significance to the politics of postcommunist and transitional societies. Having consulted this volume, readers will be in a better position to assess those complex and dynamic developments as they continue to unfold.

University of Arizona

JOHN P. WILLERTON

Power Without Force: The Political Capacity of Nation-States. By Robert W. Jackman. Ann Arbor: University of Michigan Press, 1993. 194p. \$39.50 cloth, \$14.95 paper.

Robert Jackman enters the lists of contributors to state theory with an elegant treatise, admirably succinct and sophisticated. In a mere 162 text pages, he forcefully engages much of contemporary state theory, and stakes out a distinctive niche for himself. Those familiar with the Jackman corpus of scholarship will expect clarity of thought and a penchant for proposing parsimonious categories for complex processes; they will not be disappointed.

The critical vehicle for conceptual purchase over "state," argues Jackman, is the notion of political capacity. The essence of capacity is the ability of leaders to exercise rule without force: in other words, effectively to hold power, understood as comporting authority. Political capacity in turn depends upon two critical factors: legitimacy and organizational age.

Legitimacy rests upon consent of the governed, and is thus implicitly measured by the absence of physical coercion. The Gramscian interpretation of "hegemony" wins an approving nod; civil society for states endowed with an ample political capacity becomes a willing subject. Legitimacy, for Jackman, is not regime-specific; it "simply requires a degree of acquiescence, an acceptance of the political order as generally reasonable, given the known or feasible alternatives" (p. 99, emphasis in the original).

Institutionalization is likewise central to political capacity. One important measure is simply survival over time. Institutions, argues Jackman, acquire value in the public eye through their routinization over time and the familiarity of long usage. New states are thus intrinsically fragile. Conversely, organizational age may be taken as a proxy of capacity. The longer a given institutional framework has persisted, the more likely is its continual survival.

The footprints of Max Weber and Samuel Huntington are clearly visible in this line of analysis. But Jackman is not simply a disciple of any of his intellectual predecessors; his particular synthesis of state theory is distinctive and original. His conceptual contribution is rooted in a wide-ranging inventory of political development debates, and a rich knowledge of the empirical world of nation-states. Unlike a large fraction of contemporary state theory, his reflection is not primarily based on observation of the limited universe of European or advanced industrial polities, but is genuinely global in scope.

By methodological disposition Jackman is drawn to-

ward an effort for measurement. Happily the temptation is only half-heartedly pursued. He devotes a chapter to the measurement of political capacity, but exercises appropriate restraint in actually implementing any such design. Legitimacy is at once fundamental and yet elusive, a quintessentially qualitative notion. Proxy indicators such as amounts of physical force or violence, security expenditures, or size of armed forces are dubious measures when stripped from their context. Institutionalization cannot be adequately inferred from so simple a notion as chronological age. State socialism, after all, progressively disintegrated and the decay of legitimacy of Marxism-Leninism as regime ideology was directly proportional to aging. So also has political capacity of many African states progressively declined over time, opening the way to the African variant of the Huntingtonian "third wave" of democratization experiments, however uneven their success.

Perplexing as well is the Jackman aversion to acknowledging that states can be usefully seen as macro-historical actors, an idea which he dismisses as posing "overwhelming problems of reification" (viii). Indeed so, but states do make war or peace, consume important fractions of the national income, engage in "authoritative allocation of values," and issue standing commands in the form of laws. Although their sovereign will may be institutionally fragmented, and is certainly the object of fierce struggle on the part of social groups and leading individuals, at the end of the day actions occur bearing the decisive signature of the state. Equally puzzling is his stigmatization of "historicism" as a regressive analytical trend, as if specificity of context and trajectory were not important conditioning factors to weigh against comparative regularities.

Among the particular attributes of given nation-states which comparative analysis needs to consider but often overlooks is their cultural configuration. Here Jackman deserves high praise for specifically including ethnicity as a variable. Again one may debate the completeness of this dimension of his analysis; more needs to be said about the relationship between the variable importance of identity politics and overall patterns of state legitimacy. But successful accommodation of communal difference is becoming increasingly critical to sustaining legitimation, and even in this decade to state survival.

These reservations notwithstanding, I heartily recommend this slim volume to all students of political development and state theory. The synthesis of evolving scholarly debates since the 1950s on these themes is a masterpiece of compressed yet comprehensive critique. The brief for political capacity as master concept is both stimulating and provocative. The Jackman volume is a major intellectual achievement.

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CRAWFORD YOUNG

Money, Time, and Politics: Investment Tax Subsidies and American Democracy. By Ronald F. King. New Haven: Yale University Press, 1993. 536p. \$45.00.

Taxation and Democracy: Swedish, British, and American Approaches to Financing the Modern State. By Sven Steinmo. New Haven: Yale University Press, 1994. 280p. \$32.50.

Ronald King begins his book with a quote by Joseph Schumpeter that must warm the heart of every scholar

who studies tax policy: "The spirit of a people, its cultural level, its social structure, the deeds its policies may prepare—all this and more is written in its fiscal history, stripped of all phrases" (p. 1). Yet the tax side of fiscal history is rarely studied, in sharp contrast to the voluminous literature on government expenditures. One disincentive is surely the complexity of modern tax systems. Tax attorneys spend a lifetime mastering the intricacies of the U.S. tax code. It is little wonder that most scholars have preferred to study policies that can more easily be summarized and compared.

In Taxation and Democracy, Sven Steinmo steps boldly into the gap with a comparison of tax policy in Sweden, Great Britain, and the United States. Steinmo organizes his study around both a "common path" and "divergent patterns." The common path consists of rising progressivity of taxation from the turn of the century to the 1940s, as tariffs were replaced by progressive income taxes. Most recently, tax policy has moved in the opposite direction. In all three countries, tax reforms adopted in the 1980s reduced the progressivity of the tax systems.

The divergent patterns consist of quick-and-dirty sketches of the tax system in each of the three countries. The American tax system is riddled with loopholes. As a consequence, the tax system in the United States is both extraordinarily complex and limited in its revenue yield. The Swedish tax system is characterized by stability, high revenue yields, and low taxes on income from capital. British fiscal history is marked by instability, as each new government undertakes to overhaul the tax system and reverse the policies of the preceding government.

Most of Steinmo's analytic effort goes into explaining the cross-national variation. His explanations rest upon characteristics of each country's political institutions. The reason that the U.S. tax code is filled with loopholes is explained by the power and independence of members of Congress and their electoral incentives to champion tax breaks for interest groups who are important sources of political support. The continuity and rationality of Sweden's tax policy is due to electoral rules that allowed the Social Democratic party to maintain a majority of seats in the upper chamber even though the party rarely received enough votes to obtain a majority of seats in the lower chamber. British instability stems from the combination of a first-past-the-post electoral system that exaggerates the impact of slight changes in the vote and centralization of power in the hands of the majority party in Parliament. All of these explanations are conventional and plausible. Explanations can hardly become conventional without a certain degree of plausibility. In the case of Sweden, Steinmo goes beyond the conventional wisdom in his discussion of how the constitutional reform that eliminated the upper chamber in 1970 had the effect of undermining Social Democratic dominance, thus reducing the incentives of the bourgeois parties to compromise on tax policy or any other

The most interesting pattern, however, is the pattern common to all three countries. Steinmo argues that the wave of regressive tax reforms that swept Europe and the United States in the 1980s was a consequence of changes in the world economy that have made it easier for firms to relocate production to avoid unfavorable tax environments. This assertion, however, is too simplistic. Within the standard economic framework for under-

standing the effect of profit taxes on investment, there are many different ways governments can respond to increased capital mobility. The fact that all three governments responded by broadening the tax base and reducing tax rates requires explanation. In theory at least, governments could equally well have responded by keeping tax rates high and increasing targeted tax incentives for investment in the domestic economy.

It is perhaps inevitable, in a book that covers the entire tax system in three countries from the 1800s to the present, that the descriptions and explanations are somewhat superficial. In Money, Time, and Politics, Ronald King devotes twice as many pages to one aspect of the tax system (the taxation of corporate income) in one country (the United States) from 1945 to 1980. This allows King to investigate in much greater depth the economic and distributional trade-offs that constrain tax policy everywhere. The basic dilemma that King explores is between the popular demand for the rich to pay more and high-profit taxes' negative effects on investment, employment, and growth. King describes how Democratic presidents, as much as Republican presidents, sought to rework the tax code to encourage private investment, even at the expense of reducing the effective progressivity of the tax system. The major difference between the parties was that Democratic presidents favored targeted tax incentives that minimized the decline in progressivity of the tax system, while Republican presidents were more likely to favor broad reductions of the tax rate paid by firms and shareholders.

The two authors describe the U.S. tax system in strikingly different terms. King dismisses the numerous loopholes emphasized by Steinmo as being a side-show. The tax revenue lost from the multitude of tax expenditures that benefit small groups is small, King claims. The tax expenditures that reduce revenues significantly (deduction of mortgage interest payments for owner-occupied housing, tax-free status of interest from state and local bonds and deduction of state and local taxes, deductions for employer-provided fringe benefits such as health care and pensions, and tax incentives for capital formation) are either received by broad groups of voters or, in the case of investment incentives, adopted because broad groups of voters stand to benefit from the encouraged activity. While Steinmo focuses on the role of Congress, King sees the president as the most important actor. Indeed, King's argument is that presidents have centralized decision making regarding tax policy in response to the fragmentation of political power in Congress that Steinmo observes.

If King disputes Steinmo's portrait of the United States tax system as incoherent, Steinmo challenges King's apparent belief that the trade-off between investment incentives and distributional effects is less severe in social democracies. In fact, Steinmo's description of Swedish taxes could not be a better illustration of the willing sacrifice of distributional goals for the promotion of capital formation that King uncovers in the United States, yet the normative implication of the trade-off in tax policy between the effective rate of taxation of income from capital and the level of private sector investment is easily exaggerated. The efficiency and distributional effects of the tax system cannot be evaluated in isolation from the expenditure side of fiscal policy. A government that takes in a large share of national income through efficient, regressive taxes in

order to finance redistributive expenditures may do far more to reduce income inequality than a government that attempts to tax the rich.

In conclusion, Taxation and Democracy is a fast-paced, well-written book that makes interesting comparisons and, for the most part, conventional arguments. Steinmo's book would be an excellent choice for an undergraduate course in comparative public policy. King's book, with 450 pages of text devoted to the intricacies of United States tax policy from Truman to Carter, is unlikely to fit within the constraints of courses taught at any level. Yet scholars who seek a deeper understanding of tax policy would be well-advised to find the time to read Money, Time, and Politics. Because King has studied the debate over taxation so carefully, he is able to convey the economic as well as the political considerations that shape the taxation of income from profits in the United States and elsewhere. In the final analysis, the impact of different democratic institutions on tax policy, though interesting, is of secondary importance compared to the economic and political concerns that King documents.

University of California, Los Angeles MICHAEL WALLERSTEIN

Mothers of a New World: Maternalist Politics and the Origins of Welfare States. Edited by Seth Koven and Sonya Michel. New York: Routledge, 1993. 447p. \$49.95 cloth, \$16.95 paper.

The history of women's involvement in welfare state formation poses interesting questions for both scholars of state building and scholars of feminism. Looking across Western democracies in the late nineteenth and early twentieth centuries, the authors in this volume reveal the important role that middle-class women reformers played in securing state assistance for women, children, and families. This history complicates traditional arguments regarding the role of industrialization and male labor politics in welfare state formation. Further, at a time when today's women's movements tend to emphasize equity and individual rights, the centrality of maternalism to turn-of-the-century feminism raises issues about strategy and identity in women's politics.

Mothers of a New World comes out of a series of conferences that were held at Harvard's Center for European Studies in 1986 and 1987. Many fine scholars participated at these gatherings; and even those whose work is not represented here (e.g., Barbara Nelson, Karen Offen, and Theda Skocpol) have continued to contribute to scholarly work on the issues discussed in this volume. This collection of essays is a significant contribution to scholarship on gender and politics and to the study of political development in Western democracies.

Studies of state building and political development have traditionally failed to consider gender. By focusing on the role of industrialization, militarism, and class formation, women are treated as secondary players in the history of political and institutional change. But as the essays in this volume amply demonstrate, gender and women's politics were important factors in political development of Western democracies. As the subjects of state interest, women in militarist nations (as Alisa Klaus discusses in respect to France) were encouraged to bear children, and in industrial economies with employment

concerns, they were constrained from working beside men in factories. As advocates of social provisioning, women gave voluntary assistance to poor families in private programs, which were often models for public services. They also articulated a self-defined feminine sensibility of social caring to justify state aid to women and families. The maternalist politics of women reformers was sometimes associated with efforts to expand women's political rights, sometimes not. Maternalist politics was also sometimes resisted by male politicians and sometimes coopted to please the church, the army, or male unions. The fit between maternalist policies, women's politics, and state building was a politically contested issue.

In feminist theory, there is currently a debate over the logic and effects of strategies of equality and strategies of difference for women in politics. Historical consideration of the politics of maternalism in the late nineteenth and early twentieth centuries is unlikely to settle this debate, for it reveals that maternalism was used both to justify claims of political equality and to legitimate women's exclusion from formal politics. What it also shows (as many feminist theorists, such as Carole Pateman, now argue) is that the difference/equality dichotomy in politics is often a false construction. Surely, maternalism is an irreducible difference between the sexes. Men cannot carry or bear children. Yet the political construction of this difference in Western democracies said little about the resulting political gains and status of men and women. So while there may be a point in critiquing liberal universalism (as many feminist theorists do), the essays in this volume suggest that there is little merit in drawing essential lessons about the politics of maternalism and difference that were associated with a broad range of political efforts and outcomes.

One weakness of many of the essays in this volume is that they consider women as advocates of social policy, rather than viewing them as the recipients of such programs. As Christoph Sachsse says in his essay on German welfare state formation, "Women have always been and remain overrepresented among the recipients of poor relief and welfare" (p. 136). The relationship between women reformers and women clients is complicated not only by class but also (as Eileen Boris notes in her essay on the United States) by race and ethnicity. Such complexities matter, since eligibility for social welfare and the receipt of social welfare both mark the terms under which women are incorporated into the political system. Some women reformers advocated what Seth Koven calls "civic maternalism" as a way of recognizing the contribution of all women to the polity, and therefore of their right to participate in the political process. But as Susan Pedersen shows in her study of France, social provisioning could also be used to further differentiate the citizenship status of men and women, and to weaken the political claims of working-class women by reminding them of their duties to the family and the nation. Thus, it is insufficient to consider gender and welfare state formation from the perspective of women reformers. More work needs to be done on the role of women clients in policy formation (as Molly Taylor Ladd does in her study of the Sheppard-Towner Act in the United States, and Jean Quartaert, in her discussion of class and welfare policy formation among women in Germany) and as citizens whose political status is marked by their position as welfare recipients.

War, economic change, democratic expansion, race

and class relations, and gender relations all contributed to the context in which women reformers used maternalist rhetoric to seek new social provisions for women and families a century ago. Despite the vast differences among the countries studied here (France, Britain, the United States, Germany, Sweden, and Australia), they had in common the importance of women's maternalist politics to their early welfare state formation. The consequences of maternalist campaigns may be as varied as the contexts in which they occurred; but in all of these cases, women's political efforts left a substantial mark on social policies and state institutions. This volume is a welcome correction to the previous neglect with which these campaigns have been treated by social scientists.

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GRETCHEN RITTER

Retirement of Revolutionaries in China: Public Policies, Social Norms, Private Interests. By Melanie Manion. Princeton: Princeton University Press, 1993. 196p. \$35.00

Melanie Manion's extensively researched, neatly organized, and tightly argued book is a welcome addition to our knowledge of Chinese politics, particularly in light of the paucity of monographs dealing with administrative aspects of China's huge bureaucracy. Marshaling an impressive array of Chinese official documents (some obviously internal publications) as well as extensive interviews with both retired revolutionaries and the younger generation of cadres who benefited from the retirement of senior cadres, she successfully manages to incorporate all relevant information, as well as her interesting analysis, within a thin volume of 196 pages.

This book is a case study of a series of policies specifically packaged to induce the voluntary retirement of old revolutionaries. Particularly targeted were cadres who joined the revolutionary movement before 1949. Such individuals (presumably having made a contribution to the foundation of the People's Republic of China), thereafter gradually moved into key positions in the ruling structure, and thereby defined the contour of Chinese politics over the subsequent 40 years. Throughout their careers, these cadres frequently engaged in deadly factional, ideological, and policy struggles among themselves. Many of them fell victim to purges during the Cultural Revolution and were later rehabilitated, reinstated to their previous positions, and then retired through the Deng-era policies that Manion details in her book. Deng Xiaoping's success in removing these aging cadres paved the way for the rise of a younger generation of technocrats, while avoiding the Maoist mode of political purges. Furthermore, this generational change in the Chinese leadership parallels the well-known shift in the regime's goals from Mao's revolutionary changes to the more contemporary pursuit of economic development.

It would be impossible for the Chinese to develop any rational system of managing their huge bureaucracy without first setting up an administrative norm of retirement. For this reason, Manion's detailed analysis of an evolving cadre retirement system—and particularly her conceptualization of this evolution as a process of institutionalizing a regularized administrative practice—is a fresh and welcome approach. It will certainly stimulate scholarly interest not only in retirement but in other

aspects of the cadre management system as well. The civil service system that China will hopefully develop in the coming years may be one such area of interest.

As the book's subtitle indicates, Manion's central organizing concept is captured by her term *norm*, which she defines as "a pattern of action, accompanied by an understanding of the pattern as a social standard of propriety," whose violation will invite social disapproval (p. 23). The key question raised in the book asks, "Has a norm of cadre retirement emerged?" (p. 153). If cadre retirement has become a norm, then, presumably, the social expectation and sanction, rather than any enforcement backed by state authority, will work to invoke voluntary compliance of cadres reaching the retirement age.

Manion's answer to the central question is equivocal: "It is fair to conclude that the process [of establishing an official retirement policy] is fueling the emergence of a norm of retirement for the overwhelming majority of Chinese cadres" (p. 153). She attributes this somewhat ambiguous finding to the "ambiguous, ambivalent, inconsistent, and discriminatory" nature of the measures adopted by the Chinese policymakers in their retirement policy (p. 155), a nature that violates some of the theoretical principles from the chapter specifying how norms emerge. Chinese policymakers made too many concessions to the interests of the targeted group at every phase of elaborating and implementing the policy. Nonetheless, she argues, other factors (e.g., the selfinterest of both aging revolutionaries and the younger generation of cadres and the wisdom of the strategy of 'small wins'') operated to help the gradual emergence of effective retirement norms.

Manion's qualified and somewhat conflicting conclusion leads one to wonder whether the special retirement program is an appropriate case to study the normbuilding process, particularly along the line of theoretical insights drawn from experimental social psychology. The special retirement program involves too many peculiarities to establish a general system of norms. As the Chinese term *lixiu* ("leaving the job to recuperate") indicates, the policy package included in the program was considered as a one-shot deal only applicable to the old revolutionaries. Moreover, the targets of the policy

Politics East and West: A Comparison of Japanese and British Political Culture. By Curtis H. Martin and Bruce Stronach. Armonk: Sharpe, 1992. 335p. \$49.95 cloth, \$16.95 paper.

If the title of Curtis H. Martin's and Bruce Stronach's book pays homage to Rudyard Kipling's oft-quoted line, "East is East and West is West, and never the twain shall meet," the theme of this book could not be further from the quotation's spirit. They are not saying that East and West are distinctively different. Instead, they emphasize the similarities of British and Japanese political culture. They argue that consensus is "ingrained as a social and political value" in both Britain and Japan and that it is "an expressive factor of voting behavior" and policymaking (p. 229). Martin and Stronach particularly emphasize the "collectivist traditions and acquiescent behavior" of the British and the Japanese, though they argue this behavior is more pronounced in the Japanese case (p. xv).

They do, however, draw a strong contrast between the historical origins of British and Japanese democracy. They emphasize the longevity and "homegrown" quality of British democracy, in contrast to the recent nature of Japanese democracy. The elite origins of Meiji Constitution and the Allied/American-imposed postwar constitution, they claim, make Japan a less legitimate and a potentially unstable democracy (p. xi). They interpret the differences they find in the political culture of the two countries through the lens of these diverging constitutional origins. Unfortunately, they never adequately draw out the implications of this contrast.

With more than 308 pages of densely printed text (excluding the index and references), 45 tables, and a massive bibliography of some 450 entries, Martin and Stronach must have a sense of accomplishment on seeing their book come to print. A short, well-written introduction describes the key theoretical concept of the book, political culture. Their definition of political culture (drawing heavily on Sidney Verba's definition) connotes a system of values, beliefs, and symbols about the patterns of political institutions and interactions in which there are clear linkages between individual behavior and political events (p. x). The language and structure of analysis is clearly framed in the structural-functionalist approach of Almond, Verba, Pye, and

are especially proud of their political institutions, the Japanese express pride toward their culture and economic successes. They attribute this difference to the differing political trajectories of the two countries: the defeat of Japan in World War II profoundly affected Japanese trust of political institutions (p. 65). Their argument, although plausible and ostensibly historical, seems to overlook the powerful role that political institutions have had in the construction of a Japanese cultural identity.

Part 2 focuses on Japanese and British attitudes toward political participation. It continues the discussion of the attitudinal basis of popular support and documents the range of participatory activities in Britain and Japan. One of their conclusions is that traditional values continue to play an important role in voting and in the level of political participation in both Britain and Japan. They argue that the political passivity inherent in traditional values has inhibited participatory democracy in both countries (p. 173). Nevertheless, the value given to consensus, they conclude, "has a much greater effect on voting in Japan because it is a so much more deeply ingrained social and political value" (p. 229).

Part 3 continues to look at the influence of values and attitudes through the lens of policymaking. Using their political culture approach, they compare five different groupings of institutional relationships encompassing various configurations of policy actors (ministers, bureaucrats, party officials, legislatures, interest groups, and local government officials). Here, they conclude that "both Japanese and British decision making rest on the foundations of strong elite consensus and control, buttressed by tenacious though different collectivist norms and traditions, and by an acquiescent citizenry" (p. 301).

Unfortunately, there is no concluding chapter. This is where they might have tied together a theoretical argument about the relevance of the political culture perspective. While they acknowledge that the debate around political culture has been controversial, their response is to write that they "shall attempt to keep these criticisms and refinements in mind" (p. xii). Essentially, they sidestep the question of causality (culture causes behavior) but still view political culture as creating limits, providing resources for problem solving, and conditioning behavior (p. xi).

If they had attacked this political culture debate head on, the limitations of their argument would have become clearer. The problem is that the political culture approach (at least, the one they utilize) leads to a skewed view of politics. Their approach inherently overemphasizes consensus and tends to ignore societal conflict, that is, why, when, and how dissatisfaction erupts. Since this is a book published prior to the 1993 rise of Japan's Hosokawa government, it cannot be faulted for making the usual claims for the impervious power of the Liberal Democratic party. However, we can ask how their political culture approach might have helped us to explain this transformation. Essentially, political culture acts as a residual explanatory category—explaining everything yet nothing. Moreover, there is a tendency in this type of approach to ignore or downplay the variations within a society. The result is that conflict between societal subgroups is, for the most part, left unexam-

Martin and Stronach's book provides a well-written synthesis of survey results. It will be useful for scholars who want to familiarize themselves with recent survey work related to political culture, participation, or policy-making in Japan and Great Britain. Their book will be less helpful in unraveling the complexities of political change in these two countries. In essence, Martin and Stronach have not addressed the fundamental theoretical problems surrounding the structural-functionalist approach to political culture.

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SUZANNE B. RYAN

Lessons of Struggle: South African Internal Opposition, 1960–1990. By Anthony W. Marx. New York: Oxford University Press, 1993. 347p. \$16.95 paper.

Anthony Marx's study of the South African internal opposition to white supremacy from 1960 to 1990 is illuminating, detailed, and theoretically sophisticated. Lessons of Struggle offers an analytical framework that privileges the role of ideology in the making of history. More specifically, Marx argues that the way in which oppressed people interpret their own experiences is decisive in determining their patterns of resistance to repressive rule and their aspirations for an alternative and more equitable social order. Subjective interpretations, however, do not emerge from a vacuum; they are molded and constrained by material constraints. In the tradition of Antonio Gramsci, Anthony Marx emphasizes the mutually conditioning interactions between ideologies and structures in the process of social change. It is from these interactions that oppressed people draw the lessons of their struggle.

In South Africa, under apartheid, race became the defining category: it was the single most important factor in determining a citizen's life chances. Through its total control of state power, the white minority institutionalized racism and monopolized wealth, status, and privilege. The institutionalization of racism confined the black majority to a powerless and destitute existence. Black South Africans were politically emasculated and transformed into exploitable units of labor. Not surprisingly, the indignities of racism and the realities of material deprivations decisively shaped the ideological configurations of black opposition to apartheid.

While race has inevitably colored oppressed peoples' perceptions of the dividing line separating them from their overlords and produced a black nationalism pitted against a white nationalism, Marx cautions us against oversimplifying the ideological orientations of South Africans. Lessons of Struggle shows that despite its overwhelming significance, race is not-and has never been-the sole identity to which South Africans have given their loyalty. Ethnic, class, and religious identities have also claimed their allegiances. These competing identities, as Marx insightfully demonstrates, are not immutable: they are in fact quite malleable. Elites manipulate them for their own political ends, but they cannot do so as they please. "Images of collective identity" mobilize people only when they "resonate with the masses under particular historical circumstances" (p. 236). Evoking the power of normative symbols and the experiences of daily reality, such images have contributed to the development of rival ideological families.

For instance, Marx explains how during the early 1970s, the search for "psychological liberation" induced the emerging Black Consciousness movement (BCM) to

adopt a strategy of inclusive "racial assertiveness." Unlike the earlier Africanism of the Pan-Africanist Congress, which emphasized the exclusive racial unity of Africans, the BCM developed a broader notion of blackness. Black did not merely refer to Africans, it also encompassed Indians and coloreds. Blackness thus embraced all oppressed South Africans, irrespective of race.

Marx rightfully emphasizes that the more inclusive stance of the BCM did not imply "multiracialism," for whites were not welcome to join the movement. As he explains, the "everyday experiences" of the advocates of black consciousness "reinforced the idea that oppression was linked to race" (p. 43). It was only when blacks had achieved unity and a sense of their humanity that whites would begin to think about surrendering their power. According to Marx, the immediate goal was black psychological emancipation. The struggle against white supremacy was thus waged at the level of ideas. Supporters of black consciousness were convinced that apartheid would crumble once blacks had rejected their mental enslavement to white supremacy, "regardless of the intentions of those in power, or the balance of material forces controlled by the state and its opponents" (p. 47).

By the mid 1970s, the focus on psychological emancipation had fostered a widespread culture of black defiance, particularly among the township youth. Submissive quiescence to apartheid gave way to an acute sense of moral outrage. Marx persuasively argues that such anger, combined with the deterioration of black living standards, led to the Soweto revolt of 1976. While the revolt failed to dislodge the white minority from power, it indicated a turning point in South Africa's history.

The Soweto uprising marked "the beginning of a decline in the mass influence" of the BCM (p. 72), as all hopes of using the sole weapon of psychological liberation to overthrow apartheid vanished under the weight of state repression. The BCM's eclipse and fragmentation facilitated the political ascendancy of the African National Congress (ANC) and trade union organizations. Marx thoughtfully argues that the emergence of these movements prompted a redefinition of identities to suit new forms of struggle and generated new ideological constructs.

While the BCM—reorganized in 1978 under the name of the Azanian Peoples Organization (AZAPO)—embraced socialism and the concept of class, it remained deeply committed to the "ideals of racial identity" (p. 88). As Marx remarks, AZAPO defined blacks "as a 'race of workers,' thereby collapsing the class category of workers into the racial category of blacks" (p. 87). According to the author, AZAPO became so absorbed in doctrinal debates that it failed to concentrate on the more crucial task of organizing a mass following. By the early 1980s, the search for doctrinal purity transformed the once-dominant BCM into a relatively weak political organization. Taking advantage of this weakness, the ANC in exile successfully imposed its hegemony over the internal opposition movement in South Africa.

Marx's explanation of the rise of the ANC is well argued and convincing. He shows how the exiled leadership's patience and discipline contributed to make the organization into the symbol of African opposition to apartheid. The ANC was greatly helped by Nelson Mandela, whose long imprisonment only enhanced his already mythical stature at home and abroad. Moreover, the ANC developed an international network of support

that enabled it to acquire the material resources with which to absorb thousands of young exiles who wished to join Umkhonto we Sizwe—the movement's armed wing. While Umkhonto's violent resistance never posed a serious military threat to the white state, it greatly increased the ANC's prestige and popularity. Material and organizational resources were thus decisive in establishing the supremacy of the ANC over the internal opposition. Yet material resources by themselves, Marx argues, do not fully account for the movement's phenomenal success. Clearly, other factors came into play.

Chief among them was the ideological convergence of the ANC with the United Democratic Front (UDF). Founded in 1983, the UDF "grew out of local civic and professional groups" that had strong connections to the ANC (p. 14). These connections rapidly led the front into adopting the fundamental tenets of the ANC's seminal Freedom Charter. The charter purposefully "eschewed absolute ideological coherence" to embrace a flexible and pragmatic vision of a multiracial struggle against apartheid (ibid.). The goal was to forge a nonracial national identity that would serve as the basis of the "most inclusive possible unity" of all forces opposed to white supremacy (ibid.). As Marx contends, "Not only did such a national identity avoid the exclusion of any race or class, but it also was elastic enough to accommodate the broadest possible array of groups and ideas" (p. 121).

Thus, the ultimate aim of "charterism" was to offer an alternative social order that would be acceptable to all except virulent white supremacists. A "historic compromise" ushering in "majoritarian rule with various checks" (p. 262) became virtually inevitable when, in 1990, President De Klerk unbanned the ANC and accepted the negotiated demise of apartheid.

Marx does not imply that the road to a democratic postapartheid society will be easy. He acknowledges the difficulties stemming from the very idea of a "historic compromise." Among radical ANC supporters, particularly in the Congress of South African Trade Unions and the South African Communist party, there are clear apprehensions that an "accommodation among elites" (p. 262) will not resolve the acute divisions and injustices of class and that the promise of a socialist South Africa will give way to a pragmatic acceptance of capitalism. Among conservative Africans, such as the Zulu chief Gatsha Buthelezi, there is fear that the triumph of the national identity espoused by the ANC will undermine their tribal bases of support and thus confine them to political oblivion. For Marx, it is this fear (not ethnic enmity) that has generated the escalating violence between African supporters of the ANC and anticharterist adherents of Buthelezi's Inkatha party. Finally, the author does not dismiss the dangers of a right-wing white backlash degenerating into indiscriminate vigilantism.

Marx, nevertheless, remains optimistic, highlighting the fact that "most South Africans have remained committed to the formation of a nonracial society, in which races are not merely accommodated but cease to be divisive" (p. ix). The author's assessment seems warranted. Since the publication of Lessons of Struggle, negotiations have led to a major agreement for multiracial elections and majority rule. Thus, even if South Africa's future is as yet uncertain, there is clearly reason for hope.

The author's cautious optimism is grounded in his thorough familiarity with the South African terrain. With that knowledge, Anthony Marx has produced the

most comprehensive and informed account of black South African opposition to apartheid for the 1960–90 period. Lessons of Struggle is a must read for anyone interested in social change, for its insights apply beyond South Africa. Marx's outstanding study is timely, intellectually rewarding, and thought-provoking. It deserves a wide audience.

University of Virginia

ROBERT FATTON, JR.

Encounters with the Contemporary Radical Right. Edited by Peter H. Merkl and Leonard Weinberg. Boulder: Westview, 1993. 277p. \$65.00 cloth, \$19.95 paper.

This book provides an insightful account of the current state of the radical Right in numerous countries. The collection of essays, which are loosely organized according to region (Western Europe, Eastern Europe/Israel, Anglo-American Democracies) clearly transcends the Eurocentrism or American (German, British, etc.) "exceptionalism" that often underlies the approach to this subject. Taken together, the various contributions demonstrate the almost global dimensions of the phenomenon of right-wing extremism and support the view that we are dealing with a "normal" pathological condition in modern dynamic societies.

But the value of this volume is more than its geographic reach. It raises basic conceptual questions concerning underlying assumptions in the analysis of the radical Right. Thus, Leonard Weinberg, in the introduction, points out the confusion in the use of various terms (extreme Right, radical Right, fascism) and offers a working definition that starts with a concept of political extremism, a basic differentiation of left and right, and the identification of themes common to the radical Right, such as nationalism and ethnocentrism (not necessarily anti-Semitism), the use of conspiracy theories, and populism (pp. 7–8).

Moreover, the role of historical continuities and contexts (esp. the relationship of the contemporary radical Right to historical fascism) is given considerable weight in the book. For example, in an extremely informative and comprehensive examination of the French Front National (FN), William Safran provides a valuable interpretation of the FN's electoral performance and support patterns while at the same time analyzing similarities and differences between Le Pen today and Hitler in the 1920s and between the Poujadist movement and the FN. In a similar vein, Eckart Zimmermann and Thomas Saalfeld distinguish three waves of right-wing extremism in West Germany from the end of World War II to unification and suggest more discontinuities between the last wave of the 1980s and its predecessors than between the first two waves and the Nazi regime. Likewise, Trond Gilberg, in his contribution on Romania, discusses to what extent historical legacies of interwar fascism and the Ceaucescu regime (e.g., extreme nationalism, ethnochauvinism, agrarian populism, and native fascism) provide the basis for current expressions of the radical Right and impose limits on democratic renewal in contemporary Romania. The theme of historical continuities is taken a step further in Peter Merkl's conclusion, which summarizes and synthesizes many of the individual chapters' findings and includes comparisons with countries that are not even covered by the contributions to the book—particularly, East Germany before and after unification.

The book, however, is very uneven. For one, the quality of the individual contributions varies. None of the other essays matches the rigor and the richness of Safran's analysis. Another problem is that part of the book is outdated. The time-boundedness of some of the essays shows most clearly in the case of Vladislav Krasnov's study of the Russian Pamiat. While it is very important to have a look at Russian nationalism, especially in the light of recent developments, it is surprising that the chapter focuses only on Pamiat. There is little anticipation of the Zhirinovsky phenomenon (except in Merkl's perceptive conclusions) and much reliance on the old Kremlinological approach of interpreting Soviet politics via (Pamiat) documents and official statements, rather than through a more structural analysis of the place and power of right-wing elements in Soviet (and post-Soviet) political life. Similarly, Zimmermann and Saalfeld's conclusion on the waning relevance of Schönhuber in the face of the "organizational chaos on the political right" and the "good long-run economic forecast" (p. 73) is refuted by developments since 1990 in Germany. It could be argued, of course, that these developments were unpredictable. However, a different focus (i.e., less on economic factors and more on changes in culture and social structure) would have enhanced the capacity of the study to withstand the test of time. It is also curious that Zimmermann and Saalfeld make so much of the role of conservatives in power. This does not apply to France, the United Kingdom, or the United States

But beyond these and other individual shortcomings, the book suffers a more basic problem of unevenness. The criteria for defining the radical Right across countries, for example, changes from chapter to chapter. While some of the contributions examine parties and electoral trends on the far Right (and Safran goes even further in looking at the much-too-often-neglected links between the radical Right and established parties), others focus on fringe groups, right-wing militant behavior, and violence outside of the "mainstream." Thus, it is questionable whether it makes much sense to focus on the American radical Right in terms of hate groups and Nazi organizations (as Weinberg does), while excluding the New (Christian) Right and the larger context of racism and right-wing orientations in the American public. This fails to do justice to Weinberg's own set of criteria laid out in the introduction. It also contrasts sharply with Merkl's inclusion of the New Christian Right in the concluding summary, as well as with the level of analysis informing other chapters.

Finally, while raising some interesting theoretical and conceptual questions, this book nonetheless neglects much of the basic (as well as the more recent) scholarly work on the subject. Apart from Zimmermann and Saalfeld's effort to discuss some of the concepts of right-wing extremism in the light of the (West) German evidence, there is hardly any attempt (e.g., in the introduction or the conclusion) to provide a more theoretically informed discussion that would integrate the considerable cross-country evidence presented in this book with Theodor W. Adorno's, Seymour M. Lipset's, or Ernst Nolte's work or with any current theoretical literature

The book makes a significant contribution through cross-country surveys of the modern far Right. The editor's claim to provide a book with an "unusually wide scope and comparative thrust" (p. 11), however, is only half met. A genuinely *comparative* study of the contemporary radical Right has yet to be written.

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MICHAEL MINKENBERG

Public Opinion and Regime Change: The New Politics of Post-Soviet Societies. Edited by Arthur H. Miller, William M. Reisinger, and Vicki L. Hesli. Boulder: Westview, 1993. 310p. \$44.95.

Russia is a work in progress, and so, in many ways, is the subject of this book. The three surveys that form the central core of most of the chapters were fielded in various parts of the Soviet Union in the spring of 1990 and, for a section on Estonia, the winter of 1991. The analyses may thus be seen as a useful first pass at the unfolding story of Soviet and post-Soviet public opinion.

The authors address their studies to readers who are not specialists in the politics and economics of the region, and they organize the sections thematically, treating the organization and methods of public opinion research in the Soviet Union, the individual and politics (political tolerance, participation, political culture, and integration), and attitudes relating to economic questions and foreign policy issues. The surveys and about a third of the chapters are welcome results of the collaboration between Soviet and American scholars in survey research and analysis. The editors contribute the introduction and conclusions, Michael Sacks a foreword, and Thomas Remington, an afterword.

While generally successful, the editors' thematic organization for nonspecialists poses a difficult task, since each contribution tends to repeat both contextual information necessary to understand the Soviet issues of the time and relevant dimensions of the surveys that most of the authors draw on and that are described more fully in the appendix. Though sometimes too thin, these introductions can be masterful examples of comprehensive concision, as is the case with the chapter on centerperiphery relations by Vicki Hesli and Joel Barkan.

Jeffrey Hahn's "Public Opinion Research in the Soviet Union: Problems and Possibilities," provides a checklist for Westerners new to the enterprise to evaluate Soviet survey research and is still to the point, even as new survey organizations mushroom and new sampling methods are designed (though often undercut by rapidly deteriorating conditions of public safety and transportation). Mapping public opinion on various issues is also an important contribution. Cynthia Kaplan, in "New Forms of Political Participation," gives a nuanced overview of traditional and unconventional forms of political participation.

Although the surveys reported here do not yet link attitudes to voting behavior (with a very limited exception), they do illuminate sources of support for reform, both political and economic. James Gibson and Raymond Duch, in "Emerging Democratic Values in Soviet Political Culture," examine tolerance on a number of dimensions, including willingness to permit rights for disliked groups, and are optimistic about what they see as an emergent democratic political culture. A valuable addition to this discussion would have been an analysis of the varying degrees of perceived threat these disliked

groups posed for respondents—a part of the tolerance debate that becomes increasingly important over time and across periods of confrontation and civil strife.

Gibson and Duch find (as do Arthur Miller on regime legitimacy, William Zimmerman on attitudes toward foreign policy, and the SIP émigré survey and earlier Soviet surveys) that the great achievement of the Soviet era-near universal literacy and at least secondary education—served to make the young aware of alternatives and dissatisfactions, rather than socializing them in the norms of the system. Interestingly, three Russian contributors are considerably more pessimistic about the irreversibility and depth of attitude change. Gennady Denisovsky, Polina Kozyreva, and Mikhail Matskovsky find that overall, no more than a third of the respondents displayed the value orientation necessary to manage a difficult transition to the market economy. Reisinger and Nikitin point to party formation as a key component of the transition process and warn, on the basis of more current observations, that the inability of reform leaders to mobilize supporters may lead to electoral rejection of reformist leadership, as several recent cases in the region have demonstrated.

Several of the authors look at a changing pattern of confidence in political institutions. Andrei Melville positively assesses what he finds to be a growing confidence in new institutions. Plumbing regime support during regime change presents difficulties, not least of which is the identification of an institution as "new" or "old." The Supreme Soviet (and its parent, the Congress of People's Deputies) was, by the time of the May-June 1990 survey that Melville reports, a reconstituted, newly elected body, whose contentious and freewheeling debates were broadcast to a spellbound television audience. At the time, respondents in other surveys considered the institution—if not "new"-at least "renewed" and not "old," as the Party, military, and youth organization were. Melville's examination of support for new protoparties also needs to balance a favorable conclusion about such responses with the large number of people who evidently did not answer or, perhaps, recognize these groupings. A systematic analysis of the don't know response, such as that given by Gibson and Duch would have been helpful throughout the book.

Underlying nearly all the contributions is the search for trends. Since the research reported is not longitudinal, evidence of trend is extracted from comparisons with other surveys (Soviet and émigré) or respondents' retrospective evaluations (John Willerton and Lee Sigelman's "Perestroika and the Public: Citizens' Views of the 'Fruits' of Economic Reform"). Several of the authors have, since publication of this book, replicated their earlier surveys and have produced direct analysis of trends.

A discussion of ethnicity is central to several chapters of the book. At this early stage, ethnicity, though it figures importantly, is related primarily to titular nationality alienation from central Soviet power in selected non-Russian republics. But ethnic issues proved to be far more durable and complex. The discussions by Hesli and Bracken and Lois Sayrs and James Lindsay attempt a finer-grained analysis by comparing the attitudes of titular and nontitular ethnic groups within a given region; but, as is often the case, rather few of the latter fell into the sample, making it difficult to generalize from, say, the Russians in West Ukraine in the sample or the unspecified but undoubtedly small numbers of Poles

and Jews. However, readers charting the subsequent course of communal politics will be usefully reminded, by these studies, of the initial sources of ethnically based disaffection.

The public opinion industry in the former Soviet Union has certainly been a growth sector, but the December 1993 Russian parliamentary election had a sobering effect. Between the period of the surveys reported in this book (a year-and-a-half before the Soviet . Union dissolved) and 1993, when Vladimir Zhirinovsky, a candidate of obscure origin and overweening selfpromotion, rode a wave of deep, underestimated discontent to Russian parliament, attitudes, as well as the environment, had changed. As the studies in this book and later surveys make clear, there is little sentiment in Russia for reinstating noncompetitive elections (and it should be recalled that by no means all post-Soviet societies have replaced them); but the fragility and constant fission characterizing political parties make them a critically weak link in the political process.

Duke University

ELLEN MICKIEWICZ

The Culture of Conflict: Interpretations and Interests in Comparative Perspective. By Marc Howard Ross. New Haven: Yale University Press, 1993. 240p. \$30.00.

Conflicts become acute not only because of what is at stake but because of the social and psychological consequences of winning and losing. Anthropological evidence indicates that societies develop characteristic patterns of disputing that determine which groups and individuals fight, what they fight about, how they fight, and how those fights are resolved. Cultural patterns determine the intensity of conflict in any society and the extent to which that conflict is directed against outsiders or members of the society. Cultural orientations toward conflict are developed and transmitted through social relationships. Early childhood experiences build (or fail to build) trust, security, and efficacy toward self, others, and the social environment. Institutions and practices reinforce these psychocultural dispositions. They sanction certain values and behaviors, determine group identity, and define aggression and appropriate responses to it.

Using data from a sample of 90 preindustrial societies, Marc Ross develops and tests hypotheses about the relationship between cultural norms and the intensity of conflict. He finds that internal and external conflict are higher in societies where socialization is relatively low in warmth and affection and harsher and higher in male gender-identity conflict. Conflict is correspondingly lower in societies that express warmth and affection and encourage the development of unconflicted gender identities. In peaceful societies, overt expression of aggression is often discouraged by quickly removing the would-be offender from the situation. In such societies, children have multiple, affectionate caretakers, there is continual surveillance of behavior, gender differentiation and sexual tension are low, and sharing is highly valued. Internal images of others tend to be relatively supportive, trusting, cooperative, and responsive. When these expectations are reinforced by behavior, the pattern of low conflict becomes self-sustaining.

Ross also acknowledges the role of structure and identifies two dimensions that are relevant to conflict

behavior. The first is the extent to which people with common interests in one domain share them in another. When social organization reinforces a single, dominant cleavage, conflicts escalate because there are few common interests to restrain them. When the lines of cleavage are many and cross-cutting, the parties are likely to have closer ties and more common interests and thus more incentive to contain their conflict. The second dimension is the level of socioeconomic development and political complexity. Development increases internal conflict because it brings more resources, military capability, and inequality; but political complexity can limit this conflict through the peacemaking role of the state. The data are less ambiguous with respect to external conflict: more complex societies have higher levels of external conflict.

In preindustrial societies (as in modern states), internal and external conflict are positively correlated. Ross contends that both kinds of conflict have similar psychocultural causes: low warmth and affection, harsher and higher male gender-identity conflict. The structural causes vary. In societies with strong cross-cutting ties, hostility is more likely to be directed against outsiders. In societies where there is a single, dominant cleavage, little differentiation is made between internal and external targets.

Ross argues (and I agree) that the psychocultural approach is germane to understanding contemporary conflicts. Modern states are admittedly very different from the small, traditional, preindustrial societies that largely comprise the data base for the study; but they share important social and psychocultural dynamics. This said, Ross's attempt to the explain the relative tranquillity of Norway and the violence of Northern Ireland in psychocultural terms is unpersuasive. Proponents of purely structural explanations have a ready one at hand: Norway is peaceful because it is ethnically and economically homogenous, and Northern Ireland violent because of acute sectarian and class divisions. Moreover, any argument that explains Northern Irish violence in terms of the culture of the inhabitants. Catholic and Protestant, cannot explain the relative peacefulness of the Republic of Ireland, whose citizens share the same culture. A more convincing demonstration of the psychocultural thesis would have utilized cases that appear anomalous and difficult to account for, for example, a violent, homogenous society or one that is peaceful but ethnically divided or economically stratified.

Ross has nevertheless written a very important book. He has identified cultural traits associated by violence and social structures that determine its focus. His findings indicate that conflict management must be tailored to the nature of the society and that the success of different strategies will depend, in the first instance, on the nature of culture in which they are applied. This is the subject of a companion volume, *The Management of Conflict*. Ross also provides a powerful demonstration of the influence of culture on the definition of interests and the choice of means to advance and defend them. His evidence indicates that interest-based rational choice arguments that fail to build on culturally determined conceptions of interest and rationality will have only limited utility.

University of Pittsburgh

RICHARD NED LEBOW

The Political Logic of Economic Reform in China. By Susan L. Shirk. Berkeley: University of California Press, 1993. 399p. \$48.00 cloth, \$15.00 paper.

Susan Shirk's new book will long remain a classic account of the policies that revitalized China's economy during the 1980s. Drawing on extensive interviews with Chinese officials, primary sources, and a rich variety of social science theory relevant to her analysis, Shirk explains how and why the political institutions established in the era of Mao Zedong did not prevent but, instead, shaped (and perhaps ironically may have facilitated) China's reforms.

Shirk's essential argument is that a political logic, rather than an economic logic, best accounts for the policies adopted to reform the key state industrial sector of China's socialist economy. Unlike Mikhail Gorbachev, who sought support among the general population as a counterweight to the vested interests of Soviet central planning, Deng Xiaoping and his associates built a coalition of support for reform within a portion of the existing system's political elite, mainly provincial party officials. The political consequence of this successful strategy to establish the dominance of the reformist line was to strengthen the power of provincial leaders of the communist party. The economic consequence was that reform policies emerged as "particularistic contracts" providing mutual benefits for local leaders and contending central elites in a mixed socialist-market economy. Pork-barrel politics, Chinese style, resulted in a "secondbest" outcome of extensive (as opposed to efficient, intensive) growth. Uniform, impersonal rules of the game for actors in a fully marketized economy, though perhaps economically optimal, were not politically viable.

The book is presented in three parts. Part 1 contains a broad overview of the argument and research design, as well as a brief background analysis of China's ambitious reform program. Part 2 presents a masterful study of the Chinese communist political institutions on which Shirk focuses to explain the path of economic reform in China's state industrial sector. Five chapters cover authority relations (the principal-agent relationship between Communist party and state bureaucracy), leadership incentives (rooted in reciprocal accountability between leaders and members of the elite selectorate), bargaining arena (organizational setting), enfranchisement (representation of sectoral and regional interests), and decision rules (delegation by consensus). A sixth chapter integrates the theory with an analysis of China's economic reforms. Together, these chapters could easily stand alone as required reading in any comparative politics course. Though some may find specific points with which they take issue (especially the weight assigned to the Central Committee, as opposed to the elders, military, and preeminent leader in the elite 'selectorate"), Shirk's argument is elegant, is compelling on its own terms, and demands to be addressed. Her institutional analysis provides a persuasive explanation of the pace (gradual), sequence (postponing the difficult, redistributive choices), content (dangerously expansionary), and form (particularistic contracts, rather than uniform rules) of the policies that China embraced during the 1980s.

In part 3, Shirk fleshes out her argument by tracing the history of several key economic policies, most notably the fiscal decentralization known as "eating in separate kitchens" (that granted greater economic power to provincial leaders) and the attempts to recapture lost central revenues by substituting uniform tax policy for the method of negotiated contracts to hand over a share of enterprise profits to the state. These case studies demonstrate how the incentives established by China's political institutions facilitated reforms that allowed central leaders to dispense favors to potential supporters among the provincial elite (e.g., "eating in separate kitchens," profit contracting), and usually precluded those that harmed provincial interests or deprived central leaders of control over political pork (e.g., a universally applied tax code). Although this might sound like mighty dry territory, the story remains fascinating because Shirk's prose is as elegant and understandable as her theoretical argument. She deftly highlights the link between these economic policy debates and the palace intrigue among aspiring successors to Deng during the

One of the strengths of Shirk's argument is that it clearly establishes a basis for testing her hypotheses about policy and political incentives against evidence emerging in the period after that covered in her study. Vested provincial interests in half-reform based on particularistic contracting helps explain the limited ability of conservatives and central planners to exploit the political crackdown following June 1989 to roll back the basic reforms. Shirk's analytical framework also illuminates the enthusiastic response by local officials to Deng's 1992 call for the resumption of high-speed growth and provides a perspective for interpreting the renewed attempts beginning in 1993 to deal with the negative economic consequences of the type of reforms feasible in the 1980s. China's recent drive to increase the efficiency of state-owned enterprises, to create effective macroeconomic tools for the central government, and especially to rationalize the collection of tax revenues are part of a package intended to move from a system where particularistic arrangements based on bargaining and political interest prevail to one in which uniform rules are set and economic actors compete. But as Shirk puts it, comprehensive reforms entail painful redistribution and challenges to vested interests. This is possible "only if the government has the financial wherewithal to buy off those who stand to lose by the policy or if the party leadership is united and strongly committed to the policy" (p. 309). Given strained central resources and the ongoing succession struggle for the leadership of post-Deng China, Shirk's analysis underscores the political-institutional obstacles to economically sensible comprehensive reform confronted in the mid-1990s.

In a book covering such a broad range of important issues, it is inevitable that readers will find points with which they disagree. One of Shirk's more controversial assertions is that the tendency of leaders to "play to the provinces," though economically suboptimal, was not politically dangerous. She argues that the central party apparatus' powers of appointment and resource allocation mean "the center has little reason to fear that delegations of fiscal authority to the provinces will lead to a loss of central control or national disintegration" (pp. 149–50). However effective these levers of power may now be, their durability in China's changing socioeconomic context is questionable.

A second concern for some will be that Shirk's selective focus (appropriate in the development of a rigorous theoretical argument) necessarily omits—or gives short shrift to—a number of important influences on the reform process. For example, focusing on the incentives and constraints established by the roles and institutions of the Chinese communist polity, Shirk recognizes that she can refer to but cannot fully incorporate the personal power wielded by Deng Xiaoping (and other leading individuals). Yet there can be little doubt about Deng's distinctive influence in establishing the overall goals and direction of reform and his ability to intervene at critical junctures (e.g., endorsing special economic zones, initiating ambitious price reform in 1988, and most notably reviving aggressive marketization in early 1992).

Readers may at times also disagree with Shirk's emphasis on the connection between political-strategic calculations and the substantive policies advocated by key leaders. Why, for example, did Deng choose to support the initiation of market reforms in the late 1970s? Shirk emphasizes the power-political, rather than economic, rationale for Deng's position, arguing that it was his strategy for wresting power from Mao's designated successor, Hua Guofeng. Shirk is right to remind us that power concerns, as well as policy concerns, must be analyzed. But it is not clear that Deng needed to embrace the unorthodox policies he did in order to defeat Hua. Given Hua's mishandling of the economy, Deng could have eased his rival aside simply by claiming greater competence as a leader and by drawing on his "broader and deeper" base of power, to use Lowell Dittmer's phrase. Deng's decision to initiate market reforms may be better explained not by power politics but by his substantive policy preferences (reflecting lessons he had been drawing about the Chinese experience with socialism since the collapse of the Great Leap Forward), the sort of consideration that Shirk acknowledges but that necessarily remains outside her analytical framework.

Some readers may wish that Shirk had not limited her empirical focus to the reform of state-owned industry, especially as it is clear that by the 1990s, the country's center of economic gravity was shifting to the private, collective, and foreign joint-venture sectors (a point Shirk acknowledges). But the economic importance of state-owned industry during the 1980s, the continuing political power of its representatives in the central leadership, and its ideological meaning for a regime determined to retain the fig leaf of socialism even as it establishes a market economy all suggest the significance of the issues Shirk addresses. Moreover, the elite dynamics she highlights in a bureaucratic-authoritarian regime of the communist type, may, with modification, prove useful for understanding the opportunities and constraints facing China's leaders as they tackle other sectors of the economy.

Susan Shirk's book is essential reading for those claiming to understand the reform era in China, the evolution of communist regimes, and the dynamic interaction of politics and economics. Her work not only casts fresh light on the recent past but also establishes a theoretical perspective that will be at the center of scholarly debate for years to come.

University of Pennsylvania

AVERY GOLDSTEIN

Cages of Reason: The Rise of the Rational State in France, Japan, the United States, and Great Britain. By Bernard S. Silberman. Chicago: University of Chicago Press, 1993. 487p. \$65.00 cloth, \$19.95 paper.

Analyses of public bureaucracy have tended to fall into two categories. Theoretical approaches, such as Max Weber's ideal type and economics-based models dealing with budget maximization, slack, and agentry shirking, often posit a high degree of uniformity in bureaucratic structure and/or behavior. Empirical studies, by contrast, reveal great variety in the organization, behavior, patterns of corruption, and cultures of national bureaucracies. Thus, rather than converging as much theory would predict, public administrative rationalization has resulted in different outcomes. By deftly analyzing national administrative development in France, Japan, the United States, and Great Britain, Silberman seeks to build a theory explaining such variation.

Silberman's thesis is that administrative "rationalization took only two courses. One . . . was characterized by the location of skills, information, and their regulation in the individual per se through the process of professional training . . . The second basic type . . . is charcterized by organizational control and allocation of information and skills as well as by organizational surveillance and control of information usage" (p. x). These two forms of rationalization create different organizational and political dynamics. Professionals are apt to have a public service orientation, engage in self-regulation, and be able to practice their professions in a variety of organizational settings. Organizationally oriented bureaucracies have much tighter career and control structures. Here, individuals must commit early to careers in the public service, lateral entry is unusual, and the opportunity for training is heavily regulated by the bureaucracy itself. Organizationally oriented bureaucracies have stronger departmental identities and cultures, are less permeable, and are more difficult for outsiders to control.

Leaving aside the question of whether these really are the only two courses available, why would a polity opt for one or the other? Silberman argues that "the variation can be correlated to historically different experiences with regard to the problem of [political] leadership succession in the nineteenth century" (p. 413). Where there was high uncertainty regarding succession processes and outcomes, as in France and Japan, organizationally oriented administrative rationalization reduced the risks faced by new leaders seeking to solidify their power, authority, and legitimacy. In low uncertainty circumstances, as in the nineteenth century United States and Great Britain, clear and systematic rules govern leadership succession. The problem for political leaders under low uncertainty is to rationalize administration without losing control over the distribution of public goods. Professionally oriented bureaucracies serve nicely because they reduce corruption while seeming incapable of exercising great independence or autonomy.

Silberman further maintains that within each of these overall patterns of rationalization there is variation depending on the character of the leadership structure. Leadership based on social networks results in bureaucratic domination under conditions of high uncertainty

and bureaucratic consensus under low uncertainty. Leadership based on party structure results in either single (high uncertainty) or general party domination of administrative posts.

Silberman's framework substantially advances theory regarding variation in national administrative rationalization and should be used to inform additional studies. It will be particularly interesting to learn whether other courses of bureaucratic development are available. It is not clear why Silberman's types are either exhaustive or dichotomous, unless his contention is merely definitional—either bureaucracies have tight career structures or not, and, if not, are "rational" only if professionally oriented. Even then, hybrids would seem possible. The extent of professional or organizational orientation need not be constant across all agencies in a

national bureaucracy. Policy arena and administrative function may often be more useful units of analysis. Because Silberman's analysis focuses more on the formal qualities of bureaucracy and law, it does not say much about diversity in administrative practices, organizational cultures, and corruption within and across national bureaucracies. Further, as Silberman argues, leaders face high and low uncertainty regarding succession in different social, economic, and legal contexts—and there are many additional contexts to be explored. All this said, Silberman has produced an invaluable, densely packed work that those with deep knowledge of public administrative development will find extremely rewarding.

American University

DAVID H. ROSENBLOOM

INTERNATIONAL RELATIONS

Revolution and World Order: The Revolutionary State in International Society. By David Armstrong. New York: Oxford University Press, 1993. 328p. \$59.00.

Revolutionary states, by their very nature, deliberately have placed themselves in a posture of confrontation with international society. The mutual relationship between such revolutionary states and the Westphalian international order is the central theme of David Armstrong's new book. It is a compelling account of how states that have fundamentally rejected the premises of the international society of states over time come to adapt themselves to that system. But it is also a rendering of how conceptions of international order mutate (at the margins) to accommodate the powerful ideas which provide the impetus of revolution. This study asks, To what extent do revolutionary states succeed in altering international society? To what extent do they become "socialized"? Finally, to what extent is international society possible in a world where some states deny the very existence of common rules, interests, and institutions?

Armstrong explores these questions by reviewing the ways in which revolutionary thought in America, France, Russia (and, in less detail, Libya, China, Cuba, and Iran) clashed with prevailing premises of the "Westphalian conception of international society." In various ways, he argues, revolutionary ideas have challenged traditional notions of state sovereignty and sovereign equality. Popular sovereignty, class struggle, and religious principles have fueled revolutionary challenges to the view of the state as the source of all legitimate authority. Revolutionary states have often questioned the validity of positivist international law and some have rejected its most central tenet, that of nonintervention in the domestic affairs of other states. The consequences for international relations are potentially stark.

The challenge to state-centric, positivist international order is a theme that runs through the study. Jefferson's belief that the underlying principles that should govern the behavior of states (as well as individuals) were to be derived from natural law demanded a standard of morality in the conduct of international affairs that sat uneasily alongside the power politics of eighteenthcentury Europe. French revolutionary thought proposed that sovereignty is invested in the people, not in the ruler, and advanced a novel principle of international legitimacy that threw into question state consent as the basis of international obligation. Perhaps the most thoroughgoing rejection of the traditional conception was the Bolsheviks' historical materialism that viewed the true motive force of history as the struggle between classes. The transnationalism inherent in the claim that "working men have no country" constituted a comprehensive denial of the state itself—the basic unit of the Westphalian structure. A number of parries against the traditional international legal system have emanated from the Third World in the twentieth century (China, Indonesia, Cuba, Libya, and Iran); and while these have disparate geneses, they have in common the tendency to view world politics as a struggle between good and evil, to reject the notion of any integrated international society, and to deny that they are bound by established rules of international law.

Armstrong weaves a subtle description of the interplay between the philosophy, the politics, and the diplomatic practices of each of these revolutionary states. In the case of the United States, France, and the Soviet Union, he is especially careful to present the texture of the domestic debate and political struggles that underlay the formation of the revolutionary state's posture toward established international society. In the American case, for example, Jeffersonian universalism, which Armstrong describes as "moralistic," even "selfrighteous," was in time counterbalanced by Hamilton's views that in order to participate in international relations on equal terms with the European powers, it would be necessary to adapt American ways to theirs. Hamilton's thinking therefore represented an attempt to bring about the socialization of the United States within the prevailing international society, though Jefferson's universalism is clearly discernible in the various moral crusades in which the United States has taken part in this century.

An interesting subtheme in this work is how and why international socialization itself takes place. Armstrong writes that "the existence of the international society is a reality with which all states, however unique, have to come to terms through a socialization process that is common to all new entrants" (p. 77). In the American case, the need for international legitimacy and respectability was the driving force for socialization; the primary American goal, after all, was to become a state. The revolutionary French found that their claims to a unilateral right to set aside treaty obligations regarding the Schelde River, as well as their assertion of some nebulous "general will" to justify interference in the domestic politics of other states, increasingly made it difficult to conduct foreign relations with the British. Even Lenin, whose Marxist philosophy denied the ultimate legitimacy of the state at all-not to mention an international "society" of states-had to come to grips with the political reality that the "socialist fatherland" was surrounded by imperialist states perceived to be bent upon its destruction. During the interwar years, a schizophrenic Soviet diplomacy reflected the perceived need for allies in the international labor movement, as well as the very real imperative to carry on relations with other states.

The relationship between revolutionary states and international society is hardly unidirectional, however. Each of the cases presented in the book displays a high degree of socialization—strong testimony to the lure of state-centric international law for those who would grasp the reins of state power. But over time, the Westphalian order has had to adapt in some respects to the ideas of revolutionary states. As Armstrong notes, "When other ideas about the role of the state—that it should embody the 'nation', that it should be responsive to the will of the 'people', that it was the standard bearer of 'history', that it was responsible for the welfare of its citizens or of other states' citizens—began to take hold, there were inevitable consequences for the society of states" (p. 40). The French Revolution, for instance,

introduced the idea that peoples or nations might have some standing in international law and, indeed, might have a strong legal claim to having some say in their state affiliation or even a claim to a state of their own. Not only has this idea been a driving force behind decolonization after World War II, it has been cited to justify boundaries as multinational empires crumbled in the 1910s and again in the 1990s. The French and American revolutions spawned the essential republican ideas that underlie international law's relatively recent concern with human rights, most especially those contained in the International Covenant on Civil and Political Rights. The American rejection of traditional European diplomacy and the use of force based on the balance of power can be viewed as a precursor to Wilsonian collective security, in which any unlawful international aggression might be openly debated and, hopefully, either deterred or arrested collectively. Even the Bolshevik Revolution has had its mark on international law: the traditional requirement of pacta sunt servanda (treaties must be served) has since been widely accepted to be qualified by the subclause rebus sic stantibus, which expresses the principle that treaty obligations may be invalidated by a fundamental change of circumstances, such as, the Soviets have argued, socialist revolution.

Nonetheless, one is impressed more by the durability of the Westphalian model than by its malleability. Despite the fact that "the Westphalian conception of international society found itself squeezed between two powerful alternative visions which had their origins in two very different revolutionary experiences" (p. 75), the challenge to the system appears to have been more at the margins than at its core. As Armstrong notes, state-centered international society endures and continues to be accepted by states "because the foundation of their association has been the common defence of sovereignty" (p. 311). This book successfully weaves together the political, legal, and philosophical threads of revolutionary challenges to the enduring fabric of international society and is an important contribution to our understanding of the mutual relationship between the revolutionary impulse and the conservative pillars of international society.

Duke University

BETH A. SIMMONS

Breaking the Ice: Rapprochement between East and West Germany, the United States and China, and Israel and Egypt. By Tony Armstrong. Washington: United States Institute of Peace Press, 1993. 187p. \$13.95 paper.

This slender volume is a welcome addition to the growing literature on the causes of peace. Employing the method of "structured, focused comparison," Armstrong examines three cases of rapprochement between states with a long history of hostile relations. From these he extracts five "principles of icebreaking," which he uses to shed light on the prospects for reconciliation between some present-day adversaries.

Unlike most previous studies, Breaking the Ice attempts to identify both structural factors (circumstances) and process factors (strategies, tactics) conducive to rapprochement. Armstrong's intent is to develop theory that is not only explanatory but policy-relevant. Hence,

he focuses mainly on process (which is more susceptible to manipulation by policymakers), and four of the propositions he derives from his case studies are couched in prescriptive language. Among these findings are that "major issues should be fractionated in negotiations aimed at improving relations" and that negotiations "should be conducted at a high level . . . and should be nonpublic" (p. 134). Armstrong admits that his conclusions "contain no great surprises; indeed, they embody much that is common practice and appears to be common sense" (p. 134). Still, by confirming the validity of assumptions that had not been systematically tested, he has performed a valuable service to theorists and policymakers alike.

Studies of rapprochement, like those of détente and related phenomena, encounter certain conceptual difficulties. Is rapprochement a *process*—a movement along the continuum between war on the one hand and stable peace (security, community) on the other? Is it also a *condition*, a specific type of relationship resulting from such movement? If so, what are its essential characteristics?

Armstrong operationalizes rapprochement as the normalization of relations, that is, mutual diplomatic recognition and the exchange of ambassadors. This definition provides a simple criterion for identifying comparable cases. Unfortunately, normalization is a legal process/ event that says relatively little about the actual quality of a relationship. In particular, its significance with respect to war and peace is unclear. Normalization is no guarantee of peace; most belligerent countries have normal relations prior to the outbreak of conflict. Conversely, the severing of official ties need not imply that war is likely. Consider, for example, the U.S. government's withdrawal of recognition from the Republic of China (Taiwan). In fact, normal diplomatic relationships may range from acutely hostile to quite cordial, while those that are not normal may also vary considerably. To his credit, Armstrong acknowledges that normalized relations do not "constitute the substance of improved relations" (p. 6). But what does? While an answer is provided for each specific case, the book never offers a substantive definition of rapprochement as a general phenomenon, as an analytic category. The result is a work in which (1) the meaning of rapprochement (normalization) in practical and theoretical terms is not clearly articulated and (2) the cases being studied are not entirely comparable. As Armstrong's discussion shows, normalized relations between Egypt and Israel were quite different in substance from those between East and West Germany and between China and the United States.

Although it may correlate only weakly with the substance of a diplomatic relationship, normalization does signify some improvement in relations between states that previously refused to recognize one another. If the cases examined in this volume are any indication, such improvement is typically modest, and "breaking the ice" is often the first step in a process that policymakers hope will continue on into the future. For this reason, scholars interested in the subject will need to concern themselves with the *consequences* of normalization as well as its causes. An important question is whether strategies of negotiation conducive to normalization may ultimately be counterproductive from the standpoint of improved relations. Armstrong concludes, for example, that "ambiguous formulations allowing of differing interpreta-

tions should be used to overcome disputes over matters of central principle" (p. 134). Papering over disagreements may enhance the prospects for normalization, but the approach seems likely to create difficulties down the road. One explanation for the collapse in the late 1970s of Soviet-American détente (not a case of normalization, of course) is that its founding document, "Basic Principles of Relations between the United States of America and the Union of Soviet Socialist Republics," was too vague. Each side adopted its own interpretations, which were then violated by the other side, leading to mutual recrimination and charges of bad faith. While "ambiguous formulations" may be beneficial to the short-term objective of achieving normalization (or some other limited improvement in relations, e.g., détente), they may be unhelpful, even damaging, to the chances of more fundamental rapprochement over the long term.

Because Armstrong is interested primarily in process, it is not surprising that the book is weaker in its analysis of the structural conditions favorable to rapprochement than its treatment of strategies and tactics. Armstrong hypothesizes that "initiatives to improve relations with an adversary will succeed when . . . the costs of maintaining hostile policies are growing or threatening to and when "the prospects of gain through such policies are significantly diminished in comparison to previous expectations" (p. 23). In principle, the costs and gains associated with hostile policies may be internal as well as external. Nonrealist scholars have long argued the importance of domestic sources of international behavior. From a fundamentally realist perspective, Steven David has proposed that classical balanceof-power theory be amended to account for the fact that alignment decisions of Third World states are frequently based on considerations of internal, rather than external, security (Choosing Sides, 1991). Armstrong is aware of the potential significance for rapprochement of domestic conditions; but fearing that to explore both domestic and international circumstances would detract from his inquiry into process, he defines structure in terms of international circumstances alone. As a result, his investigation is incomplete.

Fortunately, the book's defects are more than offset by its virtues. The cases it examines are important and deserve comparative analysis. Armstrong's case studies, based mainly on secondary sources and a few autobiographical works, fulfill their purpose, which is not to provide complete, historical explanations but to test and modify a set of hypotheses developed in the opening chapter. Indeed, this volume is exceptional for the clarity with which the case studies are informed by theory (and vice versa). An empirical investigation into the causes of peace, *Breaking the Ice* breaks new ground. It is worth reading.

Vassar College

STEPHEN R. ROCK

The Political Subject of Violence. Edited by David Campbell and Michael Dillon. New York: St. Martin's, 1993. 185p. \$59.95.

In conventional international relations theory, politics and political philosophy have often been separated. This divide has been reinforced by the legacy of twentiethcentury political realism, which proclaimed separate spheres for politics and ethics. This volume, which includes contributions by political philosophers and critics of conventional international theory, attempts to bridge this gap. Drawing upon Weber, Neitzsche, Heidegger, and many contemporary philosophers influenced by them, the authors center their analysis on the relationship between reason, violence and politics.

All contributors share a pessimistic view of the modern era, which, they claim, "has no ethical discourse capable of responding to its lethal global predicament" (p. 19). Influenced by continental European philosophers deeply troubled by the legacy of two world wars, facism, and the Holocaust, they see a more intimate relationship between modern instrumental reasoning and modern political violence than contemporary strategists writing in a rationalized Clausewitzean tradition.

Over the past three centuries, a Eurocentric world has achieved security within by banishing legitimated violence to the boundaries of the international system, between frontiers of newly emerging states or colonized territories. But in the post-Cold War era, the authors see the potential for violence increasing as traditional boundaries between order and chaos are collapsing in the face of ethnic conflict and technologies of globalized warfare. With the achievement of what he ironically terms "world society," Howard Caygill claims that the distinction between the sovereignty of reason within and violence at the boundary has collapsed. Since our identities are constructed within the constraints of time and space and depend on differentiation from those on the outside, we have no defensible basis for deploying our philosophical categories globally.

The implications of this growing ubiquity of violence and the privileging of identity over difference is made more concrete in Michael Shapiro's essay on the Gulf War as an example of contemporary warfare in which, he claims, the identification of friend and foe no longer depends on political and geographic considerations but on electronic devices that render enemies invisible to soldier and civilian alike. For Shapiro, the implications of this "weapons eye view" are all too obvious. It legitimates large-scale violence by a state whose identity has become intertwined with its arsenal of weapons of mass destruction designed to achieve security vis-a-vis an enemy whose human face has disappeared.

If the unipolar moment, applauded by some recent international relations writings, is not a cause for celebration for these authors, the volume disturbs recent claims of conventional theory in other ways, as well. Drawing on the work of contemporary poststructural French philosophers, Simon Critchley points to the fragility of democracy: when no one holds the place of power, totalitarianism is an ever-present possibility. In an era when posttotalitarian democracies in both East and West Europe are witnessing a revival of xenophobic nationalist thinking and where democracy is constantly being undermined by the violence of ethnic and religious conflict, these authors prompt us to ask whether we can fix our hopes for world order on the claims of an evolution toward "mature anarchy" and the belief that democracies do not fight each other.

This volume also contributes to the contemporary discussion of the meaning of security. Examining the historical evolution of the definition of security in Western political philosophy, James Der Derian offers a Nietzschian interpretation, protection from the unknown, and a desire for certainty as being the most representative of our modern understanding of this

term. Yet, claims Der Derian, in our late modern world, not only has the security of boundaries between the known and the unknown disappeared but our attempts to achieve security are no longer adequately grounded in a stable "reality." His analysis of modern warfare points to a "simulation syndrome" where a false confidence in technological imperatives override human intentionality.

Challenging the boundaries within which our conventional understanding of security has been framed is also the subject of R. B. J. Walker's contribution. In an implicit challenge to conventional theories that are beginning to draw boundaries between zones of peace and zones of turmoil, Walker problematizes the traditional levels of analysis of international theory. Drawing on Weber's analysis of the state, he points to the problematic relationship between the violent identities of modern citizens socialized into an exclusionary mode of thinking and the legitimate monopoly of violence by the state, which are mutually constituted in ways that predict the likelihood of violence on the inside, as well as the outside.

Since all of these authors claim that modern reason has been complicit in the politics of violence that, they believe, now threatens our very existence, they assert that we cannot depend on reason to extricate us from these dilemmas, a conclusion that fundamentally challenges the faith in instrumental rationality that lies at the heart of conventional international theory. Is there any way out of this deeply disturbing condition? In the "postface," David Campbell and Michael Dillon suggest "a cultivation of life for no reason" or for reasons of life itself. The ethical implications of this perspective are located in the ideas of Emmanuel Levinas, who sees human beings not as the atomized individuals of Western liberal thought but as essentially social, with identities constituted through their relationship to others. As a prescription for a less violent world, this view of human nature translates into an ethic of responsibility out of which a socially democratic world society, where difference is celebrated rather than denied, could be constructed.

As radical in its conclusions as it is pessimistic about our present condition, this volume fundamentally challenges the core of conventional international theory at many levels. Yet its deeply held skepticism, grounded in a strong sense of alienation from the contemporary world of international politics, is unlikely to convince those who are not already sympathetic. Its complex philosophical arguments make it more accessible to political philosophers than to conventionally trained scholars of international relations. It is a text that will disturb, but it deserves our attention as we struggle with the growing presence of new types of apparently unavoidable violence.

College of the Holy Cross

J. ANN TICKNER

Relocating Middle Powers: Australia and Canada in a Changing World Order. By Andrew F. Cooper, Richard A. Higgott, and Kim R. Nossal. Vancouver: University of British Columbia Press, 1993. 232p. \$65.00 cloth, \$29.95 paper.

One sense in which middle powers have been "relocated" during the 1980s and 1990s, according to Cooper,

Higgott, and Nossal, pertains to the structural outcome of processes: changes in their position in the world economy relative to a declining U.S., ascending East Asia, an integrating Western Europe, and disintegrating communism in the Soviet Union and Eastern Europe. A second, more theoretically interesting sense in which the authors use the term relocating has more to do with agency: qualitative changes in the kinds of activities and goals that middle powers undertake or, more broadly, in the international role they define for themselves and seek to play. Tacking back and forth effectively between state agency and the mix of constraints and opportunities thrown up by a post-hegemonic, post-Cold War international structure, Cooper, Higgott and Nossal present a persuasive argument that middle powers such as Australia and Canada have been finding and exploiting new opportunities for the exercise of leadership toward the purpose of multilateral cooperation.

A surfeit of leadership from those with the structural basis to supply it—the U.S. and Japan—has created the space for alternative forms of leadership, while at the same time providing middle powers with ample incentives to define such a role and develop the bureaucratic capacity to implement it. Following Oran Young's recent reconceptualization of international leadership (see International Organization, 1989 and 1992), the authors focus on non-structural types of leadership, i.e., types not based on leverage derived from preponderant material capabilities. The principal alternatives are intellectual and entrepeneurial leadership: the former refers to formulation of order-building ideas and/or the provision of improved information, and the latter connotes activities such as brokering, mediation or coalition building (though these and other leadership activities, e.g., agenda setting, may straddle the two categories). The theoretical and practical argument set forth in the first chapter is that international leadership is necessarily becoming pluralized in terms of both the forms of it that may be effective in achieving multilateral cooperation and the number and types of countries providing it.

The authors next describe how in the 1980s first Canada and then Australia responded to changes in their international economic environment with administrative reorganization—amalgamations of their foreign and trade ministries designed to facilitate a more economically oriented foreign policy and to provide the bureaucratic infrastructure for the exercise of intellectual and entrepeneurial leadership in multilateral fora. The heart of the book lies in a series of chapter-length case studies of the role of Australian and Canadian diplomacy in recent cooperative endeavors, including the formation of the Cairns Group of agricultural exporters and the Group's role in the Uruguay Round trade negotiations; the genesis and evolution of the Asia Pacific Economic Cooperation (APEC) forum; and coalition building in relation to the Persian Gulf conflict. These are joined by shorter capsule studies of Australian and/or Canadian initiatives concerning arms control, regional security, human rights and the environment.

Though the impact of the Cairns Group had faded by the end of the Uruguay Round negotiations and the APEC process was to gain its own momentum, these are two paradigmatic cases of middle power exercise of intellectual and entrepeneurial leadership on multilateral economic issues. It is difficult to arrive at an alternative interpretation of Australian efforts to build the Cairns Group coalition among fourteen disparate countries, and for its activist role in steering the Group's attempts to shape the Uruguay Round negotiating agenda, to generate confidence building measures and innovative policy reforms, and to mediate and seek compromise solutions between the rigidly-held negotiating positions of the United States and the European Community. Similarly, Australian initiatives to launch the APEC process demonstrate the scope for middle powers to offer innovative solutions in the absence of structural leadership from either the United States or Japan. Indeed, once launched, APEC has since served to catalyze and harness the constructive energies of both of these major powers.

Security issues offer less latitude for leadership initiatives by middle powers, instead putting them in more of a "followership" role and entailing "coalition joining" rather than coalition building. The authors argue that a traditional middle power emphasis on multilateralism accounts for both Canada and Australia initially joining the American-led coalition to diplomatically and economically isolate, and militarily contain, Iraq. But once the United States transformed the purpose of the coalition to the military expulsion of Iraq from Kuwait, the two countries found themselves in a situation, "where the coalition follower, like the queasy roller coaster rider, has little desire to continue the ride but has no option but to hang on tightly until the end" (p. 139). Not all security issues, however, constrain middle powers in this fashion, as illustrated by Canadian and Australian initiatives relating to arms control and nonproliferation and to regional security cooperation. In these issue areas, as well as in issues like human rights and the global environment, both countries have found ways to generate constructive leadership initiatives.

This well organized and clearly written book succeeds in establishing that a focus on the intellectual and entrepeneurial leadership of middle powers yields a significant gain in explanatory power when used to complement more orthodox approaches to the collective action problems inherent to forging cooperative international institutions. The case studies provide an innovative application of Young's plural approach to international leadership and serve to corroborate the excess empirical content claimed for this reformulated concept. Although put to robust use, the expanded leadership concept, qua concept, is not refined or made more precise in the process. But this is a small nit to pick, and not worth pursuing at further length here. For those seeking an antidote to the great power chauvinism and often parochial character of much American scholarship on questions of leadership, hegemony, cooperation and world order, Relocating Middle Powers provides an informative and provocative alternative.

University of Nebraska, Lincoln

DAVID P. RAPKIN

Economic Vulnerability in International Relations: East-West Trade, Investment, and Finance. By Beverly Crawford. New York: Columbia University Press, 1993. 283p. \$55.00 cloth, \$17.50 paper.

Global economic interdependence renders states vulnerable to external pressures in providing for their national security. After World War II, this concern was most apparent in East-West trade, investment, and financial relations. However, Beverly Crawford argues, the West's Cold War economic vulnerabilities were more apparent than real. Her book asserts that America's failure to assess vulnerability accurately and to recognize available mitigation strategies led to intra-alliance disputes in the West and could imperil policies designed to reduce post–Cold War vulnerabilities.

Ideas of state vulnerability as a limitation to openness are not new. Philosophers from Hume to Hirschman described the potential conflict between a state's pursuit of wealth maximization through free trade and the goal of national security through state control over markets. They cite excess specialization—good for efficiency but potentially harmful to national security—as the latent danger in liberal economic policies. The inability of the state under free-trade conditions to develop or maintain national resources that would contribute to the economic and military power of the state and the possibility of being cut off from essential supplies during an emergency create vulnerability and justify state intervention.

Building on this tradition, Crawford identifies three Cold War vulnerabilities perceived by the West: (1) technology exodus that could contribute to the economic or military potential of the East (particularly the Soviet Union), (2) resource dependence that could leave the West vulnerable to a threatened or actual cut off in the supply of an essential commodity, and (3) debtor leverage that could undermine the Western financial system or force creditors to renegotiate less favorable terms. To cope with these concerns during the Cold War, the United States championed market intervention through export, import, and financial controls.

Crawford maintains that U.S. fears were exaggerated and that U.S. efforts to coerce allied governments to accept America's heightened sense of vulnerability were misplaced. Importantly, she identifies four alternative strategies to state controls that have reduced and will reduce these forms of vulnerabilities: (1) corporate strategies that typically withhold key technologies from importers limit the potential for harmful technology exodus; (2) multilateral coordination (e.g., through COCOM) that achieves normative consensus in meeting shared security goals can effectively reduce national vulnerability; (3) "sacrificing" strategies such as coordination between private and public creditors (including the possibility of debt forgiveness) can reduce vulnerability; and (4) for vulnerabilities associated with postcommunist-era governments, the establishment of stable property rights on the basis of market rational behavior can reduce vulnerabilities associated with the breakdown of order (e.g., weapons proliferation and unsound economic policies that threaten creditors' rights).

The book's empirical chapters examine the vulnerability claims of the "economic nationalists"—essentially those in the Reagan administration who advocated extensive government control over trade, technology transfer, and credits to the East. Their claims included the notion that technology transfer narrowed the technology gap between West and East and contributed to significant advances in Soviet military capabilities, that Western Europe was vulnerable to a cut-off of Soviet natural gas shipments, and that Eastern debtors could threaten Western banks and the Western financial system with default. With the exception of mutual financial vulnerability, Crawford finds the evidence insufficient to justify America's heavy-handed attempts to compel its

allies to seek greater market control through government action.

The empirical chapters lend credence to her claim that the Reagan administration exaggerated Western vulnerability. The evidence in support of her argument is generally convincing, if at times uneven. For example, in considering whether Western technology transfer contributed substantially to economic growth in the Soviet Union, she relies on studies of Soviet technology absorption made during the 1970s and 1980s. Although these studies cast doubt on the claims of the economic nationalists, who (she rightly notes) did little to justify their claims of vulnerability with contrary evidence, they are limited in scope.

The book could also benefit from addressing other issues in greater depth. Why the threat was exaggerated is a critical question left to a brief discussion in the concluding chapter. There the author concedes that the Reagan administration's exaggerated vulnerability claims might be explained by misperception based on ideology or by bureaucratic self-interest. Although the author has placed this question outside the scope of her study, it carries implications for her book's premise, namely, that vulnerability can be assessed more rationally and that accurate assessment and a better understanding of mitigation strategies can lead to better policy. If threat perception is a function of ideology, however, then it may not respond to a more rational framework and better data. Likewise, if policy is determined by bureaucratic self-interest, then outcomes are the result of a political, rather than an analytical, process. Either way, these answers challenge the notion that a better framework for assessing vulnerability would have led to more effective policymaking during the Cold War and can do so in its aftermath. Similarly, the book does not examine how the Reagan administration sustained its vulnerability claims in the absence of empirical support and in the face of opposition from its allies and American exporters. The answers to that question also may turn on ideology and institutional strength and question the author's emphasis on rational decision making.

These concerns aside, Crawford's conceptual framework and its application go a long way toward clarifying thinking both as to the sources of state vulnerability and the available mechanisms for ameliorating such dangers. Her insights improve our understanding of alliance dynamics during the Cold War and could conceivably contribute to more effective policymaking in addressing post–Cold War vulnerabilities stemming from trade, technology transfer, and financial flows.

Georgia Institute of Technology

WILLIAM J. LONG

Sino-Soviet Normalization and Its International Implications, 1945–1990. By Lowell Dittmer. Seattle: University of Washington Press, 1992. 382p. \$35.00.

This book is basically a new and systematic interpretation of Sino-Soviet relations from 1945 to 1990, utilizing an approach the author calls "prismatic." Dittmer has taken a prismatic approach in the sense that he uses a series of analytical frameworks in his analysis of different aspects of the Sino-Soviet relationship at the different stages of development, and, in turn, makes a

major contribution to our understanding of Sino-Soviet conflict and cooperation.

Drawing on a wealth of primary and secondary sources in several languages Lowell Dittmer provides a fresh analysis of fundamental changes in the Sino-Soviet relationship and brings the reader up to date on the most recent developments, including the Gorbachev-Deng summit meeting in Beijing in May 1989 and its implications for the great power triangle between China, Russia and the United States. The demise of the Soviet Union and the creation of the fifteen independent republics of the Russian Federation that has succeeded the former Soviet Union has not fundamentally altered the author's argument in the strategic triangle model but its implication is perhaps more significant than the author has assumed at the time of concluding this book in 1990.

Instead of taking a narrative and historical interpretation of Sino-Soviet relations, the author has taken a number of analytical approaches, such as convergence theory, linkage theory, and development/modernization theory in his explanation of cooperation and conflict, frequent transitions from one stage of development to another, patterns of economic development, the search for national identity, and the struggle for international power and security. Dittmer has operationalized these theories in the context of Sino-Soviet relations and succeeded in establishing the linkage between area studies and political science, comparative politics and international relations, domestic politics and foreign relations, as well as the synthesis of the Eastern and Western modes of thought.

The developmental perspective Dittmer uses in Part I focuses on the general theory of convergence, which assumes that industrialization and modernization imply convergence and that convergence implies rapprochement in Sino-Soviet relations. He argues for the connection between domestic development and bilateral relations between China and the former Soviet Union. Part II looks at China's search for national identity in terms of China's relationship to meaningful international "reference groups." Part III focuses on the security dimensions of Sino-Soviet relations and develops the Strategic Triangle Model, which might be the best and most sophisticated model developed for the study of the Sino-Soviet-American relationship and for international relations generally. Dittmer develops the model on the basis of rich empirical and historical data presented in chapters nine through fifteen. Dittmer's data set for the model, however, could have been more extensive. In the post-Cold War era, the archival materials of the past are now made available to scholars in Beijing, Moscow, and Washington. Dittmer therefore could have utilized some of these documents to test his model further, instead of relying on the secondary sources that were utilized, even though the Gorbachev-Deng summit in 1989 marked the "end" of the triangle.

This book is highly theoretical in its analysis of Sino-Soviet relations and may be used to predict the future development of rapprochement between China and Russia. Dittmer makes a significant contribution to some of the core concerns of comparative communism and political development by integrating them into the theory of international conflict management.

University of Connecticut, Storrs

ILPYONG J. KIM

Political Theory, International Relations, and the Ethics of Intervention. Edited by Ian Forbes and Mark Hoffman. New York: St. Martin's, 1993. 249p. \$69.95.

This volume grapples with an old problem, but does much more than rehearse familiar debates between realists and liberals, or between cosmopolitans and communitarians. It does much more than address the standard questions of whether (or when) intervention in the affairs of other states may be justified: it forces us to consider whether the very concepts with which we often begin to answer these questions—the state, sovereignty, legitimacy, law, prudence, right, obligation-are the appropriate starting points for ethical inquiry. In so doing, it brings to bear a wide variety of competing approaches and perspectives, and it makes great effort to combine theory and history. It therefore breaks all of the rules of academic compartmentalization. Its iconoclasm and eclecticism are quite refreshing, and the result is an extremely thought-provoking book.

Following an introduction and overview by the editors, the book is divided into four parts of three to five essays each. Part I, "Definitions and Dilemmas," begins with a solid literature review by Richard Little, and offers preliminary cuts at the problem of intervention from the perspective of international law (Anthony Carty), the history of Western political thought (Cornelia Navari), and analytic philosophy (Peter Johnson). Part II, "Theoretical Perspectives," offers a critique of Realism's approach to the problem of intervention (Leo McCarthy), a pragmatic defense of the principle of non-intervention (Caroline Thomas), a rights-based justification of intervention grounded in a doctrine of basic human needs (Raymond Plant), and an Aristotelian analysis grounded in an account of virtue (Barrie Paskins). It ends with an essay by R.J. Vincent and Peter Wilson that is generally critical of these approaches, suggesting that the problem of intervention will remain intractable until international relations theory has access to an appropriate conception of international legitimacy. Part III, "Hard Cases," includes essays on Israel's intervention in Lebanon (Ali Sadeghi), Vietnam's intervention in Kampuchea (Michael Leifer), international sanctions against South Africa (John Hoffman), and the plight of the Kurds in northern Iraq (James Mayall). Part IV, "Theoretical Departures," seeks guidance on the ethical problem of intervention in contextualism (N. J. Rengger), critical theory (Mark Hoffman), and Foucault

Many of these essays are gems. Anthony Carty, for example, makes brilliant use of the decision of the International Court of Justice in Nicaragua v. US to demonstrate the weaknesses of international law as a basis for ethical judgment—weaknesses inherent in the paradoxical idea of an authoritative body of law grounded in the actual practices of states, and in the tendency of international lawyers and justices to confuse regimes with the states they govern, begging many of the most important ethical questions. Similarly, Leo McCarthy deftly unveils the paradoxes and inconsistencies of Realism's treatment of the problem of intervention, concluding that the moral egoism upon which Realism is founded means that "[t]he state has available to it no arguments to prohibit intervention against itself, and so must deny the moral significance of its own predication" (p. 87). But while these and many of the other essays in Parts I through III do a superb job of explaining why none of the most common principled positions on the problem of intervention is entirely satisfactory, they do not point us in the direction of a solution. By the time the reader reaches page 179, he or she begins to despair of finding any moral compass at all.

It is this despair that Part IV is intended to address. And the essay with which it opens, N. J. Rengger's "Contextuality, Interdependence and the Ethics of Intervention," is the highlight of the volume. Rengger masterfully surveys the weaknesses of traditional approaches to the problem of intervention, and argues (correctly, in my view) that the international system has evolved in such a way as to render them anachronistic. The international system may still be state-centric, but states are far weaker vis-à-vis non-state actors than ever before, and are pulled from without and within by the simultaneous and contradictory pressures of globalization and fragmentation. The result is a greatly complicated moral landscape no longer susceptible to foundationalist analysis. He concludes by making an appeal for a contextual approach to ethical judgment, and teasingly suggests (also correctly, in my view, although perhaps too quickly to be persuasive to the skeptic) that contextualism is neither relativism nor an open invitation to arbitrary judgment.

With the reader eager to hear more of what Rengger has in mind, the volume abruptly shifts gears with the final two chapters, in which the editors offer their own visions of the answers to the problem of intervention. I found these chapters frustrating for many of the same reasons that I find critical theory and deconstruction frustrating in general. Both are motivated by entirely laudable goals: addressing the unselfconsciousness of many scholars about their key concepts and axioms; correcting the illusion of pure objectivity in the study of human affairs; making the link between theory and practice more obvious; baring the dominance-and thereby the historical contingency—of particular meanings, practices, and relationships; cataloguing their effects; and opening our minds to how the world might be different. But while these are entirely laudable goals, they do not require radical epistemological departures; they do not justify relaxing our rules of inference or the precision of our concepts; and they do not entirely blur the distinction between studying the world and changing it. They are goals that careful scholars can pursue within the rubrics of empirical social science and analytic philosophy, whose comparative advantages in clarity and rigor are fully compatible with creativity and circumspection. Hoffman's pitch for critical theory, and Forbes's pitch for deconstruction, represent calls to abandon ship. But they raise a number of red flags that should make us wary of so doing. Forbe's chapter is built upon the foundation of a concept—power—that is problematic enough when used restrictively by mainstream social science, but that is stretched beyond all usefulness by Foucault; Hoffman's openly embraces a political agenda that is not only self-contradictory but prejudicial to the very ethical questions that it is ostensibly seeking to answer ("A long-term goal would be the promotion of forms of radical democratic political communities in which all inhabitants would have an equal right and opportunity to participate in a way that would not entail any enforced denial of identity" [p. 205]). Both chapters raise serious questions about the radical subjectivity of their interpretations, and what Hilary

Putnam would call the "rational acceptability" of their propositions.

Turning from individual essays to the volume as a whole, the editors should be complimented for the extent to which many of the essays manage to speak to each other directly and engage each other in debate. Clearly they have made a considerable effort to put together a volume that coheres. In this they are largely, if not entirely, successful. Caroline Thomas's prudential defense of the principle of non-intervention, for example, immediately follows Leo McCarthy's brilliant exposé of the logical contradictions inherent in that position, and exhibits all of them. Yet it makes no attempt at rebuttal. If the two had been reversed, at least they might have been seen as point and counterpoint.

More seriously, although a number of essays note that the concept of intervention can refer to a wide variety of acts and relationships (and, in its most liberal interpretation, is coextensive with international politics as a whole), no two essays seem to use the word in precisely the same way. It is therefore often difficult to know exactly what problem the book is seeking to explore. This indeterminacy raises particular difficulties for the authors of the case studies, all of which are interesting and informative, but none of which is truly a "hard case" in the absence of a clear definition of the phenomenon. It is possible, of course, that the editors felt they could not impose a single conception of intervention upon their authors without fixing analytical parameters of the kind that they regard as problematic; for doing so would seem to require taking "states" or "cultures" or "communities" as basic units of analysis. But one can address the philosophical questions the volume raises in the context of a more tightly-focussed discussion of a particular class of acts, and some readers will wish that the editors had worried less about form prejudicing substance and more about the tractability of the book as

Still, the book very effectively challenges the reader to think about the problem of intervention in new ways and to grapple with the profound issues that it raises, and anyone seriously interested in the ethical problems of intervention will want to read it.

University of Toronto

DAVID A. WELCH

Human Rights and Peace: International and National Dimensions. By David P. Forsythe. Lincoln: University of Nebraska Press, 1993. 206p. \$35.00 cloth, \$13.95 paper.

To date, the debate about democracy and peace has had little fall-out in the human rights literature. This is surprising, because human rights and democracy are so closely related. David P. Forsythe's book, the first in a new series on human rights edited by himself, sets out to fill this gap.

Forsythe concentrates primarily on the first generation of human rights, namely, civil and political rights, and secondarily on the second generation, namely, social, economic, and cultural rights. He gives short shrift to any notion of a third generation of rights to peace, development, common heritage, and a healthy environment, discounting UN resolutions on these topics as mere diplomatic rhetoric. The right to peace has never been formalized into a legal principle; more important, it

has no independent and specific meaning beyond already familiar concepts in international law such as jus ad bellum. Proclaiming the right to peace does not tell us whether we should embrace unilateral disarmament, peace by nuclear deterrence, or preemptive disarming of potential aggressors. In making this stand, Forsythe circumvents the easier solution of creating a definitional link between human rights and peace and sets himself the more ambitious task of investigating the empirical link.

While Forsythe strikes a middle ground in relation to human rights—retaining second-generation but not third-generation rights—he adopts a narrow concept of peace as the absence of direct physical violence. He examines the general relationship between peace and human rights at the level of interstate war, then in relation to covert action, and then in relation to domestic war. Nearly half the book is devoted to three case studies of domestic war—in Sri Lanka, Liberia, and Romania

With regard to interstate war, Forsythe joins the broad consensus that established democracies do not fight each other. He finds realism, still dominant in U.S. international relations textbooks, to be deficient in not paying much attention to human rights and an emerging international morality but still worthwhile for dealing with interaction with authoritarian states. Democratic countries are fully capable of acting contrary to international law, committing aggression, and violating human rights in armed conflict, although probably less so than authoritarian states. Even in dealing with newly emerging participatory polities, democracies may resort to force, usually covertly, as the United States has done in Iran (1953), Guatemala (1954), and elsewhere. While the opponents frequently were not fully democratic, what led the United States to intervene was primarily the combination of economic concerns and perceived national security, rather than concern for democracy. National security concerns transcend the Cold War and are based on an informal American ideology of greatness, racism, and hostility to revolution. David Forsythe leaves open the question whether "covert war" against radical quasi-democracies is a specifically American trait or whether other democracies are merely content to let the United States do their dirty work for them.

Reverting to his discussion of overt international war, we should note Forsythe's view that avoidance of war is not fully accounted for by political democracy. He embraces what he calls the "developmentalism" of John Mueller in Retreat from Doomsday (1989), who argues that not only democracies but also developed states in general have long sought to avoid war, because it is costly, as well as morally repulsive. Giving this argument an original twist, Forsythe links Mueller's ideas about war avoidance to the second generation of human rights, thus broadening the human rights-peace nexus. While this is an interesting argument, it suffers from the lack of any systematic empirical evidence of any gain in explanatory power by adding the second generation of rights. Besides, the increasing openness at the end of the Cold War has raised serious questions about the fulfilment of second-generation rights in countries that deny their inhabitants political freedom, as distinct from the window dressing at which many such countries excel. Thus, whether it was developmentalism, deterrence, or other factors that led the Soviet Union to pursue a relatively moderate foreign policy must remain unanswered. Finally, Forsythe is less original than it might seem in shifting the attention from democracy—peace to human rights—peace. Rudolph Rummel, in his early work on the subject, spoke not of democracy but of "libertarianism," emphasizing freedom (including political rights), rather than democracy in the sense of competitive politics and majority rule; only recently did he adjust to the prevail-

ing terminology. Forsythe's views on human rights and domestic war are less precise. Interpreting revolution as a crisis in legitimacy, he outlines six sources of domestic legitimacy (legal, moral, historical, ideological, personal, and functional) and three sources of international legitimacy (legal, moral, and political) and sees the nonfulfilment of human rights as figuring prominently in some but not all legitimacy crises. He suggests that it may be possible in the future to move towards a predictive theory, but for the time being he is satisfied to remain at the level of a "typology of preconditions" and to call for an analysis of case studies in order to discern which sources of legitimacy are invoked. At this level of precision, we cannot expect too much of the case studies. They are quite interesting, but since they have been selected precisely as cases of domestic war, nothing can be said about the conditions under which different legitimacy deficiencies may coexist with more benign outcomes than war.

Forsythe's book is of interest as a pioneer effort toward forging an empirical link between human rights and peace and specifically because of his views on the peace-building effects of the social and economic rights. That case cannot be regarded as proven but will hopefully inspire systematic empirical study in the future.

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Business and Banking: Political Change and Economic Integration in Western Europe. By Paulette Kurzer. Ithaca: Cornell University Press, 1993. 261p. \$39.95.

Paulette Kurzer has written a book that will make a substantial contribution to our understanding of national adaptations in the 1980s to changes in the international economy. The institutions and political arrangements of small democracies in Europe are widely thought "to be adept at coping with the repercussions of international crises and changes" (p. 13). Cooperation across class divisions, social partnerships, are usually considered central to this capacity for adaptation. The partnerships, it is increasingly clearly understood, must serve the interests of both labor and business. From the standpoint of labor, the "political trade-offs" depend on the expectation of workers that current sacrifices yield higher earnings and more jobs later" (p. 9). The business community, work by Jonus Pontusson and Peter Swenson has shown, gains from the predictability of labor relations as long as national wage rates are set by bargains in the internationally exposed export sectors. But, Kurzer observes, these partnerships "have not fared well during recent periods of dramatic international changes" (p. 15). Implicitly, these are the countries most likely to be able to adjust to the new global economy. The capacity of Social Democracy to provide a distinct political strategy for economic troubles is at issue. Moreover, if these countries are unable to maintain their core political bargains that permit adaptation, then other countries with even weaker capacities for adaptation will find it difficult to avoid changes forced

by globalization.

Using the cases of Austria, Belgium, the Netherlands, and Sweden, Kurzer sets out to explain why the crossclass social partnerships have broken down. The argument is that globalization with capital mobility and financial integration breaks down the basis of these compromises. It both augments the power of business and diminishes business interests in the bargains that underpin the partnerships. Kurzer effectively constructs the analytic foundations of the argument building on models of Prezworski and Wallerstein. Notably, she highlights the growth of transnationalism and its effect on social protection and compensation rather than an exhaustion in labor power resources. Governments are driven into a competition for the favor of business. To curry favor they seek to adapt their institutions to the preferences of business and finance. She next contends that the national response varies with the character of the ties of business and finance to the international economy. "Small countries differ in the way in which they are linked to the global economy and international financial markets, and this difference defines the extent to which capital can move from one market into another or from one form of investment into another" (p. 16). She defines a country's external relations by the international and export activities of business and finance. The result of these differences is that business and financial interests will vary, state policy will vary, and central bank policy will vary. The basic bargains are under pressure or have broken down in all four cases. Those most exposed—Belgium and the Netherlandshave seen the deepest break. Those with a more muted international orientation—Austria and Sweden—have "not escaped unharmed," but they have been able to at least dampen or retard the consequences of globalization. Kurzer's argument is effectively presented and the evidence in the form of these four cases is convincingly mobilized. She concludes pessimistically that "because the trend in the advanced industrialized world is toward ever-increasing financial integration and business internationalization, national accords between labor, government, and capital are unlikely to reemerge.'

The reader is forced to think through the nature of changes in the international economy and their influence on domestic politics. Kurzer is persuasive, but is she right? The effective presentation of the argument and evidence obscures a number of very fundamental questions that must be debated before we draw conclusions. Let us consider each of the elements of the argument in turn. First, globalization, like interdependence, is a catchall phrase that does not specify relationships. To Ford, globalization meant sourcing parts from the globe for a car designed for all markets. For Toyota it has meant building up entire production systems within each global region. Consequently the precise nature of developments must be clarified. Consider finance. The numbers demonstrate the vast increase in international financial markets. Yet those global markets are largely wholesale markets, and in fact may consist heavily of trade in derivatives that act as insurance against a more limited body of underlying transactions. In any case the transformation of savings into investment remains for now largely a national matter. Do national financial markets endure despite liberalization, regulation and reregulation, securitization, and internationalization?



Arguably they do. Certainly there has been change, but it remains an open question also whether these national markets have converged or evolved along national trajectories. Consider national technology systems, which are increasingly understood to be national and to be at the heart of economic growth. Many international alliances often look like a trade of technology by companies rooted on different technology trajectories. Indeed, multinational firms may roam the globe but with a very few arguable exceptions, they have national homes and indeed their roaming is more focused on their home region of the globe. IBM's intense effort to reinforce the American electronics supply base by pushing for national policies emphasizes that corporate choices may be more limited than suggested at first glance. Particular to this book, one must observe that the greater economic ties in Europe do not amount to a broad drive to globalization. Rather they are creating a more tightly woven regional economy. Regionalization and globalization are not the same thing. And indeed if this is a story of Europeanization, where economic integration has been driven by political choice, the character of causation is much more complicated than suggested here.

If globalization is an ambiguous concept, the interests of business and the influence of the international economy on those interests are even fuzzier. Second, it is likely that the very terms of market competition, not the extent of "globalization" are forcing the breakdown of national social bargains. For example, business interests have become more heterogeneous. The heterogeneity makes it more difficult to strike a single national wage and work bargain. New mechanisms of bargaining have to be established. The emergence of organized white collar labor in significant sectors sheltered from the competitive international marketplace has also undermined national wage bargaining by breaking the link between productivity and rates. Competitive pressures for increasing the speed of product to market and the introduction of new production systems requires a rethinking of how to balance worker protections. Arguably neither cost of social welfare nor comparative wages, but the influence of the operation of the welfare systems on corporate strategy has driven business to distance itself from particular historical bargains. The Swedish tax reforms for example affected firm-level incentive structure, not total national revenue. Similarly, and ironically, in the United States and elsewhere current demands for worker participation and involvement come from market pressures and from businesses that find that the new production and development systems require higher levels of worker commitment and skill. Again specific to this book, I am convinced that the existing political arrangements in these countries have broken down and that changes in the international setting and economy are contributing to that. I am less persuaded that the particular case is the whole story or even necessarily the core of the story.

In sum, Kurzer's tight presentation makes her case well. At the same time it does not confront the complexity of the changes in the emerging international economy. We do not need to conclude so rapidly that one particular development in the international economy defines a broad and complex evolution or that one specific argument has such dramatically greater power than all others that we can dispense with analytic diversity. More generally, the broad application in political science of the econometrics metaphor of line-

fitting to existing lines of argument has in fact slowed our ability to develop new concepts to account for complex phenomena. As a result we may rush too quickly to conclude that something called globalization is afoot and that it will drive national convergence and shift the relative power of social groups. On balance this is a valuable book and a fine achievement. But like many other works that try to assess the domestic political implications of a changing international economy, it still seems uncomfortably constrained by older and still poorly defined concepts that are ready to be replaced.

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Conceptualizing Global History. Edited by Bruce Mazlish and Ralph Buultjens. Boulder: Westview, 1993. 253p. \$59.95 cloth, \$15.85 paper.

Past as Prelude: History in the Making of a New World Order. Edited by Meredith Woo-Cummings and Michael Loriaux. Boulder: Westview, 1993. 254p. \$55.00 cloth, \$16.95 paper.

These two books are a significant addition to the growing body of literature about globalization and global trends—from the earlier literature about the global economy (perhaps going back to *Wealth of Nations* 1776) to the more recent concerns about the global city (King 1990).

Conceptualizing Global History is breaking new theoretical ground and fine-tuning old concepts. Contrary to the widespread belief that the end of the Cold War and the collapse of communist power in Europe signify the discrediting of globalism, Mazlish is convinced that those developments have removed impediments to the true forces of globalization which were for sometime shackled by the artificial ideological frontiers of both communist and capitalist demarcation. The old divisions between the First World (of advanced capitalism), the Second World (of advanced communist countries) and the Third World (of developing societies) is giving way to a neater global dichotomy between North and South, which is itself in the process of mutation as newly industrializing powers exert their influence on the world economy. Raymond Grew warns that we may need special methodologies to deal with the complexities of modernization" (pp. 229–230).

But here the Mazlish and Buultjens volume warns us against equating global history with world history. One variety of world history is the multicultural school which seeks to restore respectability to all cultures and transcend the persistent bias of Eurocentrism. Examples cited in the Mazlish and Buultjens book include William McNeil and Philip Curtin.

Another kind of world history is "Braudelian," associated with Fernand Braudel and the paradigm of "world systems" of Braudel's distinguished disciple, Immanuel Wallerstein. The Braudelian approach emphasizes material factors, underplays the importance of ideas, and until recently had a more restricted time frame

Wolf Schäfer, in his chapter "Global History: Historiographical Feasibility and Environmental Reality," also accuses world history of trying to be "total history" (p. 51). The agenda is the whole span of time and the whole geography of the human race.

Global history, according to Mazlish and Buultjens,

also has to be distinguished from "universal history"—"the effort to encompass all of history, not just the recent past, in one sweeping account" (pp. 3, 97). Perhaps Marxian historical materialism with its epochs of ancient, Asiatic, feudal, capitalist, socialist, and communist modes of production may be regarded as one school of "universal history."

If global history is neither world history nor universal history, what is it? It is not merely a globalist approach to the study of different local histories. The global historian studies the actual processes of globalization in the real world—from the spread of technology to the internationalization of the ethics of human rights. Manfred Kossok, in his piece "From Universal History to Global History," interprets universal history as involving hegemony (pp. 106–107) while global history recognizes "manifold forms of humanity."

"But who is the poet-laureate of the global village?," Richard J. Barnett and John Cavanagh ask in their own chapter on "A Globalizing Economy" (p. 153). There were bards and court musicians for the rise of the nation-state. Do we now look to corporate managers, environmentalists, business planners, advertisers, television producers, or e-mail enthusiasts as the new celebrants of the global village? The authors of these essays seem to underestimate the extent to which the main agent of globalization may be the nation-state itself—in the process of "digging its own grave." Funding for globalizing technology, tariffs for globalizing trade, and political pressures in pursuit of globalizing human rights (p. 175) all include a disproportionate role by the nation-state itself.

Ralph Buultjens poses tantalizing questions about "Global History and the Third World." He concludes that until recently most parts of the Third World did not study societies other than their own and in any case they had different concepts of time in their cultures (p. 75). Were societies of high social consensus in Africa and Asia inclined to be low in cross-cultural curiosity? Postfeudal Europe placed less of a premium on internal consensus, developed a high curiosity about other societies, and set out to discover, explore and conquer the rest of the world. Ralph Buultjens' analysis invites us to conclude (inspite of himself) that global history, for the time being, has to be Eurocentric.

This book is fascinating not only in terms of what it says itself but also in terms of where it places itself in the wider debates of the epoch. How does global history relate to postmodernism? Bruce Mazlish takes us through the concepts of time, progress, universalism, materialism and David Harvey's phrase "the beginning of the global day" (p. 123). Mazlish also reminds us how the computer and the communications revolution may be fulfilling Jacques Attali's prediction of the rise of "the wealthiest nomads . . . freed to get through the world, permanently attached only to their friendships and their work" (p. 123). Is Salman Rushdie (imprisoned as he is by an Ayatollah's fatwa) such a nomad? Rushdie sees himself merely as a migrant "perhaps the central or defining figure of the twentieth century" (p. 123).

Wang Gungwu's chapter on "Migration and Its Enemies" is suggestive on several fronts (pp. 131–151). But the chapter seems to ignore situations where the enemies of migration are, in the first instance, the migrants themselves. This, of course, includes the trans-Atlantic African slave trade—a large-scale case of involuntary

migration. Wang Gungwu is sensitized to the Jewish Diaspora (p. 143) but not to the African Diaspora.

There are three main gaps in this book: the globalizing impact of the *arts* (herein represented only by music), the relevance of globalizing *sports* and, most surprising of all, the globalizing implications of *war* and preparation for war. John Joyce's essay on "The Globalization of Music" (pp. 205–224) does go some way towards filling the artistic gap. But in this sense the arts include both popular and high art—and range from Shakespeare to soap-operas, from printed media to television and the cinema. They have all had globalizing implications on the whole. They are missing from this book.

In his introduction Bruce Mazlish proclaims "Ours, to put it categorically, is an age of Globalization" (p. 1)—but he then promptly forgets "the century of global wars." Arming for planetary destructiveness has been a unique peculiarity of this century—from the assassination of an Archduke in the Austro-Hungarian Empire in 1914 to the formation of the North Atlantic Treaty Organization (NATO) in 1948. War as a globalizing experience is not part of this book. It is indeed a major gap in an otherwise impressive agenda.

A more modest ambition underlies the book edited by Meredith Woo-Cumings and Michael Loriaux, Past as Prelude: History in the Making of a New World Order. The historical period covered in this volume is more clearly within our own twentieth century, with special reference to what Bruce Cummings' Chapter 2 describes as the "seventy-years' crisis" (1919–1989). A European civil war which broke out in 1914 was in the process of being settled in the 1990s.

This book was evidently completed before the worst news broke about Bosnia, or about the uncertainties concerning Russia, or about the political tensions of most of the former republics of the Soviet Union. While Germany has indeed been re-united, Czechoslovakia, the Soviet Union and Yugoslavia have broken up. This book has the sense of optimism of three years ago that "the league of the North" was not at peace—and that conflict and rivalry has been relegated to the Southern hemisphere (p. 5). Unfortunately, a final European settlement to close the chapter of the First World War seems as elusive as ever.

Has Germany been irreversibly "contained" through the triple mechanisms of domestic democratization, regional economic union, and involvement in the NATO? Peter Katzenstein is satisfied that the end of the Cold War and German reunification will strengthen rather than weaken internal democracy and regional goodneighbor policies (pp. 78-81). Michael Loriaux is more circumspect about Franco-German relations and the long-term stability of Western Europe. He asks, "Is it so inconceivable that a demagogue might arise in the heat of electoral competition in France-or even in Germany-and proclaim with success that the French or Germans were getting nothing out of the European Community but unemployment and runaway immigration" (p. 109)? The "Community" has since been re-named the European Union.

The successful "containment" of Japan is also explored in this volume, but the full implications of the end of the Cold War for Japan are less deeply investigated than they are for Western Europe. Moreover, Meredith Woo-Cummings' concept of "East Asia" is too narrow and too Americocentric to deal with the spectacular economic growth rates in the People's Republic of

China in the years when the USSR was disintegrating. China was also on its way towards becoming the largest steel producer in the world. Basically this book marginalizes China and the Middle East, ignores India and South Asia, and forgets the existence of Africa.

The Third World should be grateful that at least Latin America gets stimulating treatment under the pen of John H. Coatsworth. And do we see Russia's future as a possible Third World economy hang in the balance in Valerie Bunce's comparison of the reforms of Alexander II and those of Mikhail Gorbachev?

While authors like Franz Schurmann bring fresh insights about the Gulf War, and while we do explore afresh in these pages the implications of the end of the Cold War, this book is less about the New World Order and more about the new trilateralism—the United States, Europe (now both East and West), and Japan and her sphere of influence. Is this kind of trilateralism simply a more sophisticated version of Eurocentrism? The world is a larger place than this book implies.

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The Internationalization of Communal Strife. Edited by Manus I. Midlarsky. New York: Routledge, 1992. 306p. \$64.95.

Intercommunal violence despoils many contemporary societies. Newly independent states may be especially vulnerable. Ethnic violence disturbs numerous former colonial territories of Africa, Asia, and the Middle East. Long-established polities are not necessarily immune, however, as witnessed by internal frictions that led to the disintegration of the Soviet Union and Yugoslavia and to ensuing troubles within and among their successor states. Communal strife may pose international, as well as domestic, problems. Its roots are ordinarily local, including competing claims to power or independence based upon asserted differences of ethnicity, nationality, religion, language, or social class. Its consequences, in extremis, may lead to major international war. Domestic conflict may acquire international significance by following any of several paths. Dissidents may gain support from foreign governments or from foreign opposition groups. Embattled regimes may obtain support from foreign governments or private sources. Parties to domestic conflict may carry the fight abroad, even in retaliation for aid given their opponents, in support of foreign groups that aid them, or to divert domestic or international attention from internal strife. States not original parties to a conflict may subsequently intervene in order to aid one side or another or to impose peace upon all. Once one foreign entity intervenes, others may follow in aid or opposition to the first. Generally speaking, the more "internationalized" the conflict, the greater the danger to world peace.

The body of recent literature is dispersed and unduly specialized. Numerous monographs describe individual communal conflicts, past and present. Streams of journal articles argue about the correlation between civil and international conflict and the international proclivities of specific types of regimes. Systematic theory and research about how communal conflict is generally internationalized or contained is in short supply, however. Many of the best-known treatments of the subject were penned many years ago, including Eckstein's Internal

War (1964), Rosenau's International Aspects of Civil Strife (1966), Moore's Law and Civil War in the Modern World (1974), and Suhrke and Nobel's Ethnic Conflict in International Relations (1977). The present volume intends to help remedy recent neglect.

The 13 original essays emanate from a 1991 conference at Rutgers University. Attention is devoted primarily to conflicts since World War II. Several chapters constitute case studies of particular communal conflicts. Recent instances examined in some detail include Argentina (Levy and Vakili), Iraq (Davis), Lebanon (Rasler), Peru (McClintock), South Africa (Ottaway), and Sri Lanka (Johnson). Midlarsky also examines communal strife leading to World War I. Other essays take a larger view. Gurr discusses 88 communal groups involved in protracted or intense conflict since World War II. This is part of a larger survey of 230 communal groups subsequently described in his Minorities at Risk (1993). I. W. Zartman discusses incentives and opportunities for international involvement within communal strife based in part upon his previous Ripe for Resolution (1989), which surveys African conflicts. Vasquez, previewing part of his War Puzzle (1993), examines processes that may lead generally to contagion and diffusion of international violence. Hill and Rothchild explore the relationship between the diffusion of conflict and different types of domestic regimes. Touval expands upon his previously published work regarding foreign mediation. C. R. Mitchell, who has also written before about international mediation, addresses conditions necessary for successful external efforts to promote internal peace.

The collection is partly discordant; it does not pretend to a offer an integrated theory. Each essay adopts distinctive premises and asks different questions. Authors evidently do not agree upon the meanings of internationalization or communal strife. Formal definitions of these key terms are not provided. The volume lacks a conclusion and a summary bibliography.

The compendium, nonetheless, is more valuable than a mere sum of its parts. It comprises a more representative and more complete survey of recent thoughts about international aspects of communal conflict than is elsewhere available in print. It brings together under one cover elements of wisdom that are meaningful to one another but which are otherwise dispersed and possibly forgotten because buried within singular case studies or associated primarily with specific narrowly defined lines of research. Chapters are grouped loosely according to relevant aspects and approaches, including (1) worldwide perspectives, (2) perspectives of particular states, (3) processes of internationalization, (4) instances of conflict that did not become internationalized, and (5) processes of international mediation. An index helps to guide the reader among chapters. References within each chapter highlight various avenues of research.

One may hope that *Internationalization of Communal Strife* will stimulate renewed effort to develop a general theory regarding the international aspects of domestic strife. It is a politically significant and intellectually demanding subject. The volume offers valuable preparation to those able and willing to embark upon such a worthwhile enterprise.

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A Normative Approach to War: Peace, War, and Justice in Hugo Grotius. Edited by Onuma Yasuaki. New York: Oxford University Press, 1993. 421p. \$69.00.

Once the honorific "father of international law," Hugo Grotius remains a figure of considerable interestnot only to legal scholars but to students of international relations and the history of political thought. Grotius published the first edition of his great treatise, De jure belli ac pacis libri tres (hereafter JBP) in 1625, during the last and greatest of Europe's religious wars. His aim was to identify all legal rules (laws are rules of moral action imposing obligation) that might limit both the occasions for, and the effects of, organized violence, whether public or private, just or self-serving, and present those rules in "a comprehensive and systematic manner" (JBP proleg. 1, 1.9.1). If his ambitions transcended the particulars of sectarian conflict, so, too, did the Thirty Years War and the peace that followed. Grotius inevitably came to be identified with the advent of the Westphalian system of international relations and a secular, rationalist worldview.

Was Grotius an early modern thinker? His stature in Western thought for the century-and-a-half after his death would seem to suggest as much. So do frequent contemporary allusions to a "Grotian tradition." Many of those who have examined his work are less sure. The more carefully scholars read *JBP*, the more cautious become their claims on the author's behalf.

A Normative Approach to War is the most careful, sustained reading of JBP in English. Its authors are six Japanese scholars, all members of a research group that began studying Grotius in 1976. A Japanese version of the book appeared in 1987. The English version is updated and abridged; the translation, by Sakamoto Mikio with assistance from James Crawford, is clear and as graceful as the subject permits. The book's authors follow the complete text of JBP as Grotius presented it, for convenience dividing the material into nine chapters bracketed by the editor's introduction and conclusion. Clearly a team effort, the book exhibits an exceptionally coherent perspective.

The team's members see Grotius as less modern than the many scholars writing in English who find Grotius a right theorist (pp. 36, 51): "Unlike Hobbes, Locke and Rousseau, Grotius constructed his theory of law without seeking to dismantle the household and the various intermediate corporate bodies. He adhered to the traditional view of man (as zoon politikon) that had prevailed in Europe since Aristotle, assuming the existence of multi-layered independent powers. His concept of natural law tended to justify existing political institutions rather than criticize them from the point of view of justice" (p. 9). Although Grotius took issue with Aristotle's conception of justice and distanced himself from the Scholastics, he was uncritical of the Aristotelian tradition in general (pp. 20-22, 26). This interpretation finds support in an extensively cited literature from continental Europe and Japan.

In one respect, Grotius seems modern—not early modern but fully modern. Despite some principled equivocation (*JBP* proleg. 16, 57), "considerations of utility and practicality underlie the whole structure of *JBP*" (p. 347, cf. p. 72). As the Japanese team shows, Grotius' pragmatism stems from his desire to limit the horrors of war by whatever means possible—law in every conceivable manifestation, appeals to conscience,

authority and tradition, considerations of prudence. Grotius made his system as comprehensive as he could, indiscriminately adding categories, spinning distinctions, and invoking authorities. By the Cartesian standards of the early modern era, he also made his system a shambles.

Viewing Grotius as practical but not very modern, the Japanese team translates key terms from Grotius' Latin with due caution. They find that Grotius seems to have construed summum imperium "as a matter of relative priority in a hierarchy of bearers of imperium on various levels" (p. 137). Their Grotius is no Bodin. Thus, unlike the standard English translation of JBP, they refrain from using Bodin's term, "sovereignty," except to show that Grotius' resistance to "popular sovereignty" makes him even less an Althusius (p. 138). By translating the term civitas as "state," the team does yield to anachronism, despite a sensible discussion of Grotius' corporate conception of "civil society," a term he used interchangeably with civitas (pp. 125–27).

For many scholars, the proof of Grotius' modernity is his conviction that there would be a law of nature by which to govern human affairs "even if we should concede that which cannot be conceded without the utmost wickedness, that there is no God" (JBP, proleg. 11; cf. Tanaka's article, pp. 26–29, 74–77). If, by this remark, Grotius began the process of secularizing natural law, which Hobbes and Pufendorf soon completed, this was hardly his intent. Rather, it was to support a proposition with an ancient pedigree: human beings have "an impelling desire for society," what "the Stoics called, 'sociableness'" (JBP, proleg. 6). Sociableness necessitates society, and this makes human nature "the mother of the law of nature." From this position (which is radical in its implications), Grotius could conclude that all law, including law that human beings voluntarily create for themselves, "derives its force from the law of nature" (proleg. 16).

According to the Japanese team, volitional law and natural law "often overlap and penetrate each other" (p. 53). Many writers take Grotius at his word and separate natural law from volitional law (divine and human) (JBP, proleg. 1), as if Grotius were eclectically naturalist and positivist. Insofar as all law derives its force from human nature, it hardly seems "a methodological weakness" (p. 43) for Grotius to have used the same inductive method to determine the content of natural law and volitional law. On the evidence, human nature is exceedingly plastic.

Starting with the necessity of self-preservation, an otherwise plastic human nature yields a hierarchy (though hardly a system) of law (pp. 51-55). First is mandatory natural law, then a permissive domain of natural law, itself possessing four levels of flexibility that make it effectively indistinguishable from volitional law (pp. 39-41). The latter includes municipal law, some rules of which are common to many peoples (jus gentium), and law "that in the strict sense concerns the mutual society among nations" (mutua gentium inter se societas) (p. 54). Finally, there is what today we might call "soft law" in the form of precepts advising humanity and moderation. As several team members intimate, Grotius had no independent, "Grotian" conception of "international society." Given human nature, if there is law among nations, then there is a nominal society among them as well. Grotius canvassed not just the law of nations but law in all its variety, for one purpose:

where there is law, there are limited just causes for war and unlimited opportunities for its mitigation.

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The Political Psychology of the Gulf War: Leaders, Publics, and the Process of Conflict. Edited by Stanley A. Renshon. Pittsburgh: University of Pittsburgh Press, 1993. 376p. \$49.95 cloth, \$16.95 paper.

This is an unusually wide-ranging and yet coherent edited book. The distinguished contributors represent a useful diversity of expertise and viewpoints, and they have read and used each other's chapters. The book well meets its two goals of contributing to our comprehension of the causes, unfolding, and consequences of the Gulf War and also to "our theoretical understanding of the psychology of conflict, leadership, decisions and consequences" (p. xviii).

The book is organized in terms of the course of the conflict. Part 1, devoted to the origins of the Gulf War, consists of a chapter by Carol Brown emphasizing Arab myths and mind sets that provided the context for the Gulf War and a brief commentary by Dankwart A. Rustow. Part 2 focuses on leaders and decision making. It includes a chapter on President Bush by Stephen J. Wayne and one about Saddam Hussein by Jerold M. Post, perceptively analyzing their personality characteristics and decision-making methods. Stanley A. Ranshon, in a chapter about both leaders, examines what he regards as the good judgment manifest in Bush's decisions and the poor quality of Hussein's. David G. Winter concludes part 2 with a commentary and his analysis of the motive profiles of Bush and Hussein as revealed in their speeches.

Part 2, on the conflict process, includes Janice Gross Stein's detailed analysis of the interaction between the United States and Iraqi governments, explaining why the U.S. government failed to deter Iraq from invading Kuwait and why it failed to compel Iraq to leave before going to war. She concludes, "My analysis suggests that the invasion of Kuwait was probably unstoppable, short of major concessions by Kuwait, . . . and irreversible, short of the use of force to compel complete withdrawal" (p. 144). Jarol B. Manheim provides an informative chapter on efforts to influence foreign policy by the Pentagon, the Bush administration, the Kuwaitis, and the Iraqis, concluding that the Iraqis were not effective and the Kuwaitis were effective in influencing the U.S. agenda. Robert Jervis concludes this part with a commentary relating psychological generalizations with this case.

Part 4, on mass politics, includes Shibley Telhami's insightful analysis of Arab public opinion and variations among different countries and segments of the populations. John Mueller provides a comprehensive, interpretative review of the considerable survey data on American public opinion, and Asher Arian and Carol Gordon do the same for Israeli public opinion. Herbert C. Kelman offers an extended commentary on the impact of the Gulf War and related events on the publics and governments of the region and of the United States. While their impact in the United States appears to be small, they may have much greater consequences in the Arab countries. In this, as in a few other matters, the contributors do not all agree.

Part 5, on the consequences of the conflict, includes

Marvin Zonis's contribution on the shattering of the organizing myths of Arab society, Alexander L. George's analysis of the war's possible impact on the international system, and Dankwart A. Rustow's commentary. The book concludes with part 6, an epilogue by Stanley A. Renshon discussing some policy implications.

Although written shortly after the events analyzed, the authors engage important interpretative issues and provide useful information and valuable insights. Assessments of the events' implications for the future are necessarily more tentative. A finding, supported by various strands of evidence reported by several authors, suggests one reason impacts are limited: perceptions of the Gulf War are filtered through preexisting understandings and consequently tend to confirm what people already believe. This sometimes contributes to the polarization of opinions, as in Israel. Furthermore, it is easier to discern how trends already under way are reinforced than to analyze emerging trends that are dampened.

I think that in many ways, this book illustrates the value of an edited book—giving voice to people with different perspectives. If anything, the book would benefit from even more diversity, especially since it stresses the importance of the way different people view events. Nearly all the contributors are North Americans; and the United States is generally viewed from the inside, the Arab countries somewhat from the outside. Shibley Telhami's contribution avoids this, but Arab perspectives on American organizing myths and mind sets would also be helpful.

Although the book is about the political psychology of the Gulf War, what political psychology means is not extensively articulated. The book, in actuality, includes discussions (largely by political scientists) of personality, small-group interactions, peoples' attitudes and opinions, and other subjective factors such as images, myths, religious beliefs, and collective identities. It also includes discussions of social groupings such as social movements. What concerns me is that labeling all that as political psychology may obscure other approaches. For example, shared images, norms, and understandings about roles and structures are analyzed by other social scientists, notably anthropologists and sociologists and they provide alternative ways of looking at these matters.

Finally, although neither the editor nor the contributors present their analyses as an alternative or a necessary complement to the conventional "realist" paradigm, the significance of their contributions would be enhanced if that were explicitly discussed. It seems to me that the findings and interpretations they present reveal a more complex reality than that envisaged in the "realist" model. For example, the great variation in conceptions of national interest over time and among different persons reduces the analytic values of the concept. For another example, the multiplicity of significant actors (human beings all) undermines the notion of the world as a system constituted by states acting as unified, rational actors. But I confess that these matters may be more appropriate for another book. The editor and the contributors to the work under review have produced a fine book, and it is best to do one book at a

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Louis Kriesberg

The Domestic Bases of Grand Strategy. Edited by Richard Rosecrance and Arthur A. Stein. Ithaca: Cornell University Press, 1993. 230p. \$36.50 cloth, \$13.95 paper.

Editors Rosecrance and Stein present a focused and disciplined set of empirical critiques of structural realism, and while none of the chapters are blockbusters, they are all good. The book is a useful entry in the literature of theoretical debate, and it is less soporific than the usual exegeses of competing paradigms. Of course, it will not resolve the debate, which has been recycled in various forms for centuries. My own views are eclectic but tilt toward realism.

By demonstrating that domestic politics often affect strategic decisions in ways that do not follow consistently from assessment of international constraints, this collection helps to demonstrate the indeterminacy of structural realist theory. While the authors sometimes refer to realism in general (although some strands of realism accord ample weight to domestic factors), the clear target of the volume is Kenneth Waltz. This is only fair, since Waltz's *Theory of International Politics* (1979) gave it to Rosecrance with both barrels. The authors score a number of hits that should chasten naive hyperrealists who see the distribution of international power as firmly governing state action, although they occasionally overlook aspects of their cases that are consistent with realism.

Michael Doyle points out how some rulers act in their own personal interests, rather than those of their countries; and he debunks the notion that in 1914, socialist internationalism collapsed into nationalism. John Mueller makes the case for ideas, rather than capabilities, as the driving forces in the Cold War conflict. David D'Lugo and Ronald Rogowski look at the effect of "constitutional fitness" on the Anglo–German naval race and conclude that British democracy was better able to extract military capabilities from society. (If they had considered conscription, the comparison might have looked different.)

Arthur Stein sees U.S. grand strategy as underextended before World War II and overextended after it, due to domestic disagreements and legislative—executive conflict. Rosecrance and Zara Steiner contrast the British case before World War II with Stein's analysis, arguing that domestic political pressures overrode both economic constraints and realistic strategic choices. (A realist might counter that some of the difference reflects options conferred by geography, so that Americans could debate *whether* to be involved in the European balance, while the British could only debate *how*.)

balance, while the British could only debate how.)

Matthew Evangelista invokes "postrealism" as the idea that countries can choose policies to shape their adversaries' internal debates and argues that reciprocity was the logical strategy for the United States to help Soviet moderates disprove the case of Soviet hardliners. Yet he admits that the idea is hard to test, because there was hardly any U.S. reciprocity; and the one slim reed that bears the entire empirical load for the argument (in opposition to the hawkish contention that revving up the arms race compelled Soviet accommodation) is Reagan's agreement to a summit meeting in 1985.

Jack Snyder surveys the interaction between domestic and international sources of nationalism and argues convincingly that focusing on either in isolation can yield counterproductive policies. In the most pungent of the chapters, Chalmers Johnson presents Japan's behavior both before and after World War II as antithetical to realist logic, and discusses the Meiji–Bismarckian model of development as an alternative to Western liberal or Leninist–Stalinist models. He then rails against the ethnocentric economism of Western social science, which he sees as unconscious ideology blinding Americans to Japan's current grand strategy, which is to build a new Asian coprosperity sphere.

Illustrating the variety of ways in which governments can interpret their strategic options is a reminder to the most simple-minded of realists that there is seldom only one rational option dictated by the international system (or that when there is, it is seldom obvious to all in advance) and that adaptation to external imperatives involves political competition among views of what is rational. Contrary to occasional charges in the book, however, structural realism does not deny this, since Waltz argues that it is a theory not of foreign policy choice but of international political outcomes over time. (It is often forgotten that Waltz himself wrote a whole book on domestic determinants, Foreign Policy and Democratic Politics [1967].) Yet the confusion is understandable, because there has always been ambivalence in realism about whether its tenets are primarily empirical

It is not hard to show realism's limits as a predictor. As Matthew Evangelista notes in his chapter, "Defenders of realism might argue that the theory is intended to be probabilistic rather than determinative. . . . Nevertheless, when the range of possible outcomes extends from appeasement to preventive war, one is justified in asking whether alternative approaches might do better" (p. 167). (It is not necessary to dip below the systemic level to show limitations of the theory: while realists all agree that the distribution of power is the most important concern, there is no consensus among them about which distribution—multipolarity, bipolarity, or unipolarity—is preferable.)

It is possible to appreciate these limitations without reaching as far as Rosecrance and Stein occasionally do. For example, they criticize U.S. strategists for ignoring nonmilitary dimensions of grand strategy and for failing to ask whether nuclear deterrence was affordable: "Both nuclear strategists and realists tended to neglect patterns of domestic support and economic strength that might affect long term commitment" (p. 4). Yet the most ardent proponents of reliance on nuclear deterrence (e.g., Eisenhower) were economizers worried about the unsustainable expense of large conventional forces. Neorealists like Waltz, Posen, and Mearsheimer opposed the defense spending binge of the Reagan era. And many academic strategists at least partly realist in orientation (e.g., Samuel Huntington, Paul Hammond, Warner Schilling, Glenn Snyder, Robert Art) focused much of their work on internal politics. It is also a stretch when the editors say, "There is no realist strategy to deal with domestic social forces in other countries" (p. 16). How about Henry Kissinger's admiring discussion in A World Restored (1957) of how Metternich and his counterparts fostered the consensus on "legitimacy" in the post-Napoleonic era?

At least two simple insights keep realism relevant despite such useful critiques as the volume under review. One, resisted in some respects by Grotians or Wilsonians, is that norms cannot override power when interests conflict. The other is that international politics is marked more by tragic continuities than by progress; or, as Robert Gilpin put it, "Realism must be seen as a philosophical disposition and set of assumptions about the world rather than as in a strict sense a 'scientific' theory, . . . as an attitude regarding the human condition" (in Robert Keohane's edited Neorealism and Its Critics [1986], p. 304). As I suspect at least some of the contributing authors believe, The Domestic Politics of Grand Strategy may best be taken as a corrective complement to realism, rather than a refutation of it.

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RICHARD K. BETTS

China, the United States, and the Soviet Union: Tripolarity and Policymaking in the Cold War. Edited by Robert S. Ross. Armonk, NY: Sharpe, 1993. 224p. \$55.00 cloth, \$18.95 paper.

There is no consensus among students of international relations on the efficacy of the strategic triangle as an analytical tool for studying the interactions among China, the Soviet Union, and the United States. By contrast, the six contributors to this volume adherealbeit to varying degrees—to the view that the strategic triangle is a useful concept. Nonetheless, as Chi Su's insightful chapter notes, the strategic triangle suffers from three major drawbacks as an analytical concept. First, it overestimates the weight and range of China's power. Although the United States tended to treat the People's Republic of China (PRC) as the superpower that some leading American policymakers thought it might become, the disparity between China and the superpowers had a marked effect on the interactions between the PRC and the Soviet Union.

A second problem with the strategic triangle is that while it depicts the general contour of the forest of U.S.-Soviet-Chinese relations, the triangular formation leaves much to be desired when it comes to explaining the trees. For example, in the 1970s, when the Soviet Union was perceived to be on the offensive and U.S.-Soviet relations were adversarial, the concept of triangular relations suggested that China join hands with the United States—and it did in fact move to do so. Similarly, when the Soviet threat began to subside and Beijing began to object strenuously to the U.S. attitude toward Taiwan, the triangular perspective would predict that China would strike a more independent posture. This, too, occurred. But the deficiencies of the triangular paradigm become obvious when one asks more focused questions. Why did the Chinese not attempt to forge a formal alliance with Washington during the 1970s, when Moscow posed such a serious threat, as they had done in the 1950s with Moscow and against a perceived threat from Washington? Also, why did China take the initiative in offering to normalize relations with the Soviet Union in April 1979, when Soviet expansionism was still at its peak? Why did Beijing not lean more toward Moscow while Gorbachev, and then Yeltsin, were defensive and beset with domestic problems and the United States was not only strong but also allegedly attempting to subvert the Chinese government through a "peaceful evolution" plot? These examples show that there are distinct limits to relying on the concept of the strategic triangle to explain developments in Chinese policy toward the Soviet Union.

Third, the strategic triangle tends to overemphasize the strategic dimension of the triangular relationship at the expense of other factors influencing policy. This deficiency appears to be especially conspicuous for Sino-Soviet relations. The Chinese Communist party has a convoluted relationship with the Soviet Union that predates President Richard Nixon's opening to China (and therefore the emergence of the triangle) by five decades. Hence, while Washington may be thinking in strategic terms and Beijing and Moscow may be following policies that accord with strategic rules, the policies of the two may, in reality, be more driven by historical baggage than strategic calculations. Thus, it is necessary to take pretriangle factors into account.

Stephen Sestanovich's chapter on the impact of China on U.S. policy accepts the reality of the strategic triangle but argues that it created very little usable bargaining leverage. However, the triangle did shape the evolving U.S. calculus of the balance of power. The initial opening to China provided a kind of insurance policy that the terms of a U.S.-Soviet détente would be satisfactory from the U.S. point of view. Over time, however, triangular politics created a different dynamic: they helped to build a U.S. consensus in favor of tougher policies toward the Soviet Union. When the Soviet Union attempted to intimidate China, Washington countered against Moscow, thus increasing the Soviet leadership's sense of isolation and overextension in the 1980s. Ultimately, argues Sestanovich, this contributed to the end of the Cold War.

In a further modification of the triangular concept, several authors point out that the personalities of individual leaders had a marked effect on their countries' policies. For example, the shift in American attitudes toward Moscow after 1975 occurred not because the Soviet Union changed but because the U.S. president changed. Similarly, Ronald Reagan's first secretary of state, Alexander Haig, was willing to pay a higher price for U.S.-Chinese cooperation than triangular dynamics suggested; but this was reversed by his successor, George Shultz.

Since the authors are discussing the same events albeit from different angles of a prism—there are inevitable overlaps of material. There are also slight differences of opinion among the authors. Robert Ross argues that since one of the factors that made strategic considerations triangular (China's strategic importance due to superpower rivalry) no longer exists, the rules of triangular politics no longer apply. By contrast, Robert Legvold points out that each of the three states is too important to the others—even when Russia is but a pale shadow of its predecessor—to assume safely that developments in one bilateral relationship will never contaminate the other bilateral relationship: triangularity continues to exist. If the book can be said to have a general theme, it is that the concept of the strategic triangle is a valid tool of analysis, albeit much modified by considerations of history, individual personalities, domestic political considerations, and the behavior of states not party to the triangle.

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JUNE TEUFEL DREYER

Making War and Waging Peace: Foreign Intervention in Africa. Edited by David R. Smock. Washington: U.S. Institute of Peace Press, 1993. 290p. \$19.95 paper.

Funders of policy-relevant research often worry about getting the results to the appropriate people. Left to themselves, academic researchers tend to publish expensive books or scatter obscure articles. The U.S. Institute of Peace, which sponsors a lot of high-quality research, has established its own press to publish and disseminate the results itself.

This book is a collection of such research on intervention in Africa. As such, it is not really a book in the usual sense: it is more like an issue of a good journal (perhaps explaining why there is no index). The quality of the individual pieces is quite high; but they are only tenuously related to one another, they do not present a comprehensive view of the subject, and there is not even the usual figleaf of an editor's conclusion to try to tie them together. Given these limitations, this is a very useful collection that advances our understanding of an important issue, albeit incrementally. Its low price should make it accessible to both policymakers and academics and encourage classroom adoption.

Overall, the book is stronger on cases than theory. Strong chapters on civil wars in Ethiopia, Sudan, Namibia, Angola, and Mozambique are well done and remarkably current, the best I have seen in print. Three broader chapters examine French, British, and Belgian military involvement; Organization for African Unity intervention; and the notion that successful intervention may involve many outside actors (rather than only one). The final chapter is a summary of Chester Crocker's congressional testimony on improving African peacemaking capabilities.

Overall, the book contributes to the literature both by adding new and important cases and by implicitly confirming some widely held ideas. Christopher Mitchell's argument that different interveners can play different roles in the same case is confirmed both by his own analysis of the two Sudanese cases and by the Mozambique chapter (although the apparent assumption of a single kind of settlement process remains controversial).

Several cases support the argument that settlement requires that both sides be strong internally so that they can control their extremists and deliver on their promises. Crocker argues that settlements are the result of "the right alignment—the proper constellation—of local, regional, and international events." Since this cannot readily be predicted, outside mediators should keep trying all the time. (The counterargument that some mediation acts may actually make things worse is not analyzed, although Carter's 1989 failure in the Sudanese case is cited as an example [pp. 126–27].) Finally, the authors are unanimous that foreign intervention does not make war or peace but, instead, strengthens or weakens particular locals and provides new alternatives and options for them.

The book accurately reflects current professional research in this field. Its authors focus on settlement, on encouraging local elites to decide to stop fighting, and on how foreign intervention affects that goal. There is no analysis of the problems of followers (either fighters or noncombatants) and how these may influence collective behavior, including obedience to elites. After civil war, most countries are left with massive problems, such as demobilization, development, and nation building. The role of foreign intervention in such activities is mentioned but ignored-although, presumably, a government's response to these problems will influence whether the settlement "sticks" or not. (This omission is particularly annoying in the Angolan case, where the collapse was attributed precisely to the failure of demobilization.) It is difficult to develop general ideas about when intervention takes place by focusing exclusively on the targets of such intervention, since cases of nonintervention are left out. Any such analysis would have to focus on the politics of the intervening countries themselves, looking for the dogs that do not bark, the interventions that do not occur.

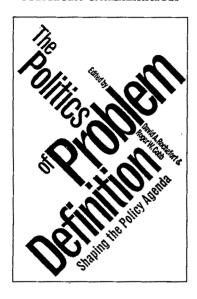
On balance, then, this is a very useful collection, with the virtues (and some of the limitations) of current research on the study of foreign intervention in the Third World.

Rutgers University

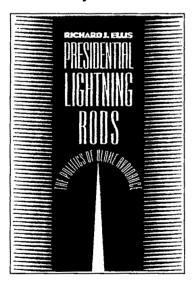
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DIVIDED GOVERNMENT, FISCAL INSTITUTIONS, AND BUDGET DEFICITS: EVIDENCE FROM THE STATES

JAMES E. ALT Harvard University ROBERT C. LOWRY Michigan State University

oes partisan control of American state government have systematic effects on state spending and taxing levels? Does divided control affect the government's ability to make hard decisions? Do institutional rules like legal deficit carryover restrictions matter? Using a formal model of fiscal policy to guide empirical analysis of data covering the American states from 1968 to 1987, we conclude that (1) aggregate state budget totals are driven by different factors under Democrats and Republicans, the net result being that Democrats target spending (and taxes) to higher shares of state-level personal income; (2) divided government is less able to react to revenue shocks that lead to budget deficits, particularly where different parties control each chamber of the legislature; and (3) unified party governments with restricted ability to carry deficits into the next fiscal year (outside the South) have sharper reactions to negative revenue shocks than those without restrictions.

'sing two decades' of evidence from the American states, we argue that divided government has fiscal consequences and that variations in institutions and rules affect fiscal outcomes and timing as well. Deficits at the state level thus have both economic and political antecedents. In a nutshell, big state deficits arise in recessions, but we find they do so where different parties control each legislative chamber or where one party controls all branches of the state government and the state constitution permits deficits to be carried over from one fiscal year to the next. We show how state constitutions and patterns of party control interact with the division of powers between an executive and a bicameral legislature to produce these results, which are consistent with some from other political contexts.

Our study draws on and has implications for three separate literatures. One is the analysis of demand, institutions, laws, federalism, and process on fiscal policy in the states,1 which has recently become prominent as American state governments have experienced significant fiscal difficulties after a long period of relative prosperity. Governors and legislatures have engaged in bitter battles over spending and taxes in attempts to pass balanced budgets. The prolonged fiscal crisis in Massachusetts and more recently the turmoil in California captured headlines, but in fact problems are widespread. For example, 10 states began their 1992 fiscal year without a budget, including New York, which set a record by going over three months past its April 1 deadline before adopting a budget, and Maine and Connecticut, which temporarily shut down all but essential public services. Some observers blame this pattern on excessive spending by state governments in the 1980s, while others argue that the "new federalism" is to blame, by increasing state responsibilities while decreasing federal revenue contributions.

Our casual observation that 9 of the 10 states lacking a budget at the beginning of the 1992 fiscal

year had divided government² brought up another literature that links divided government with budgetary outcomes and in particular deficits and debt. Some question whether governments under divided control can make hard choices and respond forcefully to crises (Cutler 1989; Sundquist 1988) or coordinate to raise taxes (Roubini and Sachs 1989). Others attribute recent U.S. federal deficits to divided party control of Congress (McCubbins 1991) or the lack of presidential party leadership (Inman and Fitts 1990).

Finally, we build on the political-economic "partisan model" of elected officials from competing parties whose core or typical constituents have different preferences which can be addressed through public provision and whose reelection depends at least partly on how well they serve their constituents' wishes. This model has been applied to economic outcomes (e.g., Alesina and Sachs 1990), monetary policy (Alt 1991; Chappell and Keech 1986; Hibbs 1987), American federal expenditures (Kiewiet and McCubbins 1991; Su, Kamlet, and Mowery 1993), and receipts (Cox and McCubbins 1992) and to other countries. Yet attempts to find partisan effects on spending at the state level have had little success even after the imposition of rigorous controls" (Erikson, Wright, and McIver 1989, 730). We use a somewhat different specification from theirs and find limited but explicable party differences in state-level taxing and spending.

American state governments provide a natural laboratory for studying these relationships between partisan control of government, different institutional constraints, and policy outcomes. In each year from 1968 through 1987, at least 18 of the 48 continental states had divided party control of their governments. Outside the southern and border states, unified party control of both the executive and legislative branches existed less than half of the time and splitlegislature governments (where each party controls one house) occurred almost a fifth of the time. The

states generally replicate federal institutions (e.g., all states but Nebraska have bicameral legislatures), but they vary significantly in their institutional details of budgeting and deficits. Every state but Vermont has some formal constraint on fiscal discretion, including requirements for governors to submit (and/or for legislatures to pass) a balanced budget, restrictions on rolling over the deficit across fiscal years (some states having biennial budget cycles), and line-item vetoes for governors. Many states also have restrictions on their ability to issue debt, and some states have recently acquired further restrictions on their ability to increase their sources of revenue (Advisory Commission on Intergovernmental Relations [ACIR] 1989; Council of State Governments 1990). Finally, states have less fiscal autonomy than does the federal government, because of federally mandated programs that are less than fully funded (Chubb 1985).

Our model of fiscal policy in the states takes each of these features into account. Starting from a popular tax-smoothing model of optimal deficits, we explicitly recognize the role of bargaining among elected politicians with different partisan preferences in formulating fiscal policy, particularly in reacting to unforeseen shocks such as a drop in revenue. We model how different partisan configurations will respond to revenue shocks that cause the government's fiscalyear surplus or deficit to deviate from expectations. We then empirically test hypotheses derived from this model by using pooled time-series cross-sectional data to estimate simultaneous equations for the determination of revenues and expenditures and by running simulations of fiscal policy outcomes under different partisan configurations for states with and without limitations on their ability to carry deficits over time.

We find three general results. First, divided party control matters—particularly in the response to exogenous shocks. Governments characterized by divided control of the legislature find it particularly difficult to adjust to exogenous shocks by returning the fiscal balance to its previous path within a relatively short time. Second, institutional constraints on the ability of state governments to manage fiscal policy matter-at least where party control of government is unified. Unified governments in states that are not permitted to carry a deficit over into the next fiscal year react quickly to income shocks so that there is little deviation in the time path of surpluses or deficits. In states without such constraints, governments are less likely to react quickly, and thus more apt to suffer deficits as a result of changes in the economy and fiscal federalism. Third, systematic partisan differences do exist between Democrats and Republicans, though the data indeed do not reveal as simple a picture as Democrats just taxing and spending more (see Garand 1988). Rather, the parties have different goals and react differently to changes in permanent expected levels of income, in ways affected by federal aid and the business cycle. Thus the level of spending at any particular year depends on partisan preferences, past histories of spending and party control, and these other exogenous variables.

A POLITICO-ECONOMIC MODEL OF FISCAL POLICY IN THE STATES

To create politico-economic foundations for our analysis, we extend Barro's (1979, 1986) tax-smoothing model of optimal fiscal policy to incorporate parties and divided constitutional government. We then offer a stylized description of the budget process given different partisan preferences, provide a model of reactions to revenue shocks by different partisan configurations (subject to legal constraints), and derive hypotheses that can be subjected to empirical tests.

From Benevolent Dictators to Divided Party Government

In the model of optimal deficits, policymakers can distinguish permanent from transitory demand for spending, and also know when it's a good time to borrow. Governments set taxes to minimize deadweight loss from now until eternity, given some target level of government spending. In this model of a benevolent despot maximizing the welfare of a representative, ageless consumer, budgets are made in which new borrowing is smooth over time, save only for unexpected shocks, transitory changes, and the relationship between interest rates and expected prices. Optimally, taxes are held constant unless there is a change in permanent expected income, with deficits occurring during times of temporarily high spending, such as wars or recessions.

First we introduce parties into the model. Now policy is set by politicians maximizing some objective function based on their own constituencies' (parties') preferences, rather than the unweighted sum of consumer and producer surplus. In the simplest case there are two sorts of policymakers, each of whom has an economically distinct and different core of supporters whose interests are maximized. One group represents constituents who favor public spending of a higher share of permanent income (high demand) and the other a lower share (low demand).³ Everything else in the economic model still holds, but the permanent share of income that government tends to spend oscillates up and down as the parties alternate in power.

We next take account of institutions that divide control over budgeting between a (bicameral) legislature with proposal power and a governor with veto power. Strictly speaking, if all the branches and houses are controlled by the same party (and there is full information, no factions, and no timing problems), nothing is added to the policy model. But there are other possibilities. One party could control each branch, or one party could control the governorship and only one house. Either information or coordination problems could emerge in these different parti-

san configurations. Particularly in the partisan divided-legislature case, when the two houses have to bargain with each other (possibly under time pressure) to reach a mutually agreeable proposal and have bilateral veto powers, each (at a cost) can stop the other from getting anything. Now a lot of time could be spent off the "optimal" equilibrium path.

Finally, constitutions have effects. Laws exist in some cases that specifically prevent increased borrowing to make up for an unexpected revenue shortfall. Then even a Barro-type government cannot borrow just to offset a transitory fluctuation, let alone a partisan government to shift upward the share of permanent income spent.

A Stylized Budget Process and the Response to Shocks of Partisan Configurations

Given this foundation, we next stylize the process of budget formation in order to derive specific testable predictions about fiscal policy in these varying partisan, institutional, and constitutional contexts. We define the budget as expected levels of spending and revenue during the coming fiscal year as determined by elected officials organized into political parties. These officials attempt to finance their partisans' desired or target level of expenditure, given expected levels of income, net of any purely transitory shocks or unplanned surpluses or deficits. Whether financing involves borrowing or only revenues depends at least partly on constitutional provisions, as will be discussed. The budget process is that the executive submits an initial proposal, but only the legislature can initiate a change in either spending or revenues. The executive can either approve or veto proposed changes, but generally must exercise the veto power within a certain time period after the legislature has acted. If the legislature fails to make a proposal or if a proposal is vetoed and the veto is sustained, then we assume that the budget is set equal to its reversion level, which is the previous year's expected levels plus any persistent effects of the unforeseen shock.⁵

We focus throughout on the "steady-state" behavior of different partisan configurations, holding for future work detailed analyses of initial transitions and any preelection end game. We ask how different configurations of party control affect the way governments behave and particularly how they react to unplanned surpluses or deficits (which we believe usually arise as unforeseen revenue shocks) in setting revenue and spending targets for the next fiscal year. To formulate an empirical model, we initially assume and subsequently attempt to verify empirically

HYPOTHESIS 1. In the states, the Democrats are the high-demand party, and the Republicans the low-demand party, and this difference is reflected empirically in their desired spending levels.⁸

However, in a government consisting of an executive and a bicameral legislature for which two political

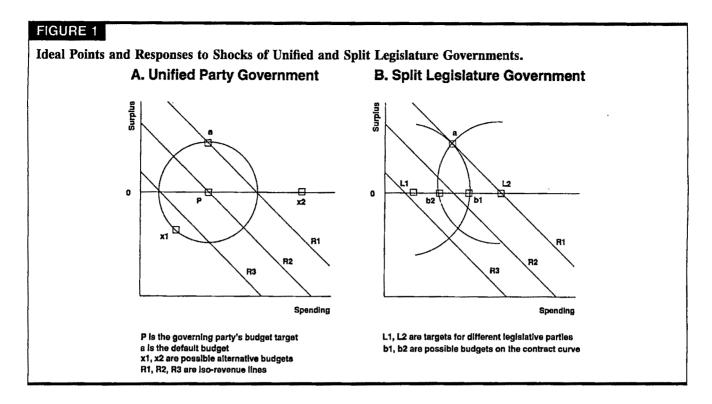
parties compete, there are eight possible combinations of partisan control (Fiorina 1992). These divide into three primary forms: unified party government (one party controls all branches), split-legislature government (one party controls each chamber), and split-branch government (the same party controls both chambers of the legislature, but the other party holds the governorship).

In the model we next lay out, the asymmetric roles of the executive and legislature in making fiscal policy interact with these patterns of control to produce important differences in reactions to unplanned surpluses. Unified governments prefer to react by increasing expected revenues following a deficit and decreasing them following a surplus, whereas splitbranch governments may seek to adjust both spending and revenues to different degrees, depending on the particular circumstances. In contrast, split legislature governments may not be able to agree on any reaction. In all, we derive seven further hypotheses, dealing with both the composition and the timing of adjustment. We stress that our stylization is sufficient rather than necessary to give rise to these and other empirical patterns we discuss.

Unified Party Government. Figure 1(a) shows a two-dimensional policy space where the horizontal axis is the level of total spending and the vertical axis is the budget surplus/deficit. For simplicity, the diagram is drawn as if parties prefer a balanced budget. Of course, it may be that parties actually prefer a deficit or a slight surplus (in order to create a "rainy day" fund), and they might even disagree over the appropriate amount of the surplus. In our analysis, however, this would not affect the basic dynamics of reactions to unforeseen shocks.

Since the surplus is identically equal to revenue minus spending, there is a unique level of revenue associated with each combination of surplus and spending. The parallel diagonal lines labeled R1, R2, and R3 represent three iso-revenue lines, with the difference in revenue levels being equal to the vertical distance between two lines. In other words, for a given level of spending, the difference in revenue between R1 and R2 is the difference in the surplus caused by each revenue level. The point labeled P is the bliss point for the party in power, assuming revenue R2, with the surrounding circle being an indifference contour.9 This mapping implies that a spending level close to the bliss point but creating a deficit, such as at point x_1 , is preferred to a spending level farther away but with a balanced budget, such as at x_2 . There is no particular requirement that indifference curves be circular, only that they are quasi concave in each quadrant with respect to the bliss point, so that no point can be preferred to another point that is both closer to a balanced budget and closer to the target spending level.

In the steady state, if one party has complete control, it acts as though it will govern forever. Thus with income at its permanent expected level the budget is simply set at point P. Following a revenue



shock, the default budget in the case of inaction such as point a lies on the vertical line passing through P. Because point P is preferred to every other point on this line, there is no trade-off: the government adjusts revenues to restore the budget to point P. If the shock is entirely transitory, then the reversion budget is at point P, so the government optimally increases or reduces debt as far as the law allows:¹⁰

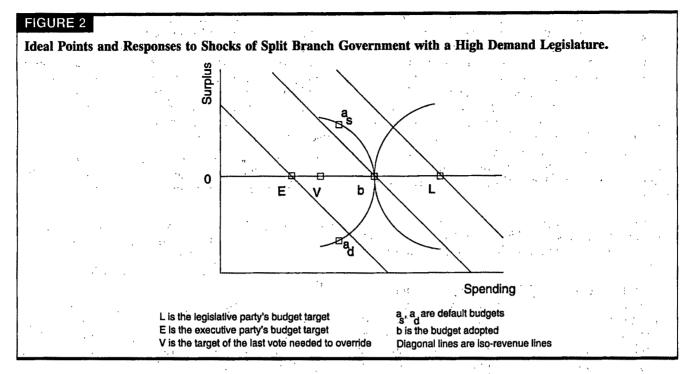
HYPOTHESIS 2. Holding other exogenous variables at permanent expected levels, expected revenues will be adjusted downward in the year following a surplus and upward following a deficit; but so long as the adjustment is to a revenue shock, a unified government will not significantly adjust spending, no matter which party is in control.

Divided Legislatures. If parties differ in their preferred levels of spending, one a high-demand, and the other a low-demand party, then the expected spending level for any given year cannot be optimal for both parties and will probably not be the same as the optimal level that would be chosen by either party in unified government. The key difference between split-legislature government and unified (or indeed split-branch) government is that if each party controls one chamber of the legislature, each possesses a veto over the other and the set of possible agreements contains multiple points determined by the indifference curves passing through the default budget.11 We show this in Figure 1(b), where point L_1 represents the ideal point of the low-demand party, point L₂ represents the ideal point of the high-demand party, point a represents the default budget, and the contract curve is the portion of the balanced budget line connecting points b₁ and b₂.

Because there is no unique equilibrium, each party will be tempted to hold out in order to seek a better deal. In Figure 1(b), for example, party L_1 may be able to achieve point b_2 if it can outlast party L_2 but will have to settle for point b_1 otherwise. This expected bargaining could produce any outcome lying between b_1 and b_2 :

HYPOTHESIS 3. Split legislatures adjust to revenue shocks with a mix of tax and (even possibly preponderantly) spending changes, unlike unified governments.

Split legislatures can respond to shocks, but this is nevertheless the likeliest situation for significant delay to occur. In such situations, as in the model of Alesina and Drazen (1991), both parties choose an optimal time to concede based on their own cost of delay, the advantage gained by waiting for the other party to concede first, and their expectation that the other party will do so. Such delay imposes costs on both parties' constituents, in the form of distortionary deficit financing or undistributed surpluses. Balanced against this is the possibility that the party that holds out longest may be able to secure a better result. The politicians risk voter anger for failure to act, but the political costs of delay will be lower in the case of divided government, since voters are uncertain which party to blame. Given this trade-off of costs and benefits, both parties will likely choose a strategy calling for some positive delay, and the result may be a "war of attrition," with no agreement during the current fiscal year. Some uncertainty regarding the other party's payoffs is crucial: if each party has perfect information, then each knows who will have to concede first, so the eventual losing party might as well avoid the cost of delay. In our case, especially



when legislatures are newly split, each party is likely to be uncertain of the other's willingness to risk the political consequences of failure to make an adjustment:¹²

HYPOTHESIS 4. Delayed responses, or even the absence of systematic responses to shocks (producing larger cumulative imbalances as reversion levels persist) are likelier under split legislatures than unified government.

Split-Branch Government. In this form of divided government, one party controls the executive, and the other controls both chambers of the legislature. Figure 2 illustrates how adjustments to expected spending or revenues depend on (1) whether the high-demand party controls the executive or the legislature, (2) whether the previous year ended in a surplus or a deficit, and (3) the location and preferences of the last legislative vote needed to override an executive veto, which we assume for now is "all-ornothing." Ideal points for the median legislator and the executive are labeled L and E, respectively, while the point labeled V is the ideal point of the last legislator required to override an executive veto. Note that if parties are perfectly homogeneous, V will be the same as either L or E, depending on whether the legislative majority party controls sufficient votes to override a veto. 13 The following discussion based on Ferejohn and Shipan (1990), focuses on the more general case of heterogeneous parties. As before, let point a represent the default budget in the case of no new agreement. Figure 2 shows the case where the "high-demand" party controls the legislature while the "low-demand" party controls the executive. A right-to-left mirror image of Figure 2 would illustrate the case where party control is reversed. Testable predictions about budget adjustment follow in each

The legislature's strategic advantage from being the only branch that can make proposals (plus our assumptions regarding preferences and the government's ability to implement the equilibrium solution) means that on the whole, the legislature can take advantage of a revenue shock to shift spending in the direction of its bliss point. For convenience, let us focus first on the case of an unexpected surplus, designated as. In general, a new budget will be set at the point of tangency between L's indifference curves and the indifference curve for V that passes through point as. This point is labeled b in Figure 2. In other words, the legislative party will be able to push through a budget that maximizes the utility of the median legislator, contingent on leaving the last legislator needed to override a veto indifferent between the new budget and the default budget that would prevail if a veto were sustained. In equilibrium, the legislature will propose this budget immediately, and the executive will accept because a veto will not be sustained. 14 By comparing the location of points a, and b, we see that the expected reaction to a positive unplanned surplus is an increase in spending but that expected revenue does not change. By shifting the location of point V and the shape of the indifference curves, expected revenues could decrease—or even increase—in response to a surplus. So long as indifference curves are quasi concave with respect to the bliss point, however, it is impossible for expected spending to decrease. Moreover, on the whole the direction of the effect of a revenue shock on spending and revenue targets does not depend on whether V is located between E and L (Alt and Lowry 1993).

In the other case in Figure 2, a high-demand legislature responds to the opportunity created by an unplanned deficit (point a_d) by increasing expected

spending and then increasing expected revenues by enough more to balance the budget. We can use the same reasoning to the mirror-image cases to show that a low-demand legislature reacts to an unplanned surplus by decreasing both spending and revenues, while a low-demand legislature reacts to a deficit by decreasing spending. Our fifth hypothesis specifies these four subcases, which predict—for any given reversion level, conditional on the direction of the shock—whether spending increases or decreases, and in some cases specify the direction of change in revenues as well, all depending on the preferences of the critical veto vote:

HYPOTHESIS 5. The response of split-branch government to an unexpected shock is this specified mix of tax-andspending changes, again unlike the case of unified government.

But unlike the split-legislature case, in general the executive can either approve or veto changes proposed by the legislature but must exercise the veto power within a certain time period (usually limited to a number of days) after the legislature has acted, or the legislative proposal takes effect. This asymmetric power with limited time for delay is the main reason behind

HYPOTHESIS 6. Split branch government (like unified government) does not result in delay as readily as split legislatures, where there is either no explicit time limit or a much longer one within which to make a proposal.

Balanced Budget and Deficit Carryover Laws. Our model does not directly address incentives to borrow nor how these might vary across the public, parties, or partisan governing configurations. This would be beyond our scope here. However, the states also have a variety of balanced-budget laws that might influence fiscal policy, though the existing literature is sketchy and inconclusive (e.g., Abrams and Dougan 1986; Bunche 1991). These laws are of two fundamentally different kinds, some requiring balance between projected revenues at the beginning of the fiscal year and others prohibiting ending a fiscal year in deficit or carrying over such a deficit into a new year. The first kind work (if they do work) by focusing attention on the credibility of revenue forecasts and spending plans. In the second case, since the end of one fiscal year coincides with the process of creating the budget for the next, the observation of an imbalance does not just bring forecasts into question but is an action-forcing event: indeed, some of the laws explicitly nullify unfunded expenditures.

Where no such law exists, a government can respond to a surprise deficit by borrowing. Indeed, if the shock is transitory, it is optimal to do so. Where such laws do exist, the response must be to raise taxes, cut spending, or disobey the law (e.g., use accounting tricks to hide the shortfall). All of these, we assume, are undesirable from the government's point of view. Therefore we believe that such laws if enforced must discourage borrowing and excess

spending. Even if there is no direct enforcement (e.g., no possibility of legal redress), we believe that if the laws' provisions are sufficiently transparent, they provide a natural focus for interest groups, media, and candidates of other parties seeking to mobilize public opinion against excess borrowing. While borrowing can be hidden in the short run, where deficits cumulate they are more likely to be discovered:

HYPOTHESIS 7. Other things equal, where a deficit carryover prohibition exists, delayed response to a negative revenue shock resulting in large cumulative deficits is less likely.

However, if the accountability of government just described is indeed the enforcement mechanism, then where all the parties participate in the government, there is no single culpable party to vote out of office:

HYPOTHESIS 8. Other things equal, where a deficit carryover prohibition exists, delayed response to a negative revenue shock resulting in large cumulative deficits is less likely under unified than divided—especially splitlegislature—government.

DATA AND ESTIMATION OF REVENUES, EXPENDITURES, AND SURPLUSES

To test these hypotheses, we employ an empirical model of state fiscal policy. The model links revenues and expenditures in the annual budget process with an allowance for the role of a federal system of grants and matching funds, the possibility of adjustments to past deficits and surpluses, and the desires of (and constraints on) different configurations of party control of government. We review our data and model, then present and discuss the results.

Economic and Fiscal Variables

The data for revenues, spending, federal contributions, personal income, and unemployment cover the 48 states other than Alaska and Hawaii from (fiscal) 1968 through 1987. The All variables (except the unemployment rate) are in real 1982–84 per capita dollar figures, calculated using the regional CPI–U price index published by the Bureau of Labor Statistics. Unemployment averaged just over 6%, ranging from under 2% in New Hampshire in the early years to 15–18% in West Virginia during the recession of the early 1980s. Average income per capita for the whole period was just under \$11,000 (but was 10% lower in the South) and grew on average by 1.35% per annum.

On average, state spending as a share of income increased from 9.7% in 1968 to 13.1% in 1976, then decreased to about 12% in 1978, after which it remained roughly constant. Aggregate, positive budget surpluses existed in every year except 1971 and 1975

and increased every year in both real per capita and percentage terms from 1981 through 1987, during which time they averaged just over 1% of per capita income per year. Spending and revenues are higher (by over 1% of personal income) in states with unified Democratic as opposed to Republican government, and aggregate surpluses are higher as well. These differences are not due to period effects, and they are also not statistically significant. Moreover, there is a lot of state-to-state variation. For instance, across the whole period Massachusetts expenditures actually exceeded aggregate revenues (and the overall balance was barely positive in Connecticut and Vermont) while there were large cumulative surpluses (over 14% of total expenditures) in Wyoming, New Mexico, and Nevada. Finally, federal contributions as a percentage of income increased (on average) from 2.2% in 1968 to 3% in 1976-77, then dropped to about 2.5% in the Reagan years. Federal contributions typically were 20-25% of total state spending, though the early period saw extremes of 13% in Delaware and 40% in Wyoming.

Political Variables

Testing our hypotheses requires that we distinguish among states with unified Democratic, unified Republican, split-branch, and split-legislature government. In each year from 1968 through 1987, between 18 and 25 of the 48 continental states had some form of divided party control. On average, outside the 15 southern and border states, unified control of both the executive and legislative branches existed less than 45% of the time, split-branch governments approximately 37%, and split-legislature governments 18%. In the South, there is unified Democratic government over three-fourths of the time, and split-branch (Republican governors) the rest.

Nor is each of these partisan configurations typical of only a handful of states. All 32 nonsouthern states we study experienced unified government: 22 had some unified Democratic government, 24 had some unified Republican government, and 13 had some of both in our data period. Twenty-six of the 32 had a split legislature at one time or another, while 30 experienced split-branch government. Put another way, 11 of these 32 states experienced all four of these types of partisan control, 15 experienced three of the four, and only six experienced as few as 2 types!18 Nor was any partisan configuration particularly ephemeral. The 22 states having unified Democratic government had 6.8 years of it on average, while the corresponding averages are 5.4 years for the states having unified Republican government, 6.1 years for split-branch, and 4.3 years for split legislatures. So each analysis of a partisan configuration is actually based on data from most of the states and for several years in the case of each state.

A vital political variable in our study is whether the state has some sort of end-of-year budget imbalance prohibition. According to the Advisory Commission on Intergovernmental Relations (ACIR), 26 of the

states in our sample are actually prohibited from carrying over a deficit into the next fiscal year (or biennium) (1989, tbl. 73). Though the ACIR does not provide a precise definition, state constitutions we have examined suggest that such laws include provisions nullifying appropriations or expenditures in excess of revenues for that fiscal year (see Alabama Constitution, amend. 26), although many states allow an exception (i.e., borrowing) if the legislature increases taxes by an amount sufficient to cover the additional expenditure within a specified time period (see Georgia Constitution, 7.4.1). To the best of our knowledge, none of the states having such laws changed them in our data period, though we note that other classificatory schemes also exist. Our attempts to measure the enforcement of these laws directly have so far proved unavailing.20

Model, Estimation, and Numerical Analysis

Transitional and transitory influences apart, partisan state governments set expected revenues equal to the share of permanent income (including federal contributions) their supporters favor spending. In practice, they do this by updating last year's revenues (which, under continuing unified government, were expected to be at last year's ideal point) for changes in aggregate personal income and federal revenue contributions and by making any adjustments in light of the previous year's (unexpected) surplus or deficit. Expected expenditures are then set equal to expected available revenue except for any allowances made for transitory influences of the business cycle (represented by the unemployment rate) and a possible further adjustment in response to the previous surplus or deficit. This formulation allows income and federal contributions to affect expenditures indirectly through their effects on revenues. This produces the following structural model:

$$R_t = a_1 + a_2 R_{t-1} + a_3 Y_t + a_4 F_t + a_5 S_{t-1} + v_t$$
 (1)

$$E_t = b_1 + b_2 * R_t + b_3 * U_t + b_4 * S_{t-1} + w_t,$$
 (2)

where R = total state revenues, E = total state expenditures, S = state surplus (R - E), Y = state personal income, F = federal government contributions, U = state unemployment rate, and V = total are normally distributed residuals with mean zero. 21

We estimate the parameters of equations 1 and 2 simultaneously, using a three-stage least squares procedure, treating the state-level income and unemployment and federal contributions as exogenous. The inclusion of lagged revenues in equation 1 appears to eliminate any need for further correction for serial correlation, besides picking up most of the fixed effects associated with different observations in our time-series cross-section data. We have undertaken considerable experimentation with other estimation procedures and specifications and believe the results we report are robust.

However, the theory that we wish to test also involves how different partisan configurations adjust revenues and expenditures in response to the surplus or deficit itself. For example, our second hypothesis was that unified governments adjust revenues but not expenditures in response to a surplus or deficit. In the case of revenues, the hypothesis is simply that $a_5 < 0$ (taxes should be reduced after a surplus). In the revenue case, hypothesis 3 predicts that under split legislatures $a_5 = 0$ (or at least is smaller in magnitude than in the case of unified government).

To find the total effect of the surplus/deficit on expenditures, we need to obtain the reduced-form expenditure function by substituting for revenues in the expenditure equation and rearranging terms (shown in detail in Appendix A):

$$E_t = (b_1 + b_2 a_1) + b_2 a_2^* E_{t-1} + b_2 a_3^* Y_t + b_2 a_4^* F_t + b_3^* U_t + (b_2 (a_2 + a_5) + b_4)^* S_{t-1}.$$
 (3)

Note that the full effects of income and federal grants on expenditures involve two parameters and that the total effect of the lagged surplus on expenditures is actually a combination of four parameters taken from the structural equations. These combinations form the basis of testing several of our hypotheses. For example, our second hypothesis was that unified governments would not adjust expenditures in response to a surprise imbalance. In terms of the estimates, this hypothesis is thus that for unified governments $(b_2(a_2 + a_5) + b_4) = 0$.

Unfortunately, some other hypotheses cannot be formalized so simply and require that we turn to simulation methods or numerical analysis. Evaluating different adjustment speeds or propensities to run cumulative deficits requires examining the behavior of the whole model over time, seeing how the dynamic parameter estimates interact with growth rates and repeated stochastic shocks to produce longer-term time paths for revenues, expenditures, and the surplus/deficit. We subject models reflecting the parameter estimates for different combinations of partisan control and legal restrictions to different starting values of income, in order to produce initial revenue shocks. We then examine the outcomes of many such runs, each differing from the others of its kind only by random elements included to represent the uncertainty of our statistical estimates. Since we fix the other initial conditions and exogenous variables at the appropriate subgroup average levels, we can isolate the effects of a recession, a split legislature, a balanced-budget law, or a combination of these on the average deficit after several years. The precise methodology is laid out in Appendix B.

EMPIRICAL RESULTS

We present the results in three parts. First, we provide the regression results and defend our choice of specification. To avoid bias, we estimate the structure directly. Second, given our model with an endogenous variable (revenue) in the expenditure equation, we combine structural and reduced-form estimates to

test hypotheses about spending and the composition of response to shocks. Third, to get at the longer-term implications of the dynamic aspects of the model, we examine simulation results to investigate the conditions under which recessions produce larger cumulative deficits. We draw out some further implications in the conclusion.

Regression Results

Table 1 shows parameter estimates for the structural equations. It contains estimates for the nine parameters of equations 1 and 2 separated into eight mutually exclusive cases by region, party configuration, and constitution. The first four subsets are nonsouthern unified governments divided by party and by whether deficit carry over is restricted. (Each subset contains at least 10 different states.) Then there are the nonsouthern split-branch and split-legislature cases, as well as the two party configurations present in the South. Estimates of some state-level fixed effects are omitted from the table in the interests of space.²²

The coefficient estimates reveal similarities and differences across cases. In the revenue equations, the estimated coefficient of lagged revenue (a_2) is generally in the region of .85 to .9 and has a small standard error-so revenues are quite sticky, tracking previous year's values closely. The income coefficient (a_3) varies from .008 to .037. Reading across the first four columns, this means that an extra thousand dollars per capita income in a given state-year produces from \$12 to \$16 more revenue in Republican states and from \$22 to \$37 more revenue in nonsouthern Democratic states, consistent with hypothesis 1 identifying the Democrats as the high- and Republicans as the low-demand parties. We include the insignificant coefficients differentiating split-control cases by governor's party in columns 5 and 6 to show that party differences in those cases are not captured by such a simple coding. Direct dependence of revenues on federal contributions is significantly estimated in all cases but one, but the extent of this dependence varies considerably from case to case.

The response of revenues to a surplus confirms (partly) the stabilization implications of the model as outlined in hypotheses 2 and 3; that is, the largest and most significant negative coefficients of the lagged surplus (a_5) in the revenue equation appear in the cases for nonsouthern unified governments (except for Democrats in legally restricted contexts, where the coefficient cannot be distinguished from zero). The estimates are negative and significant but at a lower absolute value for nonsouthern splitbranch governments (and in the South) and insignificant for split legislatures. This is what we predicted and illustrated in Figures 1 and 2: on the whole, unified governments reduce revenues following surpluses and increase them following deficits. Splitbranch governments also adjust but their reaction is, on average, less, and split-legislature governments fail to adjust revenues.

TABLE 1 Simultaneous Estimates	of State Rev	enues and Ex	penditures,	1968-87				
		NONSOUTHERN					SOUTHERN	
EXPLANATORY	UNIFIED R	EPUBLICAN	UNIFIED I	EMOCRAT		SPLIT		
VARIABLE AND	RE-	UNRE-	RE-	UNRE-	SPLIT	LEGIS-	DEMO-	SPLIT
PARAMETER	STRICTED	STRICTED	STRICTED	STRICTED	BRANCH	LATURE	CRATS	BRANCH
Revenue equation								
Constant, a ₁	57.5	40.3	-139.4	277.4	-112.5	-134.8	-12.83	-33.7
	(66.6)	(80.1)	172.7)	(118.2)	(40.4)	(58.9)	(31.3)	(59.4)
Republican governor	_	- Agentin			7.90 (12.2)	5.89 (16.4)		_
Revenue _{t-1} , a ₂	.761	.886	.902	.869	.939	.863	.896	.937
	(.063)	(.042)	(.090)	(.070)	(.028)	(.039)	(.026)	(.036)
Income, a ₃	.016	.012	.022	.037	.013	.022	.009	.008
	(.007)	(.006)	(.021)	(.010)	(.003)	(.006)	(.003)	(.006)
Federal contribution, a ₄	1.016	.196	.220	.523	.329	.417	.242	.246
	(.195)	(.078)	(.161)	(.189)	(.079)	(.094)	(.084)	(.104)
Surplus _{t-1} , a ₅	69 5	419	157	535	128	.067	183	132
	(.074)	(.089)	(.174)	(.119)	(.058)	(.089)	(.059)	(.108)
Expenditure equation								
Constant, b ₁	35.2	63.7	166.7	183.8	56.8	94.8	9.23	-7.76
	(35.5)	(63.8)	(58.6)	(95.1)	(24.1)	(34.2)	(20.9)	(28.2)
Republican governor	_		Signification		7.20 (10.7)	-9.00 (12.6)	_	-
Revenue _t , b ₂	.855	.887	.827	.811	.897	.898	.958	.969
	(.031)	(.043)	(.042)	(.049)	(.017)	(.023)	(.018)	(.027)
Unemployment, b ₃	9.33	15.57	6.53	6.48	7.20	3.90	1.12	5.70
	(3.15)	(5.49)	(3.33)	(4.66)	(2.19)	(2.67)	(1.49)	(2.48)
Surplus _{t-1} , b ₄	.023	529	439	418	607	656	546	773
	(.031)	(.093)	(.100)	(.100)	(.050)	(.072)	(.054)	(.084)
Number of cases	71	59	65	87	236	113	225	75
System R ²	.94	.95	.97	.89	.96	.96	.94	.96

Sources: State Government Finances, published by the Census Bureau, various issues of the Book of the States, and the Statistical Abstract. Coefficients are unstandardized three-stage least squares regression coefficients, calculated by SST (Statistical Software Tools). Standard errors are in parentheses. All monetary variables are measured in thousands of constant 1982–84 dollars per capita. Restricted and unrestricted refer to constitutional provisions permitting a budget deficit to be carried over from one fiscal period to another. Some state-level dummy variable coefficients are not shown to save space (see n. 22 for details).

On the expenditure side of the model, expenditures closely follow projected revenues, as in our stylization of the budget process. All cases but one (southern Democrats) display a significant cyclical response to the economy as represented by the unemployment rate. We discuss the total effect of the lagged surplus on expenditure (and the spending predictions made by hypotheses 2 and 3) in the next section, since the lagged surplus coefficient (b_4) in this equation does not have a simple interpretation (see Appendix A).

On the whole, the results reported in Table 1 are satisfactory. Our choice to disaggregate by region, partisan grouping, and deficit carryover restriction appears to capture most of the significant state-to-state variation in adjustment processes.²³ Not a single coefficient was significantly estimated with a sign contrary to expectations. The equations fit the data well. The patterns of coefficients we have described support the foundations of the model—targeting to

permanent levels of income, response to economic cycles, the stylized budget process, the relevance of deficit carryover laws, and (so far) partisan differences—quite well. The model seems to be free from obvious faults.

Most important, the adjustment parameter estimates (a₅ and b₄) do not pool across partisan configurations, while individual states appear in several different groupings—indeed, sometimes in all possible groupings (outside the South). Thus each "state" as a unit of observation reflects its experience under different patterns of partisan control with different consequences for fiscal policy. Therefore spending averaged across years by states, for instance, will reflect the varied partisan histories of each state. The disaggregation to state—years as observations, separated into partisan configurations, may be necessary to reveal partisan and institutional effects. Moreover, attempting to designate states as Democratic or Republican may well cause researchers to miss partisan

effects, because of the diversity of each state's partisan history.

Reduced-Form Tests: Parties and Composition of Adjustment

Party Differences. Our first hypothesis was that Democrats have a higher—and Republicans a lower— "target" share of state-level income toward which they would push public spending when in office. Since the parties do not typically enjoy unified control of state governments for long periods, we cannot expect observed actual spending differences fully to reflect these underlying differences in goals. We therefore derive partisan differences in spending targets from the reduced-form equation 3 (which, recall, can be calculated from the estimates in Table 1 by the procedure in Appendix A) as shares of permanent income, analogous to Alt and Chrystal's (1983) for national governments in Britain and the United States. These permanent income shares are the steady-state levels of expenditure as a share of income implied by the dynamic coefficients in Table 1—the levels at which there would be no further systematic drift up or down if a given partisan configuration governed forever. Finding these stable permanent income shares of expenditure that the estimates in Table 1 imply to be indeed higher for Democrats than Republicans would offer further support for hypothesis 1. By transforming the coefficient values from equation 3 for unified nonsouthern Democrats and Republicans,²⁴ we obtain the needed target spending shares for unified Democratic and Republican governments (and the difference between them and other partisan configurations) (Table 2).

On average, pooling across the coefficients reported in Table 1, columns 3 and 4, the steady state of the model implies that Democrats appear to want state spending to equal 11.06% of permanent personal income (line a) while (pooling columns 1 and 2 of Table 1) Republicans want to spend 4.36% (line b). To repeat, this is an underlying, idealized difference revealed by our estimates of how the parties would behave if they had sufficient unified control to have brought fiscal policy to their desired levels and believed they would govern indefinitely with aggregate incomes at sustainable levels, rather than a description of the circumstances in which many parties find themselves much of the time. This implied interparty difference in targets of 6.70 percentage points (line c) is marginally statistically significant²⁵ and amounts to a difference of over \$600 per person per year (1982 dollars) in desired spending levels. While this difference may seem too large, much spending is mandated at the state level by other branches and levels of government, limiting the discretion of parties seeking to cut spending. Moreover, while actual spending levels appear higher than these targets, recall that they are also partly financed by federal contributions, so that the actual spending shares financed out of

	IADLE 2				
	Reduced-Form	Hypothesis	Tests	for	"Permanent"
ĺ	Expenditures				

TYPE OF PARTY CONFIGURATION	PERCENT OF INCOME	STANDARD ERROR
Effects of State Income		- WAAAMAANA AAAAAAAAAAAAAAAAAAAAAAAAAAAAA
Nonsouthern		
 a. Unified Democrats 	11.06	(4.0)
b. Unified Republicans	4.36	(1.3)
c. Difference (a - b)	6.70	(4.2)*
d. Split branch	6.65	(2.2)
e. Split legislature	8.78	(3.1)
Southern		• •
 f. Unified Democrats 	6.09	(2.4)
g. Split branch	7.92	(1.0)
Effects of federal contributio	ns ^a	
h. Unified Democrats	4.06	(1.5)
i. Unified Republicans	5.18	(1.3)

Note: The permanent effect of an explanatory variable in equation 3 is equal to its coefficient multiplied by the reciprocal of 1 minus the coefficient for lagged expenditures (see n. 24 and Appendix A). For income, this effect is $b_2g_3/(1-b_2a_2)$. All estimates are based on coefficients reported in Table 1. Averages for unified governments are weighted in proportion to the number of cases in Table 1, cols. 1–4. "Nonsouthern.

p < .05.

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state incomes are probably typically between these two targets.

In any case, the implication is that a unified Democratic (Republican) government taking over where the actual income share of spending was between the two targets would exert upward (downward) pressure on spending until its target level was reached. Thus our basic, first party-differences hypothesis is statistically sustained for target levels even if not for actual levels of spending. The insignificantly higher levels of actual spending as a share of income in nonsouthern Democratic than in Republican states is partly because the two parties do not hold unified control of government long enough to reach their targets.

Table 2 reveals some other interesting results. First, the target spending share of southern Democrats (line f) appears considerably (but not statistically significantly) lower than that of nonsouthern Democrats, consistent with their image as fiscally conservative. Divided cases produce target levels somewhere between those of the parties, but the splitlegislature case (line e) has a target level closer to that of the unified Democrats, as though the high-demand party usually wins on the spending side in divided legislatures. This was not a feature of our model, though it does not contradict any specific assumption we made. Finally, there is no evidence of party differences in "permanent" dependence on federal aid: both unified partisan groups spend out of federal aid in similar ways, and in ways that seem to imply the expectation of a lot of growth in federal aid (the long-run levels are greater than actual averages),

TAB	L	Е	3
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Response to Fiscal Imbalance

TYPE OF (NONSOUTHERN)	RESPON \$1 DE		
PARTY CONFIGURATION	REVENUE	SPENDING	TOTAL
Unified Governments	.46 (.06)	.01 (.08)	.45
Republicans No Carryover allowed Carryover allowed	.69 .42	08 .11	.77 .31
Democrats No Carryover allowed Carryover allowed	.16 .54	18 .14	.34 .40
Split Branch	.13 (.06)	12 (.08)	.25
Split Legislature	07 (.09)	18 (.11)	.11

Note: From equation 3, the revenue response to each dollar of deficit is calculated as \$(-1) times a_5 . The spending response is $\$(-1)^*(b_2(a_2+a_5)+b_4)$. The total response is the revenue response minus the spending response. All estimates are based on coefficients reported in Table 1. Averages for unified governments are weighted in proportion to the number of cases in Table 1, cols. 1–4. Standard errors are in parentheses.

probably because federal aid did in fact grow through much of the period.

Composition of Adjustment. Hypotheses 2, 3, and 5 made predictions about how different partisan configurations would respond to unexpected surpluses and deficits, namely, that unified government would react more in terms of revenues and split governments more in terms of expenditures. We saw that indeed unified governments raise revenues in response to a surprise deficit and split legislatures do not. In contrast, is the total response of spending to a surprise surplus (as before, $b_2(a_2 + a_5) + b_4$) greater in the split case than the unified case? Table 3 contains somewhat mixed answers.

Each row of Table 3 contains the revenue response to an unexpected one-dollar deficit, the expenditure response, and the total response, which is the revenue response minus the spending response. For example, for each dollar of deficit, unified governments raise revenues by 46¢ and spending by 1¢ (for a total response of 45¢) in the next year. The response to a surplus is just the mirror image of this. (Since the permanent planned surplus is assumed zero and adjustments are made every year, we do not calculate "permanent" quantities in this case.) Split-branch governments have on average a much smaller revenue response (13¢) and do also cut spending (by 12¢). Split legislatures, as predicted, have the largest spending response (18¢) but the smallest total response, only 11¢ on the dollar. These qualitative differences between unified and divided governments also appear to exist in the South. All this is broadly consistent with our expectations, but the large standard errors prevent us from regarding it as more than weak confirmation.

Disaggregating the unified governments by party and deficit carryover laws reveals one important qualification. In three out of the four cases, as expected, the revenue response is larger than the spending response, which is insignificantly different from zero. Moreover, the total response is much larger (at least under Republicans) where deficit carryover prohibitions exist, which makes a good deal of sense.26 However, unified Democrats subject to strict deficit laws do not appear to respond strongly to unexpected deficits with either spending cuts or revenue increases, though we note that both lagged surplus coefficients in this case have large standard errors. Our simulation results below suggest that unified Democrats are more likely to respond to unexpected deficits where strict carryover prohibitions exist. In addition, symmetry with the surplus case implies that these constrained unified Democrats also respond to unexpected surpluses by raising spending. This is consistent with the assumption that they are a high-demand party, but outside our mod-

Split-Branch Government. Hypothesis 5 predicted four specific patterns of response by split-branch governments to fiscal shocks, depending on whether the high- or low-demand party controlled the legislature and whether the shock was positive or negative. Few significant results were found, probably because of insufficient data. The mixed quality of these results holds whether or not state-level fixed effects are included in the equation.²⁸ Of course, since we have not incorporated measurements of V, the partisan identity of the critical veto-override vote, nor stateto-state variations in veto provisions and majorities required for overrides, these anomalies could be due to measurement problems. Unfortunately, we can show that further predictable cases arise under splitbranch government depending not only on veto and override provisions, but also on which party last had unified control, so collecting further data is essential (Alt and Lowry 1993). In any case, while the results are promising, we do not carry the analysis of splitbranch government further at this time.

Simulation Results

The remaining hypotheses 4, 7, and 8 jointly predict that large cumulative deficits are likelier under split legislatures than unified governments. That is, deficit carryover laws should reduce the propensity of all governments to run deficits but in addition these laws, lacking any enforcement mechanism other than electoral accountability, should be less effective in circumscribing the behavior of split legislatures, other things equal. For reasons already given, we test these hypotheses with simulations. These describe the behavior of "typical" governments representing specific partisan configurations in specific constitutional settings, if each were to remain in power for 10 years in

Simulation Results				
TYPE OF NONSOUTHERN PARTY CONFIGURATION	INITIAL AVERAGE	MEDIAN FINAL	S.E.	90% INTERVAL
Long-run Revenue as % of Income				ALIENTA ANTONIO
Republicans	12.6	12.3	(1.5)	[10.2, 15.0]
Democrats	14.4	15.3	(3.3)	[9.1, 20.0]
Split Legislature	14.1	15.3	(3.4)	[10.0, 20.3]
Long-run Spending as % of Income				
Republicans	12.1	11.5	(1.5)	[9.6, 14.4]
Democrats	13.2	13.0	(2.6)	[8.8, 16.8]
Split Legislature	13.0	13.8	(3.3)	[7.2, 17.4]
Effect of Negative Income Shock on Per	r Capita Surplus (\$)			
Constrained governments				
Republicans		-2.1	(4.1)	[-9.3, 1.7]
Democrats		-1.7	(27.1)	[-44.5, 45.6]
Split legislature		-63.2	(15.4)	[-90.1, -43.1]
Unconstrained governments				
Republicans	-	-17.0	(8.4)	[-31.4, -6.0]
Democrats	*******	-49.5	(28.5)	[-97.3, -11.1]
Split legislature		-39.1	(28.8)	[-78.7, 14.5]

Note: Simulation methodology is explained in Appendix B. We obtain the 90% interval by omitting the values for the highest and lowest 5% of simulation runs. Descriptive statistics for revenues and spending are averages pooling runs for constrained and unconstrained governments weighted according to the number of cases shown in Table 1. Numbers of cases for constrained and unconstrained split legislatures are 59 and 54, respectively.

average economic circumstances, always allowing for the uncertainty of our estimates. We determine the effects of a surprise recession by comparing two scenarios for each configuration: in one the simulation begins in average economic circumstances, while in the other income is initially reduced by 10%, after which normal growth resumes, and nothing else differs. Given our data, 10 years uninterrupted government by any party configuration is unusually long (and a 10% shock is pretty large). Even so, the simulations give valuable insights into the meaning of the parameter estimates, or the direction in which they indicate the model would go, if governing configurations survived longer than they typically do.

Table 4 gives two main results, of which the first is simply that the models reveal no long-term tendency for any party configuration, be it Democrats, Republicans, or split legislatures, to run deficits per se. The top of the table gives the median simulated long-run outcomes for revenues and expenditures as shares of income, starting in normal, or actual average circumstances. Each line of the table contains the initial value used in the simulation, the median result (and standard error) of one hundred simulated runs, and the interval spanning all but the most extreme five cases at each end. The results show that a 10-year run of unified Republican government slightly reduces revenues as a share of income (by .3% relative to its initial average level), while spending as a share of income declines by a little more. Under unified Democrats, spending is virtually constant, but revenues rise.³⁰ None of these changes or differences remotely approaches statistical significance. There is a tendency for both revenues and spending to rise under split legislatures, again suggesting the possible importance of bilateral veto power and asymmetries in the desire for tax cuts and spending increases, though again the changes are not large in comparison to the standard errors of the estimates, so that our interpretation is speculative. However, the main point is that such spending increases and decreases as do appear to occur are financed by matching, parallel changes in revenues, regardless of party configuration.

This is important, because when the simulations begin with a major unexpected income shock, the results differ dramatically. The rest of the table shows what happens when the model is simulated after a "surprise recession" (i.e., the model starts with a 10% reduction in income from its actual average value, so that revenues drop and spending is suddenly "too high"). The long-run result under one-party unified government (either party, the results are statistically indistinguishable) subject to a deficit carryover constraint is that the deficit that results from the unexpected recession is entirely eliminated through fiscal policy. These long-run outcomes, extra deficits of \$1 to \$2 per capita attributable to the long-run effects of recessions, are tiny in comparison to the average per capita surpluses of \$75 to \$100 that existed in these subgroups. By contrast, where no deficit carryover laws exist, the model's estimates imply that both parties allow cumulative deficits larger than in the constrained case. The long-run Democratic deficits (about \$50 per capita more than in normal times) are larger than the Republican ones (below \$20 per capita), which reflects the relative spending pressures arising from the different targets of the parties. All these differences (except between the parties where no restrictive law exists) achieve significance levels of at least 10%, so they probably should not be ignored.³¹ While one could wish for the differences to be sharper, the results are clearly consistent with our initial hypotheses.

The simulation results also indicate that split legislatures would run larger cumulative deficits after recessions but apparently would do so regardless of whether or not the law permits deficits to be carried over from year to year, just as predicted in hypotheses 4 and 8. The differences between the constrained and unconstrained cases for split legislatures are not significant. One can infer that overall the average extra long-term deficit after a recession under a split legislature would be on the order of S50 per capita regardless of the legal environment. The difference between split legislatures and unified governments subject to deficit carryover laws is statistically significant³² and substantively significant as well. If these deficit carryover laws do actually work but only for unified governments, it seems likely that the ability to blame one party for undesirable outcomes is an important element in their effectiveness. In any event, these simulations confirm in the long run what Table 3 hinted at in the short run: unified governments subject to constraints respond more (and more quickly) to income shocks than do unconstrained unified governments, and more than split legislature governments regardless of whether the latter have legal restraints.

CONCLUSION

Our central message is straightforward. At the state level, fiscal outcomes—and fiscal instability in particular-result from an interaction of economic and political circumstances. Divided government matters, institutions matter, and party control matters. Our estimates suggest that even before the widespread problems of the last few years, recessions trigger state-level fiscal problems but that combinations of party and institutional control on the one hand and legal restrictions on fiscal policy on the other help explain their persistence. We show how, faced with an unexpected recession or inheriting a deficit, unified party governments not subject to deficit carryover laws might allow it to grow (if they remained in office) while those subject to such laws eliminate deficits more quickly. States with split legislatures also adjust less, regardless of the legal situation, in large part because divided legislatures do not appear to adjust revenues in response to surpluses and deficits.

Other recent research consistent with our conclusions came to our attention after we had completed most of the analysis. Using a wholly independent source of data covering fiscal 1988–92 (the period immediately following ours but containing wide-

spread deficits), Poterba (1994) shows that (1) unified state governments adjust more quickly to unexpected deficit shocks than do split governments, (2) unified governments make most of their adjustment with tax changes (rather than spending) and do so to a greater extent than split governments, and (3) where rules affecting budget balance are stronger, governments adjust more quickly to offset unexpected deficit shocks. He restricts his analysis to within-year changes, his definitions of the dependent and independent variables are slightly different from ours, and he makes no attempt to characterize the differences between parties; but his results are gratifyingly consistent with ours. They suggest that were our model and data used to forecast five further years (when the data are available), it should have some success.

Our results add a twist to recent interpretations of federal deficits in the 1980s. While we ofter further evidence of the role of a divided legislature in producing deficits (relevant to the federal experience in the first half of the 1980s), we also show that unified Democratic government not subject to deficit carryover constraints (the final fiscal years under Carter as well as the present situation at the federal level) should not be expected to reduce deficits in the face of recessionary pressures. Somewhat pessimistically, a literal translation of Tables 3 and 4 to the federal level suggests that unified Democratic government (or even to some extent split-branch government with a Democratic Senate) in the early 1980s would have raised taxes further, initially achieving a little more deficit reduction but (as Table 4 suggests) probably not achieving long-run results very different from those of a split legislature. Naturally, such interpretations are speculative, especially since our characterization of the state-level parties as representing highand low-demand constituencies may not apply well to the federal level.

On the other hand, the findings about the effect of deficit carryover laws are relevant to the national debate over balanced budget amendments. From our results, we conclude that laws affecting the end of the fiscal year are most likely to be effective and that they probably are effective (if sufficiently transparent in their requirements) by forcing explicit recognition of the past year's shortfall in the next year's plans. Moreover, the fact that they only affect the conduct of unified governments suggests that, ultimately, electoral accountability (or the threat of it) is the mechanism of enforcement, probably through the laws' enhancing the ability of media and organized groups to mobilize public opinion. A study of the effects of fiscal outcomes on election outcomes would be informative—as distinct from the state economy, for whose cyclical movements state politicians probably cannot reasonably be blamed (Chubb 1988; Peltzman 1987). Finally, we note that the desirability of such balanced budget laws hinges on how their benefits (fiscal responsibility) stack up against their costs (inflexibility in economic stabilization, much more significant at the federal than the state level). Taking

our results with Poterba's suggests that there is a gain in responsibility. Husted, Morton, and Waller (1992) find that even under these laws the loss of fiscal discretion is far from complete, so there is plenty of room left for debate.

Two methodological comments follow from the result that at the state level the Democrats have a significantly higher, and the Republicans a lower, target share of personal income for state spending. First, our model integrates the roles of economic cycles, income, federal contributions, and party control in determining state spending, rather than treating them as rival hypotheses to be tested individually. The fact that all these factors simultaneously have significant roles suggests that significant omit-ted-variables problems bias bivariate hypothesis tests. Moreover (as we have noted), almost all the states move in and out of different partisan configurations over time, and these different partisan configurations produce different fiscal outcomes. Thus efforts to attach "average" or "typical" partisan tags to individual states over time may swamp partisan differences with measurement error.

Finally, we believe the analysis of split-legislature outcomes contributes to the ongoing theoretical debate over the consequences of divided government between those who believe it is a moderating influence on policy (e.g., Alesina and Rosenthal 1994) and those who believe that coordination problems in divided government can impose significant costs (e.g., Roubini and Sachs 1989). We find little evidence for moderation, especially if spending targets are actually higher in split legislatures. However, any extra spending is normally financed through taxation, and there is no tendency for divided government automatically to produce gridlock. Divided government may produce fiscal instability but only conditional on experiencing economic instability. Naturally, no one case will settle the debate, but more concrete results can only be beneficial.

APPENDIX A: DERIVATION OF REDUCED-FORM EXPENDITURE FUNCTION

We start with the following structural model:

$$R_t = a_1 + a_2 R_{t-1} + a_3 Y_t + a_4 F_t + a_5 S_{t-1} + v_t$$
 (1)

$$E_t = b_1 + b_2 R_t + b_3 U_t + b_4 S_{t-1} + w_t$$
 (2)

After dropping all time subscripts except for lagged values, we can substitute for R in equation 2 to get

$$E = b_1 + b_2^*(a_1 + a_2^*R_{t-1} + a_3^*Y + a_4^*F + a_5^*S_{t-1}) + b_3^*U + b_4^*S_{t-1} + b_2^*v + w.$$

Rearranging terms and taking expected values gives

$$E = (b_1 + b_2 a_1) + b_2 a_3 Y + b_2 a_4 F + b_3 U$$

+ $b_2 a_2 R_{t-1} + (b_2 a_5 + b_4) S_{t-1}$.

Note that $(b_2a_2 - b_2a_2)^*E_{t-1} = 0$. Add $(b_2a_2 - b_2a_2)^*E_{t-1}$ to the righthand side to get

$$E = (b_1 + b_2 a_1) + b_2 a_3 Y + b_2 a_4 F + b_3 U + b_2 a_2 E_{t-1}$$
$$+ b_2 a_2 (R_{t-1} - E_{t-1}) + (b_2 a_5 + b_4) S_{t-1}.$$

Rearranging and collecting terms gives the reduced form:

$$= (b_1 + b_2 a_1) + b_2 a_2^* E_{t-1} + b_2 a_3^* Y + b_2 a_4^* F + b_3^* U + (b_2^* (a_2 + a_5) + b_4)^* S_{t-1}.$$
 (3)

APPENDIX B: MONTE CARLO SIMULATION METHOD

Our Monte Carlo approach was to do repeated but stochastically different runs by states with and without budget carryover constraints for each nonsouthern partisan group (omitting split-branch), and in each case with and without an adverse initial outcome shock. A set of simulations in each of these 12 cases (three partisan grouping times two legal situations times two income situations) requires carrying out steps 1 through 6.

- For each configuration of party control and budget constraint, our 3SLS produces a nine-by-nine variance-covariance matrix for the structural model coefficient estimates in the appropriate column of Table 1, and a two-by-two variance-covariance matrix for the additive residual terms.³³ Using those matrices, for each configuration repeat steps 2-6 one hundred times.
- Construct the parameter values for the structural model by adding a randomly drawn stochastic term (with variance equal to that estimated) to the coefficient point estimates.
- 3. Set the initial values of the variables for the base case equal to the means for the appropriate partisan group data subsample. For the negative shock case, use the same initial values except for income, which is set 10% lower than the mean.
- 4. Calculate the revenue and expenditures for time 1, including randomly drawn additive residual terms (from normal populations with standard deviations given by the regression standard errors) in each equation. The same residuals are applied to both base case and shock case.
- 5. Calculate the surplus for both cases at time 1 as revenues minus expenditures.
- 6. Continue updating through time 10, drawing a different pair of additive residuals for each iteration. Income is updated each period according to the grand mean annual growth rate (1.35%). The structural parameter values remain the same for all 10 iterations.

The result is a matrix of expenditures, revenues, and surpluses for one hundred runs of 10 iterations each, with randomly different parameter values for each run and randomly different additive residuals

for each time period of each run. The same stochastic terms and growth rate are applied throughout to the base case and shock case, so that the difference between the cases for a given configuration of party control and budget constraints is attributable solely to the fact that the shock case begins with a one-time, 10% drop in income.

Notes

We thank Michael Alvarez, John Freeman, William Keech, Gary King, James Poterba, and participants in workshops of the Center for Political Economy, Washington University, the University of Houston, and the Conference on Political Economics and Summer Institute in State and Local Public Finance (National Bureau of Economic Research, Cambridge) for helpful advice and comments. Financial support under NSF grant SBR-9223638 is gratefully acknowledged.

- 1. For political science studies of parties and policy, see Chubb 1988; Erikson, Wright, and McIver 1989; Garand 1988. For studies of budgeting, executives, and legislatures, see Behn 1991; Beyle 1992; Clynch and Lauth 1991. For a detailed study of powers, techniques, and institutions, see Hutchinson and James 1988. Public finance stresses the effects of fiscal health, the contributions of recession and recessionary surprises, changes in fiscal federalism and federal aid, differential cost elements, mandates at the state and national level, ability to pay, and indicators of need or demand (see Gramlich 1991). Some public finance studies of fiscal institutions are cited in Poterba 1994, especially Carter and Schap 1990 and Dearden and Husted 1990.
- 2. The 10 states include California, Connecticut, Illinois, Louisiana, Massachusetts, Maine, New York, North Carolina, Ohio, and Pennsylvania. In 9 cases the same party did not control the executive and both houses of the legislature. In the tenth, Louisiana, there was a major factional split between governor and legislature within the Democratic party.
- 3. In some models parties differ in the composition rather than level of spending (McCubbins 1991; Tabellini and Alesina 1990). For the conditions under which there is full policy convergence, see Alesina and Rosenthal 1994.
- 4. A full formal analytical model of the policy choices we analyze would also endogenize voters' choices and the resulting complex effects of transitions between types of partisan control. Alt and Lowry (1993) offer preliminary analyses of the latter.
- 5. No precise definition of state-by-state reversion levels exists. When a budget has not been passed by the beginning of the fiscal year, some states pass short-term continuing resolutions, based on prior-year spending levels. Experts believe that courts would not allow state government to shut down mandated services for long, so some continuing operation is always likely. A recent effort to codify the reversion level at the status quo ante by voter initiative in California was defeated.
- 6. A consequence of our use of a static model to analyze deficits is that we cannot deal directly with intertemporal aspects of debt, such as the desire to use debt to precommit political adversaries (Tabellini and Alesina 1990), nor do we construct a dynamic model of the deficit like that of Roubini and Sachs (1989). However, our steady-state focus enables us to examine partisan spending targets empirically and yields other results described. Two models with intertemporal and partisan differences in incentives to run deficits predict that borrowing should increase under unified governments in polarized settings (especially when they are uncertain of reelection) but offer no empirical tests of the predictions (Cohen and Noll 1984; Tabellini and Alesina 1990). Husted, Morton, and Waller (1992) failed to find significant preelection political cycles in fiscal policy at the state level. Alt and

Stewart (n.d.) examine the possibility of reconciling these approaches.

- 7. We believe that unforeseen revenue shocks (i.e., governments' greater control over the actual level of expenditures than of revenues) are more likely at the state level, where the government does not assume responsibility for defense or aggregate demand management. We lack data on targeted levels of spending and revenue for all states and years but note that recent research has found few systematic political and institutional effects on the accuracy of revenue forecasting (Bretschneider and Gorr 1992).
- 8. Note that we do not assume that one party is per se more desirous of borrowing than the other and thus predict no direct partisan effects on the level of the deficit. It could be the case that parties differ in both their desired level and composition of spending (McCubbins 1991) but we have not formally pursued this added complication.
- 9. Even a unified party government need not have a single bliss point, but again we present the simplest case. The closer the bliss points for the executive and legislature, the less likely there will be any adjustment in the expected level of spending, as we believe our discussion of split-branch governments demonstrates.
- 10. We assume that government can believe a revenue shock is transitory or, if not, that permanent income is producing more or less revenue than before.
- 11. Again, we simplify away institutional details of the bargaining game (e.g., conference committees) that would affect the identity of critical legislators. The key point is that these legislators have bilateral veto power over each other (McCubbins 1991).
- 12. Alesina and Drazen (1991) show (for heterogeneous interest groups bargaining over the tax burdens imposed by fiscal stabilization) that there is a subgame perfect Nash equilibrium where the time until the first concession is positive. Indeed, if the cost of delay is sufficiently low, no group may concede.
- 13. Another complication we ignore is that if one party nominally has the power to pass a bill in both houses but lacks the power to override a veto in one chamber, then there are two veto locations. We are currently collecting the data on override requirements and partisan majorities that will allow this possibility and others derived from the identity of V to be tested directly.
- 14. Where the executive has a line-item veto, the coalition situation is more complicated. There is likely to be considerable behind-the-scenes negotiations to determine exactly where the critical veto-override point is and what the indifference curves look like (see Beyle 1983; Carter and Schap 1990) and significant delay becomes more possible.
- 15. The data come from the Census Bureau's State Government Finances, various issues of the Book of the States, and the Statistical Abstract.
- 16. In our sample, states with unified Republican government have lower unemployment rates than Democratic states, partly because unified Republican government and lower unemployment were more common early in the period.
- 17. We group the 11 states of the former Confederacy (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, the Carolinas, Tennessee, Texas, Virginia) with the four border states in which Republicans failed to control even one chamber of the legislature for any year from 1968 to 1987 (Kentucky, Maryland, Oklahoma, West Virgiria) as "southern." We omit Nebraska because it has a non-partisan, unicameral legislature, Minnesota from 1968 to 1972 because the legislature was nonpartisan, and Maine from 1975 to 1978 because it had an independent governor, giving a total dataset of 931 state—years (631 outside the South). Control of legislative chamber is determined by which party held more seats. Where both parties held an equal number of seats, the chamber is assigned the party designation of the executive.
- 18. Two southern states had only unified Democratic government, while the other 13 also had split-branch government.
- 19. The 26 states include Alabama, Arizona, Arkansas, Colorado, Florida, Georgia, Idaho, Indiana, Jowa, Kansas,

Maine, Michigan, Mississippi, Missouri, New Jersey, New Mexico, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Utah, and West Virginia.

20. The National Association of State Budget Officers (NASBO) publishes a list of whether the laws have "enforcement provisions"—derived from a survey of state budget officers—but had no further information about exactly what this variable means. The ACIR has also published a 10-point scale of budget balance provision severity (see Poterba 1994), and the NASBO has released a classification of deficit carry-over prohibitions that would remove Arizona, Colorado, Michigan, and Pennsylvania from our list of 26 (see State Tax Notes, 27 July 1992). We have rerun our main results using this alternate listing: where the number of cases gets smaller, standard errors become larger, sometimes appreciably. The qualitative differences in behavior we describe still shine through, however.

21. The first three terms of equation 1 are an adaptive expectations setup in which expected (permanent) revenues are implicitly a function of past values of state income and federal contributions. This, adjusted for last year's unexpected surplus/deficit, yields the revenue forecast that starts the annual budget cycles, exactly as in our stylized description. Our formulation, with the revenue forecast made first (to finance targeted total spending), reflects our view of the "typical" process in the states, based on a mix of case analyses and discussions with budget officers. Naturally, which party set last year's fiscal policy affects the size of adjustment in transition periods. Moreover, in this model transitory demand increases expected borrowing, because it increases expected spending but not expected revenues, consistent with the distinction made in Barro's work. Empirically, these two equations contain the same explanatory variables used by Barro (1986) and Roubini and Sachs (1989). Because of incomplete data, we omit interest rates and expected prices, so we cannot judge the optimality of the borrowing that results. Finally, note that we do not measure fiscal surplus or deficit by examining additions to public debt (as an exact replication of Barro's model would require) but instead focus on the fiscal year deficit or surplus measured by the difference between total revenues and expenditures. We do this because states tend to use debt to finance long-run capital projects such as highway construction, so that there is no necessary connection between the yearly balance of spending and revenues and the total debt outstanding. In fact, the level of outstanding debt grew quite rapidly during the 1980s for some states that showed repeated, large surpluses on their annual balance sheets.

22. We prefer to report split-sample estimates (equivalent to a fully interactive, variable-intercepts, variable-slopes model) to keep the interpretation as clear as possible, even if it means accepting a little inefficiency from estimating some extra coefficients for interactions that are truly irrelevant. We could certainly economize by a few parameters: for instance, the two Unified Democrat spending equations are almost identical, and we do not claim that the partisan and regional types we identify differ in every way. However, the inclusion of a few irrelevant parameters should introduce no bias and will not lead to exaggerated claims of significant differences between types. To deal with fixed effects while avoiding excessive tracking of small numbers of outlying observations, variables were only included in any equation for states with nine or more observations in that subsample. In our data, this guarantees that a state appears as a separate variable in at most one partisan subcase, and no state variables appear that are not statistically significant in at least one equation. The states meeting these criteria include Connecticut, Iowa, Indiana, Massachusetts, Missouri, New Hampshire, New Jersey, New Mexico, Montana, Rhode Island, South Dakota, and Vermont.

23. The model in Tables 1 and 2 survived various hypothesis tests (conventional F-tests for the joint contribution of several coefficients based on differences in sums of squared errors). It offers significant improvement over a simple model

pooling all observations and estimating the nine parameters of equations 1 and 2, as well as models that do not control for deficit carryover provisions and unified governments. Moreover, it strictly dominates (fewer parameters, better fit) a model in which each state has a separate intercept in each equation. Even more important, a model in which each state has a separate slope and intercept does not offer significant improvement over that in Tables 1 and 2. Finally, incorporation of state-level fixed effects and separation by deficit carryover laws in the split-branch and split-legislature cases failed to produce significant improvement. All these tests are conservative in that we have not engaged in extensive specification searching to minimize the number of included parameters.

24. Equation 3 has a Koyck lag structure, which is mathematically equivalent to an equation that relates current expenditures to the weighted average (with geometrically declining weights) of all current and past values of income, federal contributions, unemployment, and surpluses or deficits. If we set each of the past and present values of the explanatory variables equal to their "permanent" expected value, then the effect on spending becomes the coefficient in equation 3 multiplied by the reciprocal of 1 minus the coefficient of lagged expenditures, $b_2a_3/(1-b_2a_2)$. Figures for all unified governments are averages calculated from Table 1, weighted in proportion to the number of cases in columns 1–4.

25. Note that the expression for the permanent income contribution to spending is a nonlinear function of several parameters. We therefore obtained an estimate of the standard deviation by simulation methods, using the estimated coefficients' variance—covariance matrix as a guide.

26. Remember that these laws often specify the conditions under which deficits must be extinguished rather than forbidding them outright, so the effectiveness of the laws is not incompatible with the existence of at least transitory borrowing. We experimented with regressions using other definitions of the laws (governor must submit or legislature must pass a balanced budget) and found no significant differences according to whether or not such laws existed.

27. Similarly, to some extent nonsouthern split legislatures spend an unexpected surplus rather than cut taxes. We had assumed for modeling purposes that legislators were equally averse to sustained surpluses and deficits and that adjustment in either direction would thus be equally difficult. In a period of persistent surpluses, a significant positive spending coefficient means that split legislatures are unable to exercise discipline in holding the line on spending following an unexpectedly large surplus, spending "free money" under bilateral veto pressure. That is, the high-demand party wants the extra spending more than the low-demand party wants the tax cut.

28. For Democratic legislatures facing Republican governors, the pattern of results confirms the hypothesis. If the shock is positive, spending rises, though only by a little. If the shock is negative, revenues subsequently rise (significantly) and spending falls. When Republican legislatures combine with Democratic governors, an unexpected surplus does result in lowered revenues. However, in this case the expected spending decrease does not appear, nor does spending decrease following an unexpected deficit in this case. However, we note that again (as in the split-legislature case), the anomalies arise over the spending behavior of Republican legislatures, so it could be that the model is too limited as well. Also, there are fewer cases of actual deficits in the present data, so we took the expedient course of letting the surplus/deficit split fall at the average shock in each case. Even so, there are only slightly over a hundred cases of each partisan type, so this means estimating nine parameters from 50 cases, leaving little room for adding state-level fixed effects or the effects of deficit carryover laws.

29. Intuitively, one simulation can be described as follows. Use the coefficients from a column of Tables 1 or 2—with the independent variables set to mean values for that column's subgroup of cases—to solve for revenues, then expenditures, and thus a new surplus. With this new surplus, let income

grow at an average rate, hold everything else constant, add a random shock, and solve again, iteratively, until 10 periods are complete. Do one hundred such interated calculations, each of which differs from the others only in its random elements which represent pure shocks in each period plus the uncertainty surrounding the parameter estimates. To isolate the effects of an initial recession, rerun an identical (random elements apart) set of simulations save only that in the first period of each income is reduced by 10% and see what effect this change of starting conditions has in the long run. The exact construction is spelled out in Appendix B. The coefficients were based on regressions with data to which population adjustments have been made which differ slightly from those reported in Table 1.

30. The differences between parties, particularly in longrun revenue levels, are clearer and approach statistical significance at conventional levels in the cases where strict deficit

laws exist. We are unsure how to interpret this.

31. Based on Table 4, the results are that for the difference between Republicans and Democrats, conditional on a carryover being allowed, t = 1.25 (p < .15) and that for the difference between states allowing carryovers and no carryovers, conditional on unified Republicans, t = 1.58 (p < .10)and conditional on unified Democrats, t = 1.35 (p < .10).

32. The results are that for the difference between Republicans and split legislatures, conditional on no carryover, t = 3.87 (p < .005) and for the difference between Democrats and split legislatures, conditional on no carryover, $t=2.19\ (p<.025)$. The numbers of cases for constrained and unconstrained split legislatures are 59 and 54, respectively.

33. The coefficients for split-legislature cases separated by carryover restrictions did not differ significantly from each other; thus the pooled estimates were reported in Table 1.

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DOES ATTACK ADVERTISING DEMOBILIZE THE ELECTORATE?

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Te address the effects of negative campaign advertising on turnout. Using a unique experimental design in which advertising tone is manipulated within the identical audiovisual context, we find that exposure to negative advertisements dropped intentions to vote by 5%. We then replicate this result through an aggregate-level analysis of turnout and campaign tone in the 1992 Senate elections. Finally, we show that the demobilizing effects of negative campaigns are accompanied by a weakened sense of political efficacy. Voters who watch negative advertisements become more cynical about the responsiveness of public officials and the electoral process.

It is generally taken for granted that political campaigns boost citizens' involvement—their interest in the election, awareness of and information about current issues, and sense that individual opinions matter. Since Lazarsfeld's pioneering work (Berelson, Lazarsfeld, and McPhee 1954; Lazarsfeld, Berelson, and Gaudet 1948), it has been thought that campaign activity in connection with recurring elections enables parties and candidates to mobilize their likely constituents and "recharge" their partisan sentiments. Voter turnout is thus considered to increase directly with "the level of political stimulation to which the electorate is subjected" (Campbell et al. 1966, 42; Patterson and Caldeira 1983).

The argument that campaigns are inherently "stimulating" experiences can be questioned on a variety of grounds. American campaigns have changed dramatically since the 1940s and 1950s (see Ansolabehere et al. 1993). It is generally accepted that television has undermined the traditional importance of party organizations, because it permits "direct" communication between candidates and the voters (see Bartels 1988; Polsby 1983; Wattenberg 1984, 1991). All forms of broadcasting, from network newscasts to talk show programs, have become potent tools in the hands of campaign operatives, consultants, and fund-raisers. In particular, paid political advertisements have become an essential form of campaign communication. In 1990, for example, candidates spent more on televised advertising than any other form of campaign communication (Ansolabehere and Gerber 1993).

We are now beginning to realize that the advent of television has also radically changed the nature and tone of campaign discourse. Today more than ever, the entire electoral process rewards candidates whose skills are rhetorical, rather than substantive (Jamieson 1992) and whose private lives and electoral viability, rather than party ties, policy positions, and governmental experience, can withstand media scrutiny (see Brady and Johnston 1987; Lichter, Amundson, and Noyes 1988; Sabato 1991). Campaigns have also turned increasingly hostile and ugly. More often than not, candidates criticize, discredit, or belittle their opponents rather than promoting their own ideas

and programs. In the 1988 and 1990 campaigns, a survey of campaign advertising carried out by the *National Journal* found that attack advertisements had become the norm rather than the exception (Hagstrom and Guskind 1988, 1992).

Given the considerable changes in electoral strategy and the emergence of negative advertising as a staple of contemporary campaigns, it is certainly time to question whether campaigns are bound to stimulate citizen involvement in the electoral process. To be sure, there has been no shortage of hand wringing and outrage over the depths to which candidates have sunk, the viciousness and stridency of their rhetoric, and the lack of any systematic accountability for the accuracy of the claims made by the candidates (see Bode 1992; Dionne 1991; Rosen and Taylor 1992). However, as noted by a recent Congressional Research Service survey, there is little evidence concerning the effects of attack advertising on voters and the electoral process (see Neale 1991).

A handful of studies have considered the relationship between campaign advertising and political participation, with inconsistent results. Garramone and her colleagues (1990) found that exposure to negative advertisements did not depress measures of political participation. This study, however, utilized student participants and the candidates featured in the advertisements were fictitious. In addition, participants watched the advertisements in a classroom setting. In contrast to this study, an experiment reported by Basil, Schooler, and Reeves (1991) found that negative advertisements reduced positive attitudes toward both candidates in the race, thereby indirectly reducing political involvement. This study, however, was not conducted during an ongoing campaign and utilized a tiny sample, and the participants could not vote for the target candidates. Finally, Thorson, Christ, and Caywood (1991) reported no differences in voting intention between college students exposed to positive and negative advertisements.

We assert that campaigns can be either mobilizing or demobilizing events, depending upon the nature of the messages they generate. Using an experimental design that manipulates advertising tone while holding all other features of the advertisements constant, we demonstrate that exposure to attack advertising in and of itself significantly decreases voter engagement and participation. We then reproduce this result by demonstrating that turnout in the 1992 Senate campaigns was significantly reduced in states where the tone of the campaign was relatively negative. Finally, we address three possible explanations for the demobilizing effects of negative campaigns.

EXPERIMENTAL DESIGN

There is a vast literature, both correlational and experimental, concerning the effects of televised advertisements (though not specifically negative advertisements) on public opinion (for a detailed review, see Kosterman 1991). This literature, however, is plagued by significant methodological shortcomings. The limitations of the opinion survey as a basis for identifying the effects of mass communications have been well documented (see Bartels 1993; Hovland 1959). Most importantly, surveys cannot reliably assess exposure to campaign advertising. Nor is most of the existing experimental work fully valid. The typical experimental study, by relying on fictitious candidates as the "target" stimuli, becomes divorced from the real world of campaigns. Previous experimental studies thus shed little evidence on the interplay between voters' existing information and preferences and their reception of campaign advertisements. When experimental work has focused on real candidates and their advertisements, it is difficult to capture the effects of particular characteristics of advertising because the manipulation confounds several such characteristics (Ansolabehere and Iyengar 1991; Garramone 1985; Pfau and Kenski 1989). That is, a Clinton spot and Bush spot differ in any number of features (the accompanying visuals, background sound, the voice of the announcer, etc.) in addition to the content of the message. Thus there are many possible explanations for differences in voters' reactions to these spots.

To overcome the limitations of previous research, we developed a rigorous but realistic experimental design for assessing the effects of advertising tone or valence on public opinion and voting. Our studies all took place during ongoing political campaigns (the 1990 California gubernatorial race, the 1992 California Senate races, and the 1993 Los Angeles mayoral race) and featured "real" candidates who were in fact advertising heavily on television and "real" voters (rather than college sophomores) who on election day would have to choose between the candidates whose advertisements they watched. Our experimental manipulations were professionally produced and could not (unless the viewer were a political consultant) be distinguished from the flurry of advertisements confronting the typical voter. In addition, our manipulation was unobtrusive; we embedded the experimental advertisement into a 15-minute local newscast.

The most distinctive feature of our design is its

ability to capture the casual effects of a particular feature of campaign advertisement—in this case, advertising tone or valence. The advertisements that we produced were identical in all respects but tone and the candidate sponsoring the advertisement. In the 1992 California Senate primaries, for example, viewers watched a 30-second advertisement that either promoted or attacked on the general trait of "integrity." The visuals featured a panoramic view of the Capitol Building, the camera then zooming in to a closeup of an unoccupied desk inside a Senate office. In the "positive" treatments (using the example of candidate Dianne Feinstein), the text read by the announcer was as follows:

For over 200 years the United States Senate has shaped the future of America and the world. Today, California needs honesty, compassion, and a voice for all the people in the U.S. Senate. As mayor of San Francisco, Dianne Feinstein *proposed* new government ethics rules. She *rejected* large campaign contributions from special interests. And Dianne Feinstein *supported* tougher penalties on savings-and-loan crooks.

California needs Dianne Feinstein in the U.S. Senate.

In the "negative" version of this Feinstein spot, the text was modified as follows:

For over 200 years the United States Senate has shaped the future of America and the world. Today, California needs honesty, compassion, and a voice for all the people in the U.S. Senate. As state controller, Gray Davis opposed new government ethics rules. He accepted large campaign contributions from special interests. And Gray Davis opposed tougher penalties on savings-and-loan crooks.

California can't afford a politician like Gray Davis in the U.S. Senate.

By holding the visual elements constant and by using the same announcer, we were able to limit differences between the conditions to differences in tone.² With appropriate modifications to the wording, the identical pair of advertisements was also shown on behalf of Feinstein's primary opponent, Controller Gray Davis, and for the various candidates contesting the other Senate primaries.

In short, our experimental manipulation enabled us to establish a much tighter degree of control over the tone of campaign advertising than had been possible in previous research. Since the advertisements watched by viewers were identical in all other respects and because we randomly assigned participants to experimental conditions, any differences between conditions may be attributed only to the tone of the political advertisement (see Rubin 1974).

The Campaign Context

Our experiments spanned a variety of campaigns, including the 1990 California gubernatorial election, both of the state's 1992 U.S. Senate races, and the 1993 mayoral election in Los Angeles. In the case of the senatorial campaigns, we examined three of the four primaries and both general election campaigns.

The campaigns we examined were all characterized by extensive broadcast advertising and, in most cases, by frequent use of negative or attack advertising.

We used the same design for all of the campaigns under investigation. That is, we manipulated advertising valence within the identical audiovisual framework. The content of the experimental advertisement, however, varied across campaigns. In general, the experimental advertisements focused on issues or themes that were particularly salient in the various campaigns. In the 1990 gubernatorial race, we created advertisements that dealt with the issues of crime and pollution. In the positive conditions, the sponsoring candidate was presented as "tough" on crime and a protector of the environment. In the negative versions, the opponent was depicted as "soft" on crime and indifferent to the quality of the environment. (Samples of the text and accompanying visuals of the experimental manipulations are provided in Appendix A.)

The experimental advertisements for the 1992 Senate primaries dealt with either the candidates' personal integrity or competence. In the case of integrity (discussed in the given examples), the advertisement described the candidate as either honest and a supporter of campaign reform or as dishonest and an opponent of reform. In the case of competence, the advertisement asked voters to consider the sponsor's "ability, determination, and leadership" (or the absence of these characteristics in the opponent).

During the Senate general election campaign, we shifted the focus of the advertisements to the issue of unemployment. The condition of the state's economy and the significant loss of jobs (unemployment had reached 10% in September) were the overriding issues in both races. All four candidates aired advertisements promising to reverse the state's economic decline. Our treatment advertisements depicted the sponsor or opponent as an advocate or critic of government-subsidized job training and industrial modernization programs.

Finally, one of our studies concerned the nonpartisan election for mayor of Los Angeles between Richard Riordan and Michael Woo. Here, the manipulation dealt with the candidates' integrity and discussed the degree to which the candidates' campaign promises to increase job opportunities and reform city government were consistent with their past actions.

In summary, our experimental advertisements dealt with a variety of campaigns and themes. In all cases, however, the advertisements corresponded to the actual focus of campaigns. In their content, the experimental advertisements closely reflected the advertisements aired by the candidates.

Subjects and Procedure

We recruited subjects by multiple methods including advertisements placed in local newspapers, flyers distributed in shopping malls and other public venues, announcements in employer newsletters, and by calling names from voter registration lists. Subjects were promised payment of \$15 for participation in an hour-long study of "selective perception" of local news programs.

Although the "sample" was obviously nonrandom, our participants resembled the composition of the greater Los Angeles area. Across all the experiments, 56% of the participants were male, 53% were white, 26% were black, 12% were Hispanic, and 10% were Asian. The median age was 34. Forty-nine percent of the participants claimed affiliation with the Democratic party, 24% were Republicans, and 21% were independents. Forty-four percent were college graduates, with the balance being evenly divided between high school graduates and individuals with some college.³

The experiments were conducted at two separate locations: West Los Angeles and Costa Mesa (Orange County). The former is a heavily Democratic area, while the latter, an affluent suburb of Los Angeles, is predominantly Republican. The experimental facilities in both locations were identical—a three-room office suite consisting of two viewing rooms and a separate room for completion of questionnaires (in addition to a reception area). The viewing rooms were furnished casually with sofas and easy chairs. Participants could browse through newspapers and magazines and snack on cookies and coffee.

When participants telephoned the facility they were scheduled for a particular time period of their choice. Experimental sessions were available from 10:00 A.M. to 8:00 P.M., Monday through Saturday. The typical session consisted of two to three participants.

On arrival, subjects were given an instruction sheet informing them that the study concerned selective perception of local newscasts. They then completed a short pretest questionnaire concerning their social background, media activities, and political interest. Following completion of the pretest, participants were taken to a viewing room, where, depending upon the condition to which they had been assigned,⁴ they watched a 15-minute (complete with commercials) videotape recording of a recent local newscast (described to participants as having been selected at random).

The experimental or "treatment" advertisement was inserted into the first commercial break midway through the tape. The political spot was shown always in the middle position in a three-advertisement break. As described, the advertisements in the various conditions were identical in all respects except for the factors of valence and source.

Following completion of the videotape, participants completed a lengthy posttest questionnaire tapping their beliefs and opinions on a wide range of campaign issues. Of course, we also ascertained participants' voting intentions and general level of involvement in the campaign. On completion of the posttest, participants were debriefed and paid.

ANALYSIS AND RESULTS

We shall limit our analyses to the effects of negative advertising on intention to vote. In our post-test questionnaire, we ascertained whether participants were registered to vote. Using registration as a filter, we then asked, "Looking forward to the November election, do you intend to vote?" (In the case of the primary election study, the question was worded accordingly.) We identified "likely voters" as those who were both registered and who stated their intention of voting.⁵

In analyzing our experimental data, we pooled the gubernatorial study, the various senatorial studies, and the mayoral study into a single data set. While the effects of attack advertising are tempered by campaign-specific constraints, including the background of the candidates and the specific content of their advertising, we are especially interested in the average effect, if any, of advertising valence. Moreover, pooling the separate studies makes it possible to obtain reliable estimates of the demobilizing effects of attack advertising.

After pooling, we compared the percentage of viewers classified as likely voters among participants who watched the positive and negative versions of the experimental advertisements. The demobilization hypothesis predicts that exposure to negative advertising will lower the percentage of likely voters. Among those who watched a positive advertisement, 64% intended to vote. Among participants who saw a product advertisement instead of a political one, 61% intended to vote. Among participants who were exposed to the negative versions of the campaign advertisement, only 58% were likely to vote. A oneway analysis of variance yielded an F-statistic of 2.2, significant at the .11 level.⁷

The decision to vote depends upon aspects of the campaign other than advertising valence. In addition, some people are more likely to vote than others, regardless of the nature of the campaign. To capture these contextual and dispositional effects on turnout, we regressed intention to vote (using a logistic regression) on advertising tone and a set of dummy variables corresponding to specific elections, as well as various indicators of individual differences. Because the positive and negative advertisements exerted symmetric effects on voting intention, we specified advertising tone as a trichotomy corresponding to positive advertisement (+1), no political advertisement (0), and negative advertisement (-1). The individual difference variables included the frequency with which people said they followed public affairs, prior voting history, the "match" between viewers' and the candidates' gender and party identification, age, race, and education. This multivariate analysis, in essence, estimates the independent effects of the campaign advertising stimulus on voting intention above and beyond campaign-specific influences and personal predispositions.

Table 1 presents the results of two logistic regressions corresponding to a full model (with all control

TABLE 1

Logistic Regression Estimates of the Effect of Tone on Intentions To Vote in the 1990, 1992, and

1993 Experiments

	1	MODEL
VARIABLE	FULL	RESTRICTED
Constant	212	230
	(.331)	(.331)
Advertising tone ^a	.110	.114
•	(.055)	(.036)
Experiments	40.4	
1990 gubernatorial	.434	.477
1000	(.203)	(.195)
1992 primary	.404 (.179)	.335
1000	` '	(.168)
1992 general election	1.221 (.208)	.778 (.138)
4000 Australia	, ,	` '
1988 turnout	1.746 (.141)	1.614 (.128)
Follow gov't offgire	.497	.501
Follow gov't. affairs	.497 (.059)	.501 (.058)
Independent	-1.112	-1.122
muependent	(.108)	(.108)
Same party	028	(
Same party	(.087)	
Same gender	033	
Guillo golidor	(.117)	
Age	.002	******
90	(.004)	
Education	.100	.129
	(.068)	(.058)
Female	.034	
	(.119)	
White	.346	.353
	(.131)	(.128)
Log likelihood	-905.5	-906.7
% correctly predicted	78.2	78.1

Note: Entries are logit coefficients with standard errors in parentheses. N=1,655.

"Coded 1 for positive ad, 0 for control ad, -1 for negative ad.

variables included) and a restricted model (with nonsignificant controls excluded). The baseline (constant) in this specification represents the 1993 Los Angeles mayoral race. Off-year local elections tend to be characterized by low levels of citizen involvement. Not surprisingly, the 1990 gubernatorial, the 1992 Senate primary, and the 1992 Senate general elections all registered higher turnout.

The individual difference factor with the greatest ability to discriminate between likely and unlikely voters, as expected, was prior voting history. Participants who reported voting in the 1988 election were much more likely to be classified as likely voters in 1992–93 than those who reported not having voted. Partisans, those with higher levels of political interest, the more educated, and whites were also charac-

terized by significantly higher levels of voting intention.

From our perspective, the most important result in Table 1 is the effect of advertising tone on voting intentions. In both equations, a one-sided t-test showed that advertising tone significantly (at the .05 level) affected turnout. Converting the logistic coefficient on advertising tone into a linear probability shows that those participants exposed to the negative version of the advertisement were 2.5% less likely to vote than those exposed to no political advertisement. Conversely, the positive version of the advertisement increased voting intention by 2.5 percentage points. In short, the initial estimate of the demobilization effect survived the multiple controls.

Overall, the experimental results demonstrate that exposure to negative (as opposed to positive) advertising depresses intention to vote by 5%. Considering the scope of our experimental manipulation (a single 30-second advertisement embedded in a 15-minute newscast) and the variety of campaigns examined, these effects seem remarkable. Despite our best efforts at experimental realism, it is possible that the effect has been magnified by some aspect of the research design. It is important, therefore, to place the experimental findings in the context of the world of actual campaigns.

Replicating the Experimental Results

To reconstruct our experimental framework in the real world, we measured the tone of the campaign in each of the 34 states holding a Senate election in 1992. Senate campaigns are especially appropriate for our purposes because the candidates rely heavily on advertising (Ansolabehere and Gerber 1993). Moreover, four of our seven experiments focused on Senate campaigns.

Our indicator of campaign tone was based on a systematic content analysis of news coverage of the various Senate races. We searched through the NEXIS and DATATIMES data bases for all newspaper and newsmagazine articles bearing on the Senate campaigns in general and the candidates' advertisements in particular. This search yielded a total of over 2,500 articles ranging from a high of 1,000 on the Feinstein–Seymour contest in California to a low of 28 in the case of the Idaho race. Based on a reading of the news coverage, campaigns were classified into one of three categories: generally positive in tone (scored 1); mixed (scored 0); and generally negative in tone (scored -1). The classification scheme is described in Appendix B, along with each state's tone score.

As our indicator of turnout, we simply computed the votes cast for U.S. Senate and divided by the state's voting-age population. In addition to turnout, we also examined ballot *roll-off* in the Senate elections. For each relevant state, we subtracted the total number of votes cast for senator from the total cast for president and divided by the latter. The roll-off indicator has two distinct advantages. First, roll-off is a campaign-specific effect indicating the degree to

which people who were sufficiently motivated to vote in the presidential election chose to abstain in the Senate race. Second, because roll-off uses the presidential vote as a baseline, it adjusts for a variety of state-related differences (e.g., demographic factors, political culture and party competition), which affect the level of voting turnout.⁸

Turnout in senatorial elections depends upon a variety of influences in addition to the tone of the campaign. These include the competitiveness of the race, the volume (or "decibel level") of the campaign, and the electorate's sense of civic duty. (For a thorough discussion, see Rosenstone and Hansen 1993). Our measure of the volume of the campaign was the level of campaign spending by incumbents and challengers (measured in logarithms). Competitiveness or closeness was measured by the squared difference between the Republican and Democratic shares of the total vote. Lastly, to incorporate differences in civic duty and other relevant orientations, we also controlled for per capita income, turnout in the 1988 presidential election, percentage college-educated, region (South, non-South), and the census form mail-back rate.9

Having compiled the turnout, roll-off, and campaign tone indicators, we proceeded to replicate the experimental results. Table 2 presents the results from the full and restricted multiple regression analyses of turnout and roll-off. Following the analysis of the experimental data, the tone variable was specified as a trichotomy (negative tone =-1, mixed =0, positive tone =1). This specification measures the deviation in turnout and roll-off of the positive and negative campaign tone categories from the mixed-tone category. ¹⁰

Do Senate races characterized by relatively negative campaigns have lower turnout and higher roll-off rates than races in which the campaign is more positive in tone? For both turnout and roll-off, we found significant effects of campaign tone. Negative campaigns decreased turnout by 2%. (This also means that positive campaigns boosted turnout by 2%, for a total difference of 4%.) Negative campaigns also increased ballot roll-off by 1.2% and vice-versa. Since the demobilization hypothesis is directional, we resorted to one-tailed tests (i.e., negative campaigns decrease turnout and increase roll-off, while positive campaigns increase turnout and decrease roll-off). The t-statistics for this hypothesis were 3.64 for turnout and -2.26 for roll-off, both significant at the .05 level. 11

The use of both experimental and nonexperimental methods to measure the very same naturally occurring phenomena is highly unusual in the social sciences. It is even more unusual if both methods yield equivalent results. In our study, the aggregate-level analysis of turnout and roll-out in the 1992 Senate elections and the experimental studies of negative advertising converge: negative campaigns tend to demobilize the electorate.

TABLE 2 Regression Estimates of the Effect of Tone on Turnout and Roff-off in the 1992 Senate Elections

	TU	JRNOUT°	RC	LL-OFF ^b	
INDEPENDENT VARIABLE	FULL MODEL	RESTRICTED	FULL MODEL	RESTRICTED	4
Constant	294 (.171)	295 (.124)	.157 (.173)	.150 (.040)	
Campaign tone ^c	.020 (.006)	.021 (.006)	011 (.006)	012 (.005)	
1988 turnout	.550 (.101	.571 (.090)	.046 (.102)		:
Per capita income	.010 (.027)	_	.048 (.027)	.049 (.019)	
Mail-back rate	.337 (.149)	.340 (.125)	−.058 (.151)		
Southern state	.048 (.015)	.047 (.013)	014 (.015)	016 (.013)	
% College-educated	.120 (.099)	.172 (.076)	215 (.100)	247 (.067)	
Log challenger \$.001 (.005)	_	011 (.005)	010 (.004)	
Log incumbent \$.013 (.007)	.011 (.006)	004 (.007)		
. Open seat	.011 (.012)	· —	009 (.012)		i
(Non)closeness	053 (.046)	068 (.039)	.058 (.046)	.069 (.037)	
R² SS residuals	.94 .0099	.94 .0102	.67 .101	.64 .0112	•

Note: Entries are multiple regression coefficients with standard errors in parentheses. N = 34.

*Total Votes for Senate Voting-age Population.
b(Total Votes for President - Total Votes for Senate)/(Total Votes for President).

= positive tone, 0 = mixed tone, -1 = negative tone.

Psychological Correlates

That attack advertisements discourage people from voting raises questions about the psychological underpinnings of this effect. One possibility is that partisanship mediates the effects of attack advertisements on turnout. It is generally thought that campaign messages resonate especially strongly among supporters or proponents of the source of the message. Campaigns thus have the effect of reinforcing or crystallizing existing partisan loyalties. Extending this argument to attack advertising implies the obverse. That is, the intention to vote among supporters of the candidate airing the negative advertisement will be unchanged, since the message provides no reasons to vote for their candidate. On the other hand, voting intention should be weakened among supporters of the candidate who is attacked, since the message provides these partisans with reasons not to vote for their candidate.

If attack advertisements demobilize on a partisan basis, we should find a significant interaction effect between advertising valence and viewers' party identification. We thus reestimated the logistic regression

presented in Table 1, this time including the appropriate interaction (valence × same party). The results revealed that the interaction term was nonsignificant and had the wrong sign.12 Partisanship does not mediate the demobilizing effects of attack advertising; supporters of the source and target candidates are not affected differently.

An alternative account of the demobilization effect is that attack advertising generates blanket negativity toward both candidates. According to this "plagueon-both-your-houses" explanation, voters not only become more critical of the target of the attack but turn against the sponsor as well (for some evidence of this effect, see Basil, Schooler, and Reeves 1991). We investigated this possibility by examining participants' evaluations of the personal traits of both the sponsor and the target of the attack advertisements.¹³ Our results indicated that attack advertisements generally "work." That is, ratings of the target were generally less positive after participants watched the attack. Ratings of the sponsor, however, were generally unaffected, suggesting that participants did not penalize candidates for airing negative messages.14

The fact that attack advertisements do not demobilize on a partisan basis and do not induce negativity towards *both* candidates suggests a third explanation for demobilization. Negative advertising may affect voting intent by conveying cues not about the candidates but about the nature of political campaigns and the political influence of ordinary citizens. Perhaps the act of attacking another candidate in a 30-second advertisement denigrates the entire process.

To explore this possibility, we measured our participants' sense of political efficacy—their beliefs in the responsiveness of public officials and electoral institutions to popular will. We used four questions, coded the responses to each as either efficacious (1) or inefficacious (0) and computed the average response to all four items. Among viewers who were exposed to the positive versions of the campaign advertisement, the mean efficacy score was .24. The mean was no different (.23) among participants in the control group. In the case of viewers who were exposed to the negative versions of the advertisement, the mean dropped to .19. The F-statistic from the one-way analysis of variance was significant at the .02 level. In short, exposure to campaign attacks makes voters disenchanted with the business of politics as usual.

In summary, we have considered three possible explanations for the demobilizing effects of attack advertisements: partisan demobilization, a plague-on-both-houses effect, and general cynicism. Our evidence points toward the third. Among our experimental participants, exposure to attack advertising significantly weakened confidence in the responsiveness of electoral institutions and public officials. As campaigns become more negative and cynical, so does the electorate.

CONCLUSION

Taken together, our studies demonstrate that attack advertising extracts a toll on electoral participation. In the experiments, voting intention dropped by 5% when participants were shown an attack advertisement in place of a positive advertisement. Our aggregate-level replication of the experimental results suggests that Senate turnout in 1992 was roughly 4% lower when the candidates waged relatively negative campaigns. Since the scope of the experimental manipulations never exceeded a single advertisement, our estimates of the demobilizing effects of campaign attacks may be conservative. Over the course of two or three weeks of sustained negative advertising, the flight of voters can be more substantial.

The effects of attack advertising on the decision to vote have significant implications for our understanding of the impact of campaigns on electoral outcomes. Voter withdrawal in response to negative advertising also raises questions concerning the legitimate and fair uses of broadcast advertising. ¹⁷

The most important implication of these results is that in the era of media campaigns, both surges and declines in turnout can be generated by high-intensity campaigns. Candidates with sufficient resources can, through the use of negative messages, keep voters away from the polls. Campaigns are not inherently mobilizing forces, and the secular decline in presidential and midterm voter turnout since 1960 (for evidence, see Rosenstone and Hansen 1993) may be attributed, in part, to the increasingly negative tone of national campaigns.

Finally, this research raises normative questions concerning the trade-off between the right to political expression and the right to vote. Should candidates be free to use advertising techniques that have the effect of reducing levels of voter turnout? In the case of publicly financed presidential campaigns, is it legitimate for candidates to use public funcs in ways that are likely to discourage voting? How do we weigh the public interest in free political expression against the competing public interest in widespread public participation? When, if ever, should politicians' expression be restrained or subjected to incentives to modify its form or content?

In other areas of public communication, allegations of "antisocial" effects have prompted extensive analysis and debate. In some areas, the outcome has been governmental regulation. Thus the tobacco companies have been banned from using the airwaves for certain forms of commercial speech and are required to include mandated health warnings in their print advertisements. Direct regulation of political speech, which is at the core of the values protected by the First Amendment, is probably both impossible as a matter of law and undesirable as a matter of policy. The classic remedy in this society for injurious speech is simply "more speech." However, there is precedent in the law governing the broadcast media requiring that "equal time" be given to the targets of certain "personal attacks" (see Ferris and Leahy 1990). Possibly, new regulations governing the broadcast media ensuring that the targets of attack advertisements have reasonable opportunity to respond (regardless of their own financial resources) should be considered. Ohio and other states are currently experimenting with "truth in political advertising" guidelines designed to make candidates think twice before resorting to false or misleading advertising. However, approaches that simply ensure that there will be 'more speech" miss the essential point raised by this research, which is that negative advertising impacts adversely on voting; remedies that can only multiply the number of negative advertisements will exacerbate, rather than address, the essential problem.

The more realistic approach to influencing the tone of campaign advertising rests on voluntary or incentive-based restraints. There have been several instances in which public controversy over the content of entertainment programming has prodded the networks, local stations, or record companies to withdraw the program in question. Similar reasoning is embodied in legislation pending in Congress that seeks to reform campaign advertising. (For a discussion of recent legislative efforts, see O'Neill 1992). One bill would impose a double standard on adver-

tising rates under which only "positive" advertisements would be entitled to the "lowest unit rate" rule. Other suggestions include the so-called inperson rule, under which the candidates would be required to deliver their attack statements in person (on camera).

A third set of suggestions for reform addresses the use of media "monitoring." In the aftermath of the 1988 campaign, the press decided to scrutinize the candidates' paid messages (in the form of "ad watches," "truth boxes," and the like). Anticipation of critical news coverage may deter candidates from relying heavily on attack messages. To this point, however, there is no reliable evidence concerning the effects of these monitoring effects on voter response to advertising.

We do not yet understand the implications of these various approaches. Some would certainly raise objections from civil libertarians, others would be objectionable to those concerned with political competitiveness. As in the case of campaign finance reform, broadcast advertising reform may work to benefit those in office at the expense of challengers. Although providing incentives for campaigns to air "positive" messages provides no assurance that these messages will be more substantive, verifiable, or honest, they would, at least, be less likely to deter voting. While the case for broadcast advertising reform has yet to be made, the relationship between negative advertising and voting suggests that these issues are worth further research and discussion.

APPENDIX A: SAMPLES OF EXPERIMENTAL ADVERTISEMENTS

The wording and visuals used in two of our studies appear below. The changes associated with the negative versions of the advertisements are given in parentheses.

1990 Gubernatorial Study: Crime

Text. It's happening right now in your neighborhood. A generation of youth slowly dying. ______ is (is not) the candidate who intends to stop this tragedy and preserve California's future. As mayor of _____, added (reduced the number of) police officers, constructed (blocked) new jails, and fought hard against drugs (opposed drug education programs). The result: major crime rates fell (increased) by 12%. His (her) record won the endorsement (was condemned by) the California Association of Police Chiefs. They know _____ will push for (will oppose) tougher sentencing and strengthen (weaken) our state's justice system.

Visuals. Schoolchildren on playground; addict injecting heroin; body bag being removed from crime scene; police officers outside courthouse; interior of prison cell; candidate logo.

1992 Senate Study: Unemployment

Text. Since 1990, California has lost two-and-a-half million jobs. The state now has the highest unemployment rate in the nation. California needs elected officials who will end the recession. _____ will work (has done nothing) to bring jobs back to our state. As a U.S. Senator, _____ will introduce legislation (_____ opposes legislation) to increase funding for job training programs and to give California companies incentives to modernize and expand their factories and plants. California needs (can't afford) _____ in the U.S. Senate.

Visuals. Closed factory; graph showing state's unemployment rate; lines at unemployment office; picture of candidate (opponent); factory workers assembling planes; workers on construction site; candidate logo.

APPENDIX B: CLASSIFICATION OF 1992 SENATE CAMPAIGNS ACCORDING TO ADVERTISING TONE

We examined a subset of the NEXIS and DATATIMES data bases that contained full-text reproductions of articles from more than 30 major daily newspapers and five politically oriented magazines (e.g., the Cook Political Report, the Roll Call, and the Hotline). A separate search was conducted for all 34 senatorial campaigns. The search was designed to access all articles about the campaign printed after the primary and before the general election. When the search produced more than 150 articles (as was the case for seven races), than a further search command focusing on campaign advertising was added. This procedure elicited a total of 2,573 articles.

Each article was read by a graduate student coder who specifically looked for discussion of campaign tone. The coder followed a strict scheme in order to place each race into one of the three campaign-tone categories. If a majority of the tone-related references to a campaign were negative (e.g., it was characterized as being nasty, dirty, or vicious and provided specific examples of negative attacks from each of the race's candidates), the race was coded as negative. If at least three articles specifically mentioned that one of the Senate candidate's was deliberately refraining from making a negative response to the opponent's attacks and no later article contradicted this information, then the race was coded as mixed. Finally, when the news coverage yielded no discernible information about negative campaign tone, the race was coded as positive.

We validated our news-based classification scheme by asking two major political consultants (David Hill, Republican, and Mark Mellman, Democrat) to rate each of the Senate campaigns on the same three-point scale. The consultants disagreed with our classification in only one instance (Kentucky), and we deferred to their expertise. (The analysis is unchanged if Kentucky is eliminated from the analysis.) Each state's tone score is shown in the following list:

		=
Negative Tone	Mixed Tone	Positive Tone
Negative Tone Arkansas California (6-yr. seat) California (2-yr. seat) Colorado Connecticut Georgia Indiana Kentucky Louisiana New Hampshire New York North Carolina Ohio Oregon Pennsylvania	Mixed Tone Alabama Arizona Florida Illinois Missouri Oklahoma	Positive Tone Alaska Hawaii Idaho Iowa Kansas Maryland Nevada North Dakota South Dakota Utah Vermont Wisconsin
South Carolina		

(Note that the California 6-year seat was contested between Boxer and Herschenson and the 2-year seat, between Feinstein and Seymour, and that the Louisiana general election was uncontested.)

Notes

Washington

This research was supported by generous grants from the John and Mary R. Markle Foundation, the National Science Foundation, and the University of California. We are indebted to Kelley Carlin, Diana Estrada, Travis Dixon, Terri Hall, Mikel Healy, Clark Hoover, Victoria Mitchell, Erin O'Neal, Raza Syed, and Sharmaine Vidanage for superb research assistance.

- 1. We use these terms interchangeably to describe whether the advertisement, or the campaign as a whole, focuses on a candidate's positive aspects or on the opponent's liabilities and faults.
- In addition to minimizing the visual differences in the advertisement, we also used identical logos, in which the sponsoring candidate's name appeared in large red letters against a brown backdrop.
- 3. Using a weighted average of Los Angeles and Orange counties as the baseline, the demographics for our sample match closely for age (median of 34 versus 31), gender (44% vs. 51% male), race (52% vs. 47% white), and partisanship (49% identifying Democratic vs. 47% registration). Our participants deviated in the local area in two respects: 26% of our participants were African–American (compared to 10% in Los Angeles and Orange counties), and 44% were college graduates (compared to 24% for the local area).
- 4. Random assignment of participants to experimental conditions was used throughout. The use of random assignment assures (subject to the rules of probability) that differences in the dependent variable can be attributed only to the experimental manipulation (see Campbell and Stanley 1969, 25). We took the additional precaution of controlling for a number of background variables considered predictive of participation, including partisanship, prior voting history, age, and education.
- 5. Responses to the two questions were generally cumulative; that is, few people who said they were not registered indicated an intention to vote. These respondents were classified as unlikely voters.

- 6. For a more detailed analysis of the effects of advertising valence in specific campaigns, see Ansolabehere, Iyengar, and Valentino 1993.
- 7. The number of cases was 1,716. Since the F-statistic is nondirectional, that is a conservative test of the demobilization hypothesis.
- 8. The average roll-off in the 1992 Senate elections (rounded to the nearest thousand) was 127,000, with a maximum of 609,000 and a minimum of -13,000. In percentage terms, roll-off averaged 4.9% with a range of -1.8% to 11.6%.
- The Bureau of the Census mails forms to every resident in each state. The mail-back rate is the fraction of forms that are completed and returned.
- 10. A simple F-test revealed that the symmetry restriction was justified.
- 11. There are a variety of other interesting results in this analysis, but since we are especially interested in the effects of campaign tone, we set them aside for future consideration.
 - 12. These results are available from the authors.
- 13. Participants rated the candidates' intelligence, honesty, compassion, leadership, toughness, arrogance, weakness, and deviousness. We formed an index by subtracting the number of negative ratings from the number of positive ratings.
 - 14. These results are available from the authors.
- 15. The questions asked participants to agree or disagree with the following statements: (1) "Generally speaking, those who get elected to pubic office keep in touch with the people in their constituencies"; (2) "In this country, politics works for the benefits of a few special interests, rather than the public good;" (3) "Most politicians are willing to tackle the real problems facing America;" (4) "Having elections makes government responsive to the views of the people."
 - 16. The F-statistic was 4.0, with 1,716 cases.
- 17. It is possible, of course, that negative advertising also exerts prosocial effects not tapped by our studies. For instance, there is some evidence that negative advertisements allow voters to differentiate more readily between candidates' issue positions, thus facilitating "issue voting" (see Garramone et al. 1990; Patterson and McClure 1976).

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RIDING WAVES OR MAKING WAVES? THE SERVICES AND THE U.S. DEFENSE BUDGET, 1981–1993

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Bureaucratic politics is the favored explanation of those addressing the perversities of defense budgeting. But it is arguably devoid of politics, given its dependence on either aggregate top-down or horizontal models. I seek to redirect analysis. I disaggregate defense spending (by service and weapon type) and study budget sensitivity to program pressures in the buildups and builddowns of the Reagan—Bush eras. Applying a two-equation model to time-series cross-sectional data, the analysis shows weapon budgets increasing with program diversification and a commitment to defense spending. In turn, it shows programs diversifying to accommodate service objectives: when turning to missions, the services increased program varieties while concentrating program resources.

ureaucratic politics is the favored explanation of those who ponder the perversities of weapon acquisition and defense budgeting. The idea that weapons and budgets are outcomes of internal governmental conflict prompted numerous studies detailing the competitive strategy and tactics involved (Armacost 1969; Evangelista 1988; Halperin 1974; Hampson 1989; Huntington 1961; Kotz 1988; McNaugher 1989). These studies are complemented by valuable statistical treatments (Majeski 1983; Marra 1985; Mintz 1988, 1989; Nincic and Cusack 1979; Ostrom 1978; Ostrom and Marra 1986). Despite their important contribution, the latter insufficiently capture the insights of the nonstatistical literature. Most often, statistical studies reveal budgets responding to prior-year amounts or to amounts proposed by other actors in the budgeting process. As a result, they subordinate bureaucratic politics to budgeting "incrementalism" (or "inertia") when not confusing the two. They thereby focus on outcomes remote from their underlying political causes within the defense bureaucracy. Statistical studies certainly acknowledge the disaggregated dynamics of defense budgeting; yet even then, they do so incompletely. For example, the military services and/or the Department of Defense are routinely treated as a single decision maker in a process that is implied to be either "horizontal" or "top-down" (on the latter, see Su, Kamlet, and Mowery 1993).

I seek to redirect analysis by offering a model that is open to both top-down and bottom-up influences. It disaggregates defense spending by service and weapon type and studies budgetary sensitivity to program pressures in the Reagan–Bush era—a period of an unprecedented buildup and less striking build-down. By assessing program concentration (and its opposite, diversification), it asks whether the services responded to higher budgets or instead actively created the budgetary realities of the period. Indeed, the latter is suggested by the bureaucratic politics literature, where the services are depicted as seeking versatile and diverse weapons to claim missions and resources—a quest well within service capability given the service role in the lengthy and complex

defense planning, programming, and budgeting cycle (see Hampson 1989, 6–27; Puritano 1985). More explicitly, in examining Reagan–Bush defense spending, this study asks whether service programming followed defense budgeting or vice versa, that is, whether budgetary increases (decreases) caused program expansion (contraction) or the reverse.

Accordingly, this study tests two equations. The first represents a top-down perspective that assumes a coherent actor (the executive branch) choosing among budgetary categories. The second adopts a bottom-up perspective that recognizes programming pressures exerted by self-interested services. While each equation incorporates the determinants of one level of the decision process (budgeting or programming), each acknowledges the possible influence of the other level. The resulting model is tested with cross-sectional time-series data drawn from presidential budget requests from fiscal years 1981-93 for the three services (air force, navy, and army) and for eight service-weapon categories: air force aircraft and missiles; navy aircraft, missiles, and ships: and army aircraft, missiles, and combat vehicles.

BUREAUCRATIC POLITICS

Bureaucratic politics has proven a fruitful perspective from which to understand the outcomes and deficiencies of the acquisition process. It has generated a literature rich in insights and examples. However, it is marred by conceptual deficiencies that limit its explanatory and predictive utility.

Controlling Acquisition

According to numerous intensive studies of defense budgeting and acquisition, bureaucratic politics dominates acquisition throughout, as weapons are initiated and shaped through rivalry. The military services and subservices are key acquisitional competitors. They use weapons to stake claims, to acquire resources, and to affirm key missions. (In this, Pentagon officials, the White House, and defense contract-

ing firms also compete even if they are not, strictly speaking, "bureaucratic" competitors.) The services bargain with other participants, form coalitions, control information, and manipulate the acquisition process to obtain key weapons. They engineer threats, strategically choose requirements, limit oversight, select and distort data, and skip and collapse acquisition stages to create political momentum and proscribe less desired alternatives.

Weapon requirements (or conceptions of "need") are especially critical to political control and follow from overlapping and changing service missions. The air force, navy, and army pursue weapons that serve essential missions: combat missions, controlling the seas, and conducting ground warfare, respectively. And they engage in politics, reinterpreting missions to match new technology or redirecting old technology to new purposes. As service instruments, requirements are animated by the complex relationship between missions and weapon technology and performance. Politics pervades weapon requirements, because any mission can be served by many different means and any means can serve many different missions. That is, bureaucratic politics thrives on weapon substitution. A service can use the incomplete fit between mission and technology to advantage, but so can rivals.

Bureaucratic competition partly occurs over single programs. Such programs are valued for key performance yet are made versatile to claim other missions or to deflect claims of other services. Single weapons can expand or contract performances to serve bureaucratic interests in three major ways. First, requirements defer to less critical missions to protect or promote more crucial ones. For this reason, during the 1980s, the air force sought multipurpose interdiction aircraft in the guise of close air support (CAS) by offering an attack version of the F-16 (the A-16) to replace an aircraft specifically designed for close air support (the A-10). Resisting army pressure to acquire dedicated CAS aircraft, the air force offered improved CAS performance largely as a byproduct of general aircraft capability enhancement. Old missions were even offered in the guise of new ones. In the 1990s, navy submariners envisioned changed missions for attack submarines—from deep-water to the shallow-water antisubmarine contingencies of regional conflict, but also from antisubmarine to such land-oriented functions as performing cruise missile attacks, engaging in intelligence gathering, transporting special forces, and undertaking the symbolic functions long performed by battleships and aircraft carriers."

Second, requirements are written to encroach surreptitiously upon rival missions. In the 1950s, the army disguised its effort to acquire an intermediaterange ballistic missile capability. Dissatisfied with the 1947 formalization, "Armies walk, navies sail, and air forces fly," the army sought to acquire ballistic missiles (the Jupiter) by emphasizing the link between the performance of missiles and artillery. Of course, the air force defended against these encroachments

by associating longer-range missiles with its strategic mission. By achieving restrictions on the range of army tactical ballistic missiles, it successfully fought off army attempts to place a technological "foot in the door" (Armacost 1969).

Third, requirements are supported or added when program alternatives are proscribed (or killed). When, during the late 1960s and early 1970s, limits were imposed on launchers (first by McNamara and then by SALT), the air force turned to multiple warheads to improve target coverage and force survival. But all the services sought flexible weapons with limited budgets and infrequent program startups. In the 1980s and 1990s, weapons such as the air force B-2 stealth bomber and air force advanced technical fighter (ATF), the army light (stealth) helicopter and M1 Abrams tank, and the navy SSN-21 attack submarine were deemed especially useful in increasing political support and ensuring adequate mission coverage.

Bureaucratic competition also yields multiple programs. Program numbers expand or contract to serve bureaucratic interests in four basic ways. First, requirements are written creating new weapons to protect other favored weapons. These requirements can be designed to ensure that the cost of one project does not endanger another. In the 1980s, the financial health of the advanced tactical fighter program, for instance, certainly moved the air force to avoid entirely new CAS aircraft. Requirements can also be designed to maintain a division of labor among weapons to ensure that projects do not threaten the rationale for more desirable ones by offering the same mission at a lower price. In the 1990s, air force advance tactical fighter proponents supported designs for an F-16 replacement that tilted toward the air-to-ground (rather than the competitive air-to-air) mission and then advocated that new designs be shelved in favor of a somewhat upgraded F-16 (Morrocco 1992, 34). Moreover, deficiencies in requirements are addressed by promoting companion weapon systems. For instance, the navy answered criticisms of aircraft carrier vulnerability by stressing the defensive layers that would have to be penetrated by an attacker.

Second, requirements are written to avoid undesirable or controversial missions and thus to create a rationale for multiple systems. The navy, for instance, spurned air force tactical aircraft and missions and, in the late 1980s, failed to embrace the ATF. Both services desired an air-superiority fighter, but each rejected the other's missions: the air force pursued interdiction and the navy sought carrier compatability, air defense, and ground support (United States, General Accounting Office 1990).

Third, requirements are formulated to promote multiple programs, regardless of overlaps. Multiplicity reduces political risk, avoids choices that could alienate bureaucratic factions, and increases political leverage to secure the survival of at least one program. In the 1970s, elected officials justified opposing one weapon (the B-1) by supporting another (cruise

missiles or the stealth bomber) (Kotz 1988, 153). Politically, critics cannot easily oppose multiple service programs. President Carter apparently endorsed the MX missile because he opposed the B-1 bomber (pp. 167–72); and Henry Kissinger apparently accepted the Trident and cruise missiles to acquire the support of the joint chiefs of staff for the SALT I agreement (Hampson 1989, 189). In any case, multiplicity increased the service claim to available resources.

Finally, requirements are written to kill a project, especially when a joint-service one, creating a need for single-service weapons. Demonstrating a subtle, passive variant of this tactic, the air force and navy might have sabotaged the joint advanced tactical fighter (ATF/NATF) program by allowing the aircraft to evolve away from navy requirements: the air force was to commit to a contractor (in 1991) for full-scale development when the navy was only completing concept demonstration/validation.

Bureaucratic politics thus centers on weapon substitution, as actors protect and promote immediate interests while constructing supportive political coalitions and combating opposing ones.³ In this environment, players can direct requirements toward single yet versatile weapons made to perform in "essential" ways though able to perform multiple missions or toward multiple weapons made to perform newly defined or more broadly defined missions. (Only the latter is the subject of the present analysis.)⁴

So directed, service program decisions can place upward pressure on the procurement budget. By diversifying programs, the services can claim future resources. Once committing procurement funds to a program, the services can argue for more funding to reverse the uneconomic production rates associated with program "stretch-outs" (services sink costs in production, then argue for more funds to reduce production inefficiencies), to keep assembly lines warm, to capitalize on the learning curve possible with large production contracts, to correct for intentionally underestimated procurement costs, to meet the increased budgetary demands of previously deferred procurement (in the 1980s, the budgetary "bow wave"), or to procure weapons linked to existing ones (tankers to refuel bombers, precision-guided weapons to serve aircraft, attack aircraft to serve aircraft carriers, etc.).

In this, Congress is variously portrayed as an active or passive accomplice. Like the services, legislators are commonly assumed to be actively self-interested: they promote programs with constituent benefits, since defense dollars mean jobs and profits and jobs and profits mean votes and campaign support. Yet while "simple, parsimonious, and persuasive," porkbarrel explanations "are also mostly wrong" (Mayer 1991, 210). Statistical analyses are rarely as exhaustive or conclusive as on this point: constituent contract benefits do not predict votes in either the House or the Senate (see, e.g., Lindsay 1990). These analyses are backed by the indirect evidence that Congress

rarely increases funding over Pentagon-requested levels. Indeed, Congress is not a highly diligent or effective acquisition manager, despite charges of excessive congressional interference in program management ("micromanagement"). The Congress generally defers on matters of acquisition policy; and when intervening, it acts through symbolic stands and informational requests that do not restrict legislative or executive options. It largely accepts the number and variety of programs contained within Defense Department requests; and when recommending changes, it does so partly to keep program funding within the overall budget. By the time Congress gets involved, options are already screened and foreclosed, and programs are too far along to be canceled. Possessing limited resources, expertise, and access, informed only after important decisions are made, and relying on dated and incomplete information, Congress cannot effectively influence crucial decisions on weapon performance, source selection, and cost.

Legislative constraints are not lost on the services. Hardly naive participants in weapon acquisition, the services establish requirements to promote missions and suppress the costs of doing so. For as long as possible, they perpetuate the myth that a weapon is required, on schedule, and is progressing as planned. The day of reckoning might come when requirements are found to be overstated and performance and/or cost targets are shown not to have been met. At that point, the political package can unravel. Critics will join forces, supportive alliances will form again, and new political opportunities for intervention can arise. Still, weapon proponents can prevail. When a financial commitment is made to a weapon, it becomes less costly than alternatives, and—ironically—alternatives can be hurt by the fear that any new program, like the current one, will develop beyond control.

Unanswered Questions

Despite the compelling image of the services as political players, the bureaucratic politics perspective leaves important questions unanswered. The bureaucratic politics perspective fails to address whether the services, in diversifying, respond more to budgetary opportunities (the top-down perspective) or to their own technological capabilities and mission objectives (a bottom-up perspective). Fronically, if bureaucratic politics remains the conventional explanation for the perversities of acquisition, it is because it embraces three ambiguous assumptions.

First, the bureaucratic politics perspective focuses on single goals (missions or interests) and neglects trade-offs and choices among objectives. It therefore understates the complexity of decision making by failing to ask whether mission and interests can conflict and, if so, under what circumstances and with what consequences. In not exploring the relationship between missions and objectives, bureaucratic politics fails to recognize that "missions" and "interests" can be incompatible: sometimes key mis-

sion performance can be purchased only with a severe price—as when weapons perform below expectation, perform inappropriately for actual missions, or drain scarce resources. Bureaucratic politics thus fails to establish under what circumstances the services translate missions into weaponry and what costs the services willingly incur.

Second, the bureaucratic politics perspective sometimes relies on assumed "uncertainty." Attributing decisional lapses (performance failings, cost overruns, etc.) to incomplete information (rather than irrationality), theorists can retain the core rationality assumptions of bureaucratic politics. Nevertheless, the bureaucratic politics perspective was not originally developed around sophisticated understandings of uncertainty and risk. Thus it does not indicate whether players are best described as risk-adverse or risk-taking. It also leaves open the possibility that uncertainty actually reflects nonrational thinking rather than informational ambiguities, gaps, and contradictions in policymaking, planning, and analysis. In any case, uncertainty can itself undermine rationality, given the problems that decision makers have in estimating and analyzing probabilities—problems that could lead them to ignore risk entirely. Bureaucratic politics thus fails to establish whether and when the services act conservatively in response to the unknowns of high-technology acquisition.

Third (and most relevant to this inquiry), the bureaucratic politics perspective generally ignores the motives of participants. It is ambiguous about whether actors should be viewed primarily as conflictual or cooperative, aggressive or passive, expansionist or preservationist. Sometimes the services are shown coveting the missions of others, unable to live by agreements, and seeking to increase their resource share. At other times, the services are shown rejecting new missions, desiring to compromise, and willing to accept their traditional share of the resource pie. Bureaucratic politics does not reveal the conditions under which the services seek to extend gains (an "active" stance) or to preserve them (a "reactive" stance). For one thing, it does not indicate whether the services measure gain in absolute or relative terms: if the former, the services could be satisfied with gains despite those of service rivals; if the latter, the services could always be dissatisfied, especially if all strive for superiority. Nor does bureaucratic politics reveal the conditions under which the services seek to maintain existing missions (an "inward" orientation) or to augment them (an "outward" orientation). As Figure 1 shows, stance does not determine orientation: if active, the services can be territorial or imperialistic; if reactive, the services can conservatively resist change or acquire new missions when protecting older ones or handed opportunities (filling a political vacuum).

The bureaucratic perspective thus requires significant elaboration. It must disclose why and when the services extend or contract commitments⁷—that is, whether and under what conditions the services react to budgetary opportunities or instead create budget-

FIGURE Four Dis		eaucratic Person	nality Types
		ORIENT	TATION
		Inward	Outward
STANCE	Activ•	Tenttorial	Imperialistic
SIANCE	Passive	Conservative Caretaker	Opportunistic Detensive

ary realities. In other words, it must reveal whether bureaucracies have distinct personalities (or motives) and under what conditions these personalities are revealed.

Seeking insights into bureaucratic motives, I shall examine the four different "personality types" presented in Figure 1. These types are based on two interconnected dichotomous dimensions—stance (active/passive) and orientation (inward/outward)—that can be empirically measured. Accordingly, as shown in Figure 2, an active stance is assumed to prevail when service program decisions determine the weapon budget, and a passive stance is assumed to prevail when the opposite is true. An inward orientation is assumed to prevail when programs contract with larger budgets, an outward orientation, when the reverse. (Here, orientation represents movement toward or away from concentration in single programs—presumably those critically tied to the sential" service mission [see Halperin 1974]—for beyond preferring combat platforms, the navy pursued control of the seas as a carrier mission; the air force conceived strategic "bombing" as a long-range, nuclear (and now also conventional) bomber mission; and the army organized its missions around the tank and now attack helicopters.)

THE MODEL

I shall examine procurement at two levels, budgetary and programmatic. The budgetary level assumes a decision hierarchy, where macro budgetary choices inform programmatic decisions. The programmatic level assesses program choices from below as an upward pressure on service budgets. Yet importantly, each level is open to the potential effects of the other. The budgetary level reflects overall levels of civilian budgetary support (from the White House or Department of Defense) for a service and/or weapon

FIGURE 2 **Expected Results for Different Bureaucratic Personality Types ORIENTATION** Inward **Outward** PROGRAMMING **PROGRAMMING** Active Temtorial **Imperialistic** POSITIVE NEGATIVE STANCE BUDGETING BUDGETING Opportunistic Conservative **Passive** Caretaker Defensive POSITIVE NEGATIVE

UPPER-LEFT CORNER: Causal priority—programming or budgeting? BOTTOM-RIGHT CORNER: Direction of relationship between programming concentration and budget size—positive or negative?

type and general trade-offs within the defense budget among research-and-development (R&D), operations-and-maintenance (O&M), manpower, and procurement. But the budgetary level is also hypothetically linked to programming pressure so as to determine whether service program tactics can explain what macro allocation decision cannot. In turn, the programmatic level presents program contraction and expansion as service political instruments. The extent of program concentration thus changes with service goals (mission objectives) and available means (technological capabilities). But like the budget, concentration levels could be open to cross-level (or multilevel) influences and are thus assessed also for their responsiveness to the prior-year weapon budgets (i.e., budgetary opportunities).

The two levels yield a model consisting of two (somewhat interactive) equations:⁸

WBDGT =
$$a_0 - b_1$$
 ACONC $(t-1) + b_2$ DEFCOM $-b_3$ OMTOFF

(1)

ACONC =
$$a_0 - b_1$$
 WBDGT $(t - 1) - b_2$ MISEMP $- b_3$ RDBLNC,

(2)

where wbdct is the service weapon budget; ACONC (or UCONC) is program concentration levels for program budget amounts (or for unit costs, controlling for the number of weapons procured); DEFCOM (defense commitment) is the service budget for nonweapons and includes R&D, O&M, and "procurement" (other than designated aircraft, missiles, ships, or combat vehicles); OMTOFF is the budgetary trade-off between O&M

and procurement (a ratio of O&M to the combined O&M and procurement budgets); MISEMP (mission emphasis) is the proportion of the weapon budget devoted to combat platforms, and RDBLNC is the balance between R&D and procurement spending (here, conceived as an interaction term).

The sign of all coefficients in equation 2 and the concentration variable in equation 1 reflect the expansionary effects commonly assumed in the bureaucratic politics literature. These relationships could be signed in the opposite direction to indicate contractive effects (as the discussion will reveal).

Equation 1 assesses aggregate budgets and their responsiveness to executive allocation preferences. For one thing, the equation posits weapon budgets to be positively related to the executive commitment to nonweapon defense spending (DEFCOM). By aggregated a wide range of budget categories, this variable has a number of characteristics that fit with executive commitment: it is insensitive to changes in emphasis within defense budgeting, it includes amounts (e.g., for O&M) that are relatively stable in the period assessed, it captures the effects of scale (among time periods and service budgets) and therefore subsumes variance attributable to "incrementalism," and it is better conceived as explaining—than as explained by-weapon budgets. The equation also suggests that weapon budgets respond to trade-off decisions (OMTOFF) among budgetary categories. Specifically, it tests whether decision makers sought to improve the operational readiness of existing systems or to procure new ones (the latter being the apparent response of the Reagan administration to the "hollow force" of

the seventies). While decision makers could also trade-off R&D and procurement, investing in future rather than current technology (the Bush administration's proposed post–Cold War transition plan emphasizing weapon prototyping over producing), the trade-off between O&M and procurement is likely to be more important in the period assessed. Finally, equation 1 defers to cross-level influences, incorporating the rival bureaucratic politics thesis through ACONC (and UCONC)—testing budget sensitivity to prior-year program concentration.

Equation 2 directly assesses programmatic influences by explaining concentration—the spread in program funding. For one thing, it posits that concentration responds to the service desire to expand, through mission emphasis (MISEMP). This variable represents the proportion of the entire service weapon budget devoted to "combat platforms" and "strategic weapons"-for the air force, strategic bombers (the B-1B and B-2), fighters (the F-15), and attack planes (the F-16); for the navy, combat ships (carriers, cruisers, destroyers, and frigates), fighters (the F-14), and attack planes (the F/A-18); and for the army, attack helicopters (the AH-64 Apache) and tracked combat vehicles (the Abrams M-1 tank, the Bradley Fighting Vehicle, etc.). These systems, crucial to service missions, are assumed to be emphasized at the times when the services choose to expand or to contract the service role (thereby enlarging or narrowing the service-weapon mix). Indeed, in these defining periods, the services might do both—expand program variety measured in unit costs (UCONC) while concentrating budgetary resources (ACONC) in more-essential programs.

If mission emphasis captures a service's will to expand, RDBLNC taps a service's capability to produce a large array of technology. It assumes that rather than being traded off against each other, R&D and procurement funding must converge for new technologies to be introduced. In other words, both are assumed to be necessary—and neither is assumed to be sufficient—for program expansion in a high-technology age. Such a balance seems especially likely to explain diversification measured in unit costs (UCONC) (i.e., controlling for the number of weapons procured) as R&D expenditures likely generate programs but are a "sunk cost" that does not increase linearly with increased production. But the R&D/ procurement balance could still yield diversification in program budget amounts. Under the prevailing practice of "concurrency," the R&D and production budgets are tied together. Even if weapons are prototyped in full-scale development, development continues into production because systems remain incomplete and continue to be elaborated, modified, or changed to capitalize on unforeseen technology opportunities or to address unforeseen problems from delayed testing, subsystem integration, or production.

Like equation 1, equation 2 tests for effects of the rival thesis by positing a relationship between program concentration (ACONC or UCONC) and the prioryear weapon budget (WBDGT). It thereby acknowl-

edges that relative concentration may respond to available resources rather than determining them, with the services widely distributing available funds (the hypothesized expansionary effect) or else burying them in essential programs (a contractive effect). In all, then, equation 2 tests three sources of expansion: objectives (MISEMP), capability (RDBLNC), and opportunity (WBDGT). ¹⁰

While offering a more dynamic, political view of the budgetary process, the model still makes two simplifying assumptions: (1) it does not assume that the services differ fundamentally in their motives but rather that they diverge in objectives, capabilities, and opportunities; (2) it does not account for changes in the political environment (e.g., the opportunities and threats presented by rival service budgets and weapons) but assumes that these changes equally affect the three services (and weapon types) except when influencing through the incentives and constraints modeled here. (For instance, while the army, by nature, seems labor-intensive and hence less likely to expand, it also has a greater R&D-procurement imbalance.)

THE DATA

All model variables are measured using available budgetary data. The resulting indicators show interservice difference over time and intraservice differences across weapon types.

Creating the Indicators

Measures were created from administration budget requests found in congressional hearings for fiscal years 1980–93, 11 including proposed program amounts and quantities and R&D, "procurement," and O&M funding. 12 They were constructed as indicated in the Appendix. 13

Program concentration is measured by the Herfindahl–Hirschman index (Hirschman 1945), adjusted by the square root (Michaely 1984) to increase sensitivity in midrange. The measure varies between 0 and 1.0, with larger values associated with higher concentration (on concentration measures, see Taagepera 1979). Concentration was calculated for program budget amounts (ACONC) and program unit costs (UCONC), both by service (e.g., air force) and service—weapon type (e.g., air force missiles).¹⁴

Weapon budget (WBDCT) includes allocations for certain kinds of nongeneric weapons, usually designated by type and model (e.g., the Trident II missile). It includes most air force aircraft and missiles; navy aircraft, missiles, and ships; and army aircraft (helicopters), missiles, and combat vehicles. ¹⁵ It excludes support equipment, ammunition, modifications, and some generic systems ("trainers." "decoys," etc.). ¹⁶ All of the spending ("other" procurement) was added to R&D and O&M spending to create the nonweapon budget, measuring the defense commitment (DEFCOM).

		AIR F	ORCE			NA	VY			ARI	MY	
VARIABLE	80-82	83–86	87–90	91–93	80-82	83-86	87-90	91–93	80-82	83-86	87–90	91–93
Amounts ^a									- 1100			,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,
Weapon	5.5	19.5	9.6	8.5	13.6	24.2	19.3	12.9	4.2	8.8	5.6	2.2
Other	10.1	20.6	20.6	13.9	2.6	10.4	8.4	9.2	3.5	10.6	8.5	4.8
R&D	7.0	13.4	14.4	11.9	5.1	8.7	8.8	7.1	3.3	4.8	4.8	4.9
O&M	13.7	19.1	19.1	16.6	16.8	23.8	22.7	19.2	12.7	18.2	9.5	6.9
Concentration							1					
Budget	.43	.42	.42	.45	.28	.28	.26	.35	.39	.34	.39	.35
Unit	.45	.41	.48	.50	.41	40	.36	.47	.46	.41	.44	.54
Mission emphasis	.73	.68	.56	.54	.65	.60	.63	.65	.54	.48	.56	.22

The next two variables rest on standard measures for budgetary trade-offs (see Berry and Lowery 1990). The O&M trade-off assumes a fixed-amount division, with O&M spending occurring at the expense of procurement and vice versa. The trade-off is expressed as a proportion to combined spending for O&M and "other" procurement spending of the difference between the two. ¹⁷ The R&D (RDBLNC), while not strictly a trade-off, is mathematically rooted in the trade-off formula, though squared to emphasize instances where R&D and other procurement diverge.

Finally, mission emphasis (MISEMP) is a ratio, to weapon budgets, of combined budgets for combat platforms and strategic weapons, including all strategic nuclear programs (also cruise missiles), attack aircraft (fixed-wing and helicopters), and fighters, navy fighting ships (aircraft carriers, submarines, destroyers, cruisers, and frigates), and army tracked combat vehicles (tanks and fighting vehicles). These budgets exclude allocations for systems such as cargo aircraft and ships, communication, reconnaissance, and electronic-warfare planes, trainers, landing craft,

support vehicles, precision-guided (nonballistic and "noncruise") missiles, and so on.

Describing the Indicators

Tables 1 and 2 offer time-series and cross-sectional views of the data, respectively. They reveal difference over time among the services and often greater cross-sectional differences among the services by weapon type.

Table 1 reveals the average service budgets, concentration levels, and mission emphasis for 1980–93 in four consecutive periods (in 1982 dollars). For all three services, the weapon budget rose dramatically into the mid1980s and declined thereafter, entering the 1990s near the levels of the early 1980s. And, while R&D and O&M budgets similarly rise and fall (with the exception of the army R&D budget), only "other" procurement changes as dramatically as the weapon budget; and then, the other budget remains relatively constant until the late 1980s for the air force and into the 1990s for the navy. Only for the

TABLE 2	•		•		
Average Budget Amounts,	Concentration 1	Levels, and Mission	Emphasis, FY1	980-93, by Ser	vice and
Weapon Type			- '		

	AIR I	AIR FORCE		NAVY			ARMY		
VARIABLE	PLANE	MISSILE	PLANE	MISSILE	SHIP	PLANE	MISSILE	VEHICLE	
Amounts ^a							and the second s		
Weapon	8.7	2.6	5.7	3.0	9.5	1.3	1.8	2.4	
Other	12.9	4.0	2.5	1.3	4.1	1.8	2.5	3.0	
R&D	9.2	2.8	2.4	1.3	3.9	1.1	1.7	1.7	
O&M	13.4	4.0	6.6	3.5	10.8	4.3	6.3	6.6	
Concentration									
Budget	.53	.56	.46	.47	.46	.71	.48	.69	
Unit	.54	.68	.35	.66	.45	.68	.70	.68	
Mission emphasis	.65	.55	.55	.48	.72	.69	.02	.73	

Note: Nonweapon amounts are estimates based on Amount × (Service Weapon Budget per Type/Service Weapon Budget). "Amounts are in billions of 1982 dollars.

TABLE 3					
The Effects of Program	Concentration or	Weapon	Budgets,	1981-93,	by Service

	ORDINA LEAST SQ		CORRECTED FOR AUTOCORRELATION & HETEROSCEDASTICITY	
INDEPENDENT VARIABLES	COEFF.	S.E.	COEFF.	S.E.
Amount cencentration (t - 1)	-16,448.26	12,382.23	-32,422.60*	12,711.60
Dummy	· 	· 	17,021.62*	5,552.12
Defense commitment	.80***	.13	.51**	.14
O&M trade-off	25,686.77***	5,860.95	8,328.24	5,328.05
Intercept	-32,312.70***	6,442.90	-492.30	4,908.73
Army	· 	, <u></u>	-2,398.63	1,932.05
R ^ź	.60		.72	-
F	17.33***		16.98***	
Unit concentration (t - 1)	2,030.58	10,950.42	-5,404.14	7,990.70
Dummy ` ´	· 	-	-20,175,70***	4,456.11
Defense commitment	.87***	.13	.58***	.12
O&M trade-off	28,492,24***	5,619.69	10,060,41	4,136.76
Intercept	-30,760.10**	8,056.78	-4,580.34	3,212.24
R² ˙	.58	·	.50	
F	15.96***		8.37***	

Note: The amount concentration dummy is for the navy, the unit concentration dummy, for the army. N = 39.

O&M budget does an R&D or O&M budget eventually drop below the levels of the early 1980s.

Program concentration is a different story. Unit concentration changes more dramatically than amount concentration over the entire period—for the navy sizably shifting toward concentration from the late 1980s into the 1990s, for the army similarly shifting over a longer period, into the 1990s. Regardless of measure, however, only army amount concentration does not reach its highest level by the early 1990s.

The table shows somewhat different findings for mission emphasis. The services differ in relative preference for combat platforms and strategic weapons and show differing patterns of change over time. The army, not surprisingly, is the least wedded to such systems, especially by the early nineties, when less than a quarter of army weapons were of this type. Furthermore, while the air force reduced its mission emphasis over time, the army vacillated, and the navy apparently persevered (with a fairly constant emphasis over the entire period).

Table 2 offers a cross-sectional look at the data, by service and weapon type. Weapon budgets and concentration levels vary more dramatically across types than over time. Missile budget levels are relatively small for all the services; and they are the smallest budgeted item for the air force and navy. Predictably, the largest budgets are for service-defining systems: air force aircraft, navy ships, and army combat vehicles. Yet these budgetary preferences are only somewhat reflected in concentration. In budgeted amounts, interservice concentration varies more than intraservice concentration, with the exception of the army missile budget (far less concentrated than the army plane and vehicle budgets). The services actually exhibit relatively constant amount concentration across programs, with the army displaying, on average, the highest concentration and the navy, the lowest. Per unit, the pattern is different. Missile costs are highly concentrated in single programs and are the most concentrated parts of the budget; army budgets are evenly concentrated across programs; and the navy aircraft budget is by far the least concentrated, incorporating fighters, attack planes, helicopters, antisubmarine warfare aircraft, combat support planes, and so forth. The R&D, O&M, and other procurement budget estimates show air force aircraft budgets and the navy O&M budget to be among the largest of the services. 19

The table also suggests that mission emphasis varies by service. Once more, the army boasts by far the lowest mission emphasis value. With an otherwise high mission emphasis, the army devoted an average of only 2% of the service missile budget (accounted for by the intermediate-range Pershing II missile) to strategic weapons (a distribution largely beyond army control). All three services show the greatest mission emphasis in well-budgeted servicedefining weapon categories, revealing higher unit costs and the priority given such systems.

In short, the data indicate that the analysis will be sensitive to whether budgets are treated by service or by service and weapon type and to whether concentration is studied by budgeted amounts or by unit costs. They also suggest patterns over time and interservice and intraservice differences that require statistical correctives.

 $p \le .05$. $p \le .01$.

^{***} $p \leq .001$.

TABLE 4

The Effects of Program Concentration on Weapon Budgets, 1981-93, by Service and Weapon Type

	ORDINA LEAST SQU		CORRECTED FOR AUTOCORRELATION & HETEROSCEDASTICIT		
INDEPENDENT VARIABLES	COEFF.	S.E	COEFF.	S.E.	
Amount concentration (t - 1)	-11,503.90***	1,920.98	-11,483.20***	1,452.71	
Dummy	· <u></u>	-	-215.07	1,124.25	
Defense commitment	.38***	.03	.32***	.03	
O&M trade-off	3,996.57**	1,168.36	3,488.32*	1,155.19	
Intercept	3,878.45**	1,179.38	5,127.69***	871.17	
Navy planes	· —	-	-2,530.23**	701.07	
Navy missiles	_		-1,805.76*	666.45	
Army planes	_		-2,433.25**	690.55	
Army vehicles			-3,716.62***	687.05	
R ²	.69		.78	•	
F	75.89***		42.39***		
Unit concentration (t - 1)	-3,921.07*	1,577.59	-3.013.88	1,338.48	
Dummy	-,···		-3,294.12*	1,249.18	
Defense commitment	.37***	.03	.38***	.04	
O&M trade-off	5,029.99**	1,353.01	-292.07	1,006.90	
Intercept	-200.90	1,020.70	29.14	699.13	
Navy planes			2,147.53***	466.27	
Navy missiles			1,542.97*	546.08	
Navy vehicles			6,333.02***	700.55	
R ²	.61		.81		
F	52.02***		58.53***		

Note: The amount concentration dummy is for air force aircraft, navy ships, and army missiles, the unit concentration dummy, for air force aircraft and navy ships. N = 104.

RESULTS

The analysis employs two different units of analysis and two different budgetary assumptions to provide perspective on observed relationships. The model is analyzed on service annual budgets, 1981-93, yielding 39 cases, and on service budgets per weapon type (service-weapons), yielding 104 cases, with the plausible assumption that the services discriminate by weapon type (e.g., treating aircraft or ships differently from missiles).20 The model is also analyzed using concentration in program amounts (termed amount or budget concentration) and unit costs (termed unit concentration or program variety)—the latter recognizing that amounts budgeted can obscure actual program variety. Given "foot-in-the-door" tactics, unit concentration (controlling for budgeted amounts and hence the number of weapons procured) could be a more useful measure.

The analysis requires iterative estimating techniques due to the use of time-series cross-sectional (pooled) data. With such data, autocorrelation patterns and residual variance can vary by cross section (by service or service—weapons). For this reason, after correcting for first-order autocorrelation by cross section, the data are merged to obtain full-sample equation estimates. Following Stimson (1985), the residuals are examined and a dummy variable is introduced

to generate a separate coefficient for cross sections with higher residual variance: the dummy is set to the value of concentration or the weapon budget (the independent variable) for higher-variance cross sections and zero for the others. Estimates are again obtained and binary dummies are fitted to adjust the intercept for cross-section residuals clustering above or below predicted values; and the procedure is repeated until all cross-section residuals spread evenly around the predicted amounts. ²¹ The results provide stronger support for equation 1. Yet they also support equation 2 and suggest that programmatic decisions have shaped the weapon budget.

The Weapon Budget Equation

The analysis reveals the importance of programmatic influences on the weapon budget. Yet results vary somewhat by unit of analysis (service or service—weapons) and concentration measure (program amounts or unit costs). Tables 3 and 4 show the findings for equation 1. Whether assessed by service or service—weapon, concentration amounts or unit costs, the tables strongly indicate a positive relationship between defense commitment and the weapon budget, a not entirely expected result given potential budgetary trade-offs. ²² Assessed by service—weapon for amount concentration, the O&M trade-off vari-

 $p \leq .05$.

^{••} $p \le .01$.

^{***}p ≤ .001.

TABLE 5
The Effects of Weapon Budgets on Program Concentration, 1981–93, by Service

	ORDINA LEAST SQ		CORRECTE AUTOCORREI HETEROSCED	ATION &	
VARIABLES	COEFF.	S.E.	COEFF.	S.E.	
Amount concentration					
Weapon budget (t - 1)	58**	.15	.19	.16	
Dummy			-1.22***	.12	
Mission emphasis	13,333.70	7,888.30	9,006.80	6,211.20	
R&D balance	-23,111.10	20,509.70	-28,212.50	17,246.00	
Intercept	36,743.40***	4,264.70	38,479.80***	3,150.50	
Air force		-	190.16	2,503.40	
\mathbb{R}^2	.30	****	.91	·	
F	5.02**	-	70.16***	-	
Unit concentration					
Weapon budget (t - 1)	−.57* *	.13	-1.03***	.23	
Dummy			5.31***	.78	
Mission emphasis	-9,366.10	7,098.20	6,921.30	10,933.50	
R&D balance	-55,996.70*	18,455.60	-114,118.90*	34,952.50	
Intercept	58,831.70***	3,837.50	58,013.90***	6,703.60	
Air force	———		7,163.70	3,084.10	
R ²	.49		.78	-	
F	11.00***		24.01***		

Note: The amount concentration dummy is for the navy, the unit concentration dummy, for the army. All coefficients are multiplied by 10⁵. N = 39.

able is also significant, though not in the predicted direction. Apart from the relationship between the weapon budget and defense commitment, O&M budget increases translate into weapon procurement. The Reagan years were apparently a true "time of plenty" in which defense trade-offs—between weapons and readiness or even present and future capability—were spurned. (Preliminary analyses showed little evidence of a trade-off between R&D and procurement spending.) Nevertheless, when assessed by service or service-weapons, or concentration by program amount or unit costs, prior-year program concentration significantly explains the weapon budget, a negative sign indicating that program expansion exerted upward budgetary pressure. (When assessing by service, program concentration is negatively related to budget amounts for all three services since the navy, represented by the dummy variable, exhibits a negative sign when the concentration and dummy coefficients are added.) The concentration coefficient is significant in the analysis by budgeted amount though not by unit cost; but for unit cost, the dummy variables (representing the army in the service analysis and air force aircraft and navy ships in the service-weapon analysis) are negative and significant. In all, the analysis suggests an expansionary programmatic influence on the weapon budget. (Indeed, the findings for budget concentration, on the service-weapon sample, are fairly robust and persist regardless of estimating technique, including corrections for autocorrelation alone.)²³

The Program Concentration Equation

The program concentration analysis yields more mixed results, varying by sample and concentration measure. As revealed in Table 5 and 6, program concentration is sensitive to both programmatic and budget-level influences. The analysis produced a number of insignificant coefficients. Still, in the service analysis, the R&D-procurement balance significantly predicts program expansion and, as expected, the conjoining of R&D and procurement spending leads to program proliferation (in unit concentration) rather than a spreading of budgetary resources (in amount concentration). Also, as expected, in both service-weapon estimates, mission emphasis is a significant determinant,24 associated with lower concentration in unit costs and higher concentration in program amounts. These findings reveal that in turning to mission-defining systems, the services concentrated program budgets in fewer systems (apparently the "most essential" ones: strategic bombers, aircraft carriers, etc.) even as they diversified the service arsenal.

The findings for the effects of the prior-year weapon budget are more complex. In the service analysis, the navy is shown diversifying in response to prior weapon budgets, both by programmed amounts and unit cost, while the air force is shown diversifying by unit cost. But the army pattern is different: in the service—weapon analysis, the army, too, diversified unit costs with increased budgets; yet

 $p \simeq .05$

^{***}p ≤.001.

TABLE 6

The Effects of Weapon Budgets on Program Concentration, 1981-93, by Service and Weapon Type

	ORDINA LEAST SQ		CORRECTE AUTOCORREI HETEROSCED	LATION &
VARIABLES	COEFF.	S.E.	COEFF.	S.E.
Amount concentration				
Weapon budget (t - 1)	-1.42***	.29	.07	.31
Dummy		********	68	.85
Mission emphasis	15,754.50***	3,995.20	24,617.70**	6,481.60
R&D balance	5,968.10	21,005.90	991.60	25,361.30
Intercept	52,064.50***	2,682.30	43,536.40***	4,415.80
Air force planes	-		-7,981.50*	2,811.80
Navy ships			-22,466.30***	2,828.50
R ^ž	.26	*******	.66	·
F	11.45***	-	19.03***	
Unit concentration				
Weapon budget (t - 1)	-2.13***	.39	49	.55
Dummy		***************************************	-6.24**	1.63
Mission emphasis	-6,992.80	5,467.20	-16,914.50**	4,644.00
R&D balance	-18,724.20	28,745.10	35,964.00	28,049.20
Intercept	73,389.20***	3,670.60	91,113.70***	3,979.70
Air force planes	· 	*******	-31,125.00***	5,316.80
Navy planes		-	-38,963.90***	4,953.40
Navy missiles	—	400,4000	-27,200.80***	4,387.30
Navy ships	·	Approximate	-41,037.70***	5,383.70
R ^ź	.27	avenueles.	.67	· —
F	12.42***	********	24.24***	_

Note: The amount concentration dummy is for air force missiles and army planes and vehicles; the unit concentration dummy is for army aircraft, missiles, and vehicles; all coefficients are multiplied by 10^5 . N=104, except for corrected amount concentration findings, where N=65 with army programs excluded.

in the service analysis by unit cost, it responded in the opposite way. (The unique army budgetary response can be attributed to army concentration in high-cost helicopter programs.) Still, evidence for a distributive effect of higher budgets is found in three of the four estimates, regardless of statistical method. (Neither the weapon budget coefficient nor the associated dummy are significant in the service-weapon analysis of amount concentration.)25 In all, then, the analysis of the concentration equation reveals that on program concentration, the prior-year budget exerts a selective diversifying influence, the R&D-procurement balance exerts an expansive influence (controlling for budgeted amount in the service sample), and mission emphasis exerts a contractive and expansive influence (in the service-weapon sample).

CONCLUSION

The analysis shows that programmatic influences, expressed as program contraction and expansion, significantly constrained weapon budgeting in the Reagan–Bush era: a general expansion in service programs, including big budget areas (air force aircraft and navy ships), pushed up the weapon budget even as the budget also responded to the overall

executive commitment to defense. While programs diversified, in turn, with budgetary opportunities and available technology, programming appears to have responded most to service missions (objectives): when turning to missions, the services increased program variety while concentrating program budgetary resources. The services thus moved into new areas while consolidating the service base.

Judging from budgetary outcomes, then, the services adopted an "active" stance and an "outward" orientation. The services thus conformed to the assertive image of the bureaucratic politics literature and appear to have adopted a programming strategy to claim future budgetary resources. Yet they were also mindful of the "business that they are in" and nurtured "essential" programs. And when increasing budgets for signature (service-defining) combat systems, the services tended to favor some systems over others.

Admittedly, the service personality is not fixed in stone. And the services are shown here acting under the exceptional conditions of the 1980s. In a time when money was plentiful and weapons seemed to provide their own rationale, the services, immune from criticism, were more than willing to promote their wares to secure a market. The services might act differently in times of austerity.

 $p \le .05$. $p \le .01$.

 $p \le .01.$ $p \le .001.$

The analysis, then, establishes a continuing need to account for bottom-up budgetary influences. Studies are required in order to close the gap between nuanced, qualitative treatments of bureaucratic behavior and quantitative efforts to model these effects. These studies admittedly pose statistical challenges not the least of which is to obtain useful data. But this behavior suggests no obvious alternative.

For in the end, defense budgets are neither simply creatures of strategic necessity nor bloodless responses to previous allocation decisions. They are contested and manipulated under conditions of information scarcity, uncertainty, and institutional deference that have long given the services bargaining advantages. Defense budgets are thus more intractable than suggested by either strategic or incremental perspectives. These budgets finally emerge as packages of nonchoices, with viable options hidden if not intentionally foreclosed.

In this post-Cold War transition period, the pressing question is whether the defense budget can be managed. At a minimum, a budget controlled from below creates inefficiencies, redundancies, and shortfalls. Beyond this, when open to decentralized influences, budgeting institutionalizes conflict. Self-interested bureaucracies maintain their strategies, tactics, programs, and budgets, insulated from global change.

APPENDIX

The variables and indicators are shown in Table A-1.

Notes

I wish to thank Lee Sigelman and Robert Stoker for their comments and suggestions.

1. The period boasted a postwar high ratio of procurement and R&D spending to total defense spending (United States, General Accounting Office 1992).

2. Barton Gellman, "The 'Silent Service' Breaks the Ice: Submariners Test Waters for New Role as Soviet Threat Dries Up." Washington Post, 19 April 1992, sec. A.

3. Requirements are influenced by numerous political actors wielding an effective program "veto." This point, and a discussion of the case of the army Bradley Fighting Vehicle, are found in McNaugher 1989, 125, 129-31

4. Weapon versatility is a less solid basis for analysis, because all of the high-profile weapon programs of the 1980s and 1990s were multipurpose.

5. For recent discussions of the weaknesses of the bureaucratic politics perspective, see Bendor and Hammond 1992 and Welch 1992.

6. Furthermore, bureaucratic politics implicitly downplays uncertainty and risk by promoting a heroic image of the decision maker as one able to manage complexity and uncertainty without formal, rigorous, and ongoing study and able to steer acquisition without proper oversight, risk management, learning and adaptation, or tailoring risk to actual need.

7. One attempt to answer these questions can be found in Clark 1984.

8. Because the service political strategy is future-oriented, program concentration and weapon budgets are assumed to operate through lagged effects, which do not require simultaneous equation estimates. The interdependent error terms found in simultaneous relationships complicate problems of obtaining estimates with a time-series cross-sectional design. Arguably, one problem cannot be adequately solved until the other is addressed.

9. If bureaucratic politics prevails, mission emphasis can change with budgeted amounts (and might not be strictly

Variables and Indicators					
VARIABLE	MEASURE	COMMENT			
Concentration ACONC [amount] uconc [unit]	$\sqrt{\Sigma(X_i/X)^2}$	X _i = individual program amount or unit costs; X = total program amounts or unit costs ^a			
Weapon budget (wbdgt)	total weapon budget for specific planes, missiles, ships, and/or vehicles	total amounts budgeted for all programs			
Defense commitment (DEFCOM)	total budget for procurement, R&D, O&M, excluding weapons	for analysis by service and weapon type, the budge is multiplied by service—weapon type wbbgt/service wbbgt			
O&M trade-off (OMTOFF)	$(OM - OP)/(OM + OP)^b$	for analysis by service and weapon type, the budge is multiplied by service—weapon type wbbgt/service wbbgt			
Mission emphasis (MISEMP)	budget for strategic weapons and combat platforms/weapon budget	for analysis by service and weapon type, the budg is multiplied by service—weapon type wbdgt/service wbdgt			
R&D balance (RDBLNC)	$((RD - OP)/(RD + OP))^{2b}$	for analysis by service and weapon type, the budge is multiplied by service-weapon type wbbgt/service; wbbgt higher values signify an "imbalance"			

Note: All measures are calculated either by service (air force, navy, and army) or service and weapon type (air force aircraft and missiles; navy aircraft, missiles, and ships; and army aircraft, missiles, and combat vehicles). *Unit costs = program amounts controlling for quantity procured.

endogenous in the model) but is always prior to programmatic concentration decisions

10. I recognize that capability or opportunity can instill

desire and influence objectives.

11. These data are for administration requests and are contained within the National Defense Authorization Act Reports (separately issued by the Senate Committee on Armed Services, House Committee on Armed Services, and Conference Committee) on authorizing military appropriations for the next fiscal year.

12. Program concentration measures were also based on administration budget requests rather than earlier service requests with the assumption that the services dominate programming decisions and that service preferences change during the lengthy programming and budgeting process.

- 13. Because annual outlays differ from annual authorizations and appropriations (money can be spent this year, though authorized and appropriated in a prior year), budgeted amounts were further distinguished. Budget concentration was calculated by summing (current and future) amounts authorized in a given year while unit concentration was calculated by summing (previous and current) amounts to be spent in that year (since the latter measures actual unit costs in a given year).
- 14. For unit concentration, a single weapon is assumed procured when no amount is designated, since these allocations are often associated with production start-up costs.
- 15. The weapon budget excludes the production costs and/or numbers for several classified programs. Most significant among them are the air force advanced cruise missile program and stealth attack fighter (the F-117) in the late and early 1980s, respectively. This study partly deals with this problem by testing the sensitivity of findings to the period examined. (In any case, the inclusion of these programs is likely to further enhance any relationship between program diversification and weapon budgets.)

16. Specifically designated marine procurement is omitted from the calculations, though some systems are used by the navy for marine support, and navy R&D budget includes marine systems.

17. "Other" procurement is used, in part, to avoid biased statistics when indicators calculated from weapon procurement are correlated with weapon procurement.

18. Budgets were corrected for inflation with the producer price index, with inflation rates assumed constant for fiscal

year 1991-93.

- 19. Estimates are based on the assumption that budgets are divided among service programs in proportion to the ratio of service-weapon budgets to each service's overall weapon budget. In administration requests, these budgets are not broken down programatically, as they are in the weapon budget. (While "other" procurement is distinguished by weapon type in the annual budget, it has been treated differently over the years and now largely centers in a formally designated "other" procurement budget and, for the army, also an "ammunition" budget.)
 - 20. 1980 data are introduced as a lag term.

21. To generate parsimonious estimators, dummies were first fitted for cross sections falling below predicted values and then to those falling above these values, if necessary.

22. The relationship between DEFCOM and the weapon budget is arguably made artificially high by the estimation procedure for the nonweapon budgets in the service-weapon sample, where these budgets are multiplied by the ratio of the service-weapon budget over the total service budget for weapons. Nevertheless, the same relationship is found in the service analysis (where estimates are not employed); and, if anything, the estimation procedure imposes a harsher test of significance on other model variables.

23. While the service-weapon estimates for the weapon budget equation (using unit-cost concentration) were obtained by first incorporating dummy intercepts to correct for negative residual groupings, the later correction for positive groupings yielded numerous nonsignificant dummy intercepts and a nonparsimonious equation. The nonsignificant

terms were dropped and the equation reestimated. Interestingly, parameter significance (at the .05 level) was uneffected by the dropping of the intercept terms, and the residuals obtained spread evenly around predicted values.

24. Army programs complicated the analysis, with residual variance requiring dummy parameters and intercepts that severely increased the size of the model and washed out significant parameters. They were thus removed from the analysis to produce the findings reported here—findings consistent with the full-sample result, corrected for autocorrelation alone.

25. Given the problems of estimating the concentration equation (when applied to program amounts in the serviceweapon sample) and the sensitivity of the findings to heteroscedasticity and service programs analyzed, equation estimates were derived separately for the 1981-86 period (a time of budgetary growth) and the 1987-93 period (a time of budgetary decline). In both periods, mission emphasis is again shown a highly significant source of contraction. Furthermore, the dummy variable-for air force missiles and army aircraft and vehicles—is linked with program expansion in the second period. Similar estimates for equation 1 generally confirmed the previous results, further establishing the robustness of those findings.

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REPRESSION OF HUMAN RIGHTS TO PERSONAL INTEGRITY IN THE 1980S: A GLOBAL ANALYSIS

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This crossnational study seeks to explain variations in governmental repression of human rights to personal integrity (state terrorism) in a 153-country sample during the eighties. We outline theoretical perspectives on this topic and subject them to empirical tests using a technique appropriate for our pooled cross-sectional time-series design, namely, ordinary least squares with robust standard errors and a lagged dependent variable. We find democracy and participation in civil or international war to have substantively important and statistically significant effects on repression. The effects of economic development and population size are more modest. The hypothesis linking leftist regime types to abuse of personal integrity rights receives some support. We find no reliable evidence that population growth, British cultural influence, military control, or economic growth affect levels of repression. We conclude by considering the implications of our findings for scholars and practitioners concerned with the prevention of personal integrity abuse.

In the past two decades there has been a burgeoning of information on governmental terror and the abuse of internationally recognized human rights in countries around the globe (see Cain, Claude, and Jabine 1992). While the development of theories to explain why, and to predict when, such crimes will be committed would seem to be a vital undertaking, social science scholars have only begun to use the newly developed information toward this end. To date only a few studies have attempted to construct and test theories seeking to explain variations in the levels of repression found in countries around the world (Henderson 1991, 1993; McKinlay and Cohan 1975, Mitchell and McCormick 1988; Park 1987).

We seek to build upon the strengths of existing empirical studies on this question, while improving upon them in several respects. We therefore construct a model of the most dramatic form of repression—repression of personal integrity rights—that tests several hypotheses suggested by previous studies, but goes on to test hypotheses suggested by theories not considered in previous empirical work on repression. The data set we employ clearly represents the most comprehensive yet analyzed in a global study of national human rights practices in terms of the number of countries and years it covers, as it includes relevant characteristics and behaviors for a pooled cross-sectional time-series sample of 153 countries for the eight years 1980–87.

RESEARCH ON HUMAN RIGHTS AND REPRESSION

Recent years have seen the publication of several empirical studies relating to human rights, but the tendency has been to focus on human rights practices as an independent variable, specifically as a determinant of foreign policy outputs.² The first studies that

sought to explain variations in human-rights-related phenomena apparently were conducted by McKinlay and Cohan (1975, 1976), who analyzed the general policy performance of military, as compared to civilian, regimes. For the period 1951-70, McKinlay and Cohan (1975) compare the performance of their two types of regimes with respect to a number of "political variables" relevant to human rights—the proportion of regime-years in which constitutions were declared not in full force and the proportions in which assemblies, political parties in general, or the Communist party were banned. Their statistical analyses demonstrated that poor regimes were more likely than richer ones-and military regimes more likely than civilian ones—to ban constitutions, assemblies, and parties.

McKinlay and Cohan's work was accomplished before Amnesty International, the U.S. Department of State, or Freedom House had begun to publish their assessments of human rights performance annually and with worldwide coverage. More recently, spurred by increasing public and scholarly interest in human rights issues, a few studies have appeared that seek to explain the variations in these more general assessments of regime performance in human rights. In a brief article, Park (1987) reports statistically significant positive relationships between political rights, evidently measured by inverting the Freedom House civil political rights index, and a physical quality of life index, urbanism, welfare expenditures, ethnic diversity, and percent Christian population. He reports statistically significant negative relationships for political rights with military expenditures, education expenditures, and percent Muslim population and no relationship for inequality and political rights.

Ambiguities in Park's discussion of measurement procedures and the bivariate and essentially atheoretical nature of his statistical analyses make us wary of the relationships that he found. Nevertheless, Park's research is potentially useful as a source of implied hypotheses to be tested in a more rigorous framework.

More recently Mitchell and McCormick (1988) went well beyond Park's study by framing several hypotheses to explain national violations of the integrity of the person through the use of imprisonment and of torture, assessed through their ratings of the reports issued by Amnesty International in 1984. Their analyses indicated that economic conditions, as measured by per capita gross national product (GNP), were negatively, if moderately, related to both types of repression and that experience with British colonial rule and authoritarian (as opposed to totalitarian) form of government, were significantly negatively related to the taking of political prisoners but not to the use of torture. Capitalist trade and investment (dependency) and length of independence, on the other hand, were not significantly related to either aspect of repression.

The strength of Mitchell and McCormick's analysis lies in its delineation of hypotheses that theoretically might explain, in part, variations in repression. Its major weakness, as the authors acknowledge, is that its analyses of these theoretically important relation-

ships are effectively bivariate.

The most recent additions to the literature seeking to explain variations in repression are a pair of studies by Henderson (1991, 1993). Henderson hypothesized that democracy, economic growth, and economic development would reduce—and "socioeconomic needs" and inequality would increase-levels of repression (1991, 123-27). His multiple regression model for repressive behaviors, using a measure created from the State Department's Country Reports on Human Rights Practices for 1985, confirmed his hypotheses concerning the effects of democracy (negative), inequality (positive), and economic growth (negative) on repression but found no significant relationship for level of economic development.

Henderson (1993) focuses on the impact of population variables on his governmental repression measure for 1985. Henderson theorizes that two aspects of population—density and rate of growth—have an effect on propensities to repress. He reports finding "a meaningful relationship" between population growth and political repression but fails to turn up much evidence of a similar relationship with popula-

tion density.

Though suggestive, the research we have just summarized has some limits that impede our ability to formulate an understanding of the societal determinants of respect for personal integrity and the practice of state terror. With one exception, the studies are cross-national, cross-sectional sample designs that take no account of the change that can occur within a given country.3 With two exceptions (Henderson 1991, 1993), the studies use little more than bivariate statistical methods to test hypotheses that require multivariate formulation. Also, only one study uses a more or less comprehensive sample of countries (Henderson 1993).

A final difficulty for those who wish to draw inferences from this literature is that the analysts employ four different dependent variables to measure respect for human rights or governmental coercion: formal measures of the effectiveness of representative institutions (McKinlay and Cohan 1975, 1976), a Freedom House rating (Park 1987), a rating derived from an Amnesty International report (Mitchell and McCormick 1988), and a rating derived from a State Department report (Henderson 1991,

We shall construct a model to test hypotheses suggested by contending theories of state terrorism and human rights abuse. A first necessary step toward that end is to define what we mean by these terms and to operationalize them using available measurement techniques.

DEFINING AND MEASURING HUMAN RIGHTS AND STATE TERRORISM

The problem of measuring human-rights-related concepts has received much attention in recent years. The problem of finding a consensual measure of any such concepts is likely an impossible one, due to inevitable disagreements regarding values and definitions (e.g., Van Dyke 1973). We believe, however, that researchers should carefully define the terms and describe the operationalizations adopted, in order to minimize confusion and enhance replicability.

We shall focus on the subset of human rights categorized as dealing with the "integrity of the person" (see Cingranelli and Pasquarello 1985; Henderson 1991, 1993; Mitchell and McCormick 1988; Stohl and Carleton 1985). We also refer to violations of these rights as instances of state terrorism, which, consistent with Gurr (1986), we consider to be a category of coercive activities on the part of the government designed to induce compliance in others. Examples of such activities include murder, torture, forced disappearance, and imprisonment of persons for their political views.

Our adoption of these definitions is not meant to suggest that components of broader definitions of human rights-including rights economic, political, or social in nature—are unfounded or that they are unimportant. We simply focus our current efforts on the integrity of the person because we believe governments abusing this right are committing the most egregious and severe crimes against humanity and that these violations are of the sort that can usually be avoided.4 Further, limiting the term to this category of rights allows us to separate the concept of human rights from related concepts (e.g., democracy, economic standing) that may be, or have been, linked theoretically with national propensities to respect human rights (e.g., Henderson 1991, 1993; Mitchell and McCormick 1988).

Even after we specify the subset of human rights with which we are dealing, difficulties with measurement of the concept remain to be solved. The evolving consensus among researchers is that the development of better data sources would be helpful but that sufficient data have been developed to begin the theory building and falsification enterprise, through the use of empirical tests.⁵

We have chosen to adopt the standards-based approach, as opposed to the events data approach described by Stohl and his colleagues (Lopez and Stohl 1992; Stohl et al. 1986).6 Fortunately, relatively recent developments in the collection of data on human rights offer us measurement alternatives that fit our purposes well. Two groups of researchers have applied similar sets of standards, each placing countries on five-point ordinal human rights scales, according to their reading of human rights information sources. Mitchell and McCormick (1988) created different variables to represent what they considered to be the two conceptually distinct dimensions of torture/killing and imprisonment, according to their reading of the Amnesty International Reports. Stohl and his colleagues (n.d.) have opted to include torture, imprisonment, and political killings and executions on the same ordinal scale and to create ratings by analyzing the reports of both Amnesty International and the U.S. Department of State.

While either classification would be acceptable, we have chosen to use the classificatory system employed by Stohl and his colleagues (Carleton and Stohl 1987; Stohl and Carleton 1985; Stohl et al. n.d.). We believe that it can be persuasively argued that the two dimensions postulated by Mitchell and McCormick stem, in reality, from the one dimension that Stohl and his colleagues tap—that both torture/killing and imprisonment are rooted in a regime's willingness to repress its citizens when they are considered a threat. To operationalize repression for this study, we used standards-based data (Poe and Sirirangsi 1993, 1994), as well as data generously provided by Stohl and his colleagues that appears in their unpublished work (Stohl et al. n.d.) and have been used in their previous studies (e.g., Carleton and Stohl 1987; Gibney and Stohl 1988; Stohl and Carleton 1985; Stohl, Carleton, and Johnson 1984). Following them, our data includes two five-point ordinal human rights indices generated through analyses of the contents of both the State Department and Amnesty International reports.⁷ These five-point scales of human rights abuse range from 1 for a country with a healthy record of respect for personal integrity (Canada, the United States) to 5 for a human rights disaster (Iran during several years in the 1980s). The scale and further information regarding the coding of cases are presented in the Appendix.

In coding the cases from the profiles available in the Amnesty International and State Department reports, it became evident that the samples of countries covered by those two sources were somewhat different. Amnesty International was not as comprehensive in its coverage as the State Department: Amnesty provided an average of 132 national profiles per year with sufficient information to code, while the State Department covered an average of 151. The simplest way to deal with this problem would be to conduct analyses on the cases that are available for each measure. But there is good evidence that this would bias the findings, since Amnesty tends not to produce profiles for countries that have respectable human rights records. Because of the rather high correlation between the two measures in our sample (zero-order correlation = .83), we instead chose to substitute the value coded for the State Department scale when profile information was unavailable on a country in the Amnesty International reports and vice versa (in the few cases where it was necessary) as the best available approximation of those scores.

Following the practice in several of the Stohl studies, parallel analyses are conducted with the two indicators. This provides us with a check against nonvalid findings that might arise due to biases in either indicator, an advantage not found in previous studies explaining variations in human rights behavior. Where the results gained with the two indicators are similar, we can be more confident that our findings are not due to biases in the measures but are, in fact, "real".

BUILDING A MODEL OF HUMAN RIGHTS ABUSE

Now that we have defined and operationalized the dependent phenomenon, we shall outline the theoretical perspectives on repression from which we draw the hypotheses that we shall test. Having outlined the theoretical justification for testing an hypothesis, we then specify how each of the key concepts is operationalized in our model.

Democracy

That democracy ought to decrease governmental resort to terrorism is strongly argued by Henderson: "The democratic process, with its emphasis on bargaining and compromise, offers a meaningful alternative for handling conflict if leaders choose to use it. Democracy should not be viewed as an idealistic process, but as a realistic way to accommodate demands with a minimum of conflict. . . . With a large measure of democracy, conflict should not grow so sharp as to invite repression¹¹ (1991, 123–24). Henderson goes on to note that democracy "cannot be based on pseudoparticipation. There must be legitimate channels, such as political parties and elections, that can carry interests forcefully into government" (p. 124).

The dampening effect of democracy on conflict surely does not represent the only way in which it inhibits repression. Effective democracy also provides citizens (at least those with political resources) the tools to oust potentially abusive leaders from office before they are able to become a serious threat. In addition, the freedoms that are essential to procedural democracy may make it easier for citizens and opposition leaders to publicize attempts at repression, thereby bringing down on would-be abusive leaders the weight of majority or world opinion.

While the arguments relating democracy to human rights are strong, there can be a problem of tautology when one tries to put democracy and human rights abuse into an independent-dependent variable relationship. Certain minority and procedural rights are normally regarded as elemental features of a democratic form of government. If the rights that must be respected in a democracy are defined very broadly, they may well merge imperceptibly into the respect for human dignity that is, by definition, antithetical to the use of state terrorism.11 If democracy is to function as an independent explanation for state terrorism and abuse of personal integrity, it must be defined in terms of procedures and rights that do not themselves preclude repression, even though they may represent considerable respect for human rights other than those most closely related to the integrity of the person. In addition, if we are empirically to examine the relationship between democracy and repression, democracy must be defined in terms that allow independent operationalization of the concept for the sample of nations we analyze.

For a theoretical definition of democracy, we turn to the work of Bollen, who, after surveying the definitions of a number of leading democratic theorists, defines political democracy as "the extent to which the political power of the elite is minimized and that of the nonelite is maximized" (1980, 372). Since power is no more easily operationalized than democracy, Bollen follows the lead of other theorists in noting the crucial role of political liberties and in seeing elections as mechanisms that "may increase the power of the nonelite." If they are to increase the power of the nonelite, elections must be fair, allow choice, be based on a universal franchise, and have results that are "binding on all parties": "Political liberties refer to the rights of all individuals and groups to protest or support-freely-government policies and decisions." These "provide additional political power for the nonelite, allowing them to organize opposition to the elites and their policies" and include free speech, free press, and freedom of opposition (p. 372).12

Two measurement alternatives suggest themselves as useful indicators for the concept of democracy as it has been defined. These are the measure of democracy used by Vanhanen (1990) in his studies of democratization and the Freedom House indicator of political freedom (see Gastil 1988; 1990). A third alternative, the index of "institutionalized democracy" in Gurr's Polity II data would have been an ideal candidate for use in our models (1990, 37-38); but it is not available for the year 1987, the final year covered by our data set. 13

Vanhanen's index concentrates exclusively on the electoral components of democracy: it is the percentage of the population actually voting in a given election times the difference between the percentage of the

votes won by the largest party and 100%. The index is zero for nations whose governments were not elected. Since it is multiplicative, it can also be zero or near zero for a nation that lacks party competition or has an extremely low turnout percentage. Though noninstitutional, a strength of Vanhanen's index is that, like the Gurr index, it does not depend upon the extent to which a nation respects the dignity of the person. And unlike the Polity II measure, Vanhanen's index is available for the whole period of our analysis and for 142 of our maximum of 153 countries with full data on our dependent and other independent variables. (Pooled N using the Vanhanen indicator = 1,136.) We were, however, somewhat uncomfortable with the exclusively electoral focus of Vanhanen's index.

Thus the second measure of democracy that we use is the Freedom House index of political rights. 14 Our examination of the criteria used to create the Freedom House political rights indicator convinced us that it can serve appropriately as an indicator of democracy, even if others have used Freedom in the World indicators as alternative measures of the state of human rights (Park 1987; Wesson 1987, pref.). 15 According to a recent volume of Freedom in the World, the Freedom House index of political rights assesses

whether the people have "the right to vote and compete for public office";

whether elected representatives "have a decisive vote on public policies";

whether "the people have a choice in determining the nature of the system and of its leaders";

whether chief executives and legislative representatives "are elected through free and fair elections"; whether "there are fair electoral laws, equal campaigning opportunities, fair polling and honest tabulation of ballots";

whether "voters are able to endow their leaders with real power, or whether unelected elements reduce

or supersede this power';

whether the system allows "the people to organize in different political parties or other competitive political groupings of their choice" and is "open to the rise and fall of these groups";

whether citizens are free "from domination by the military, foreign powers, totalitarian parties, religious hierarchies, economic oligarchies or any

other powerful group"; and

whether "cultural, ethnic, religious and other minority groups have reasonable self-determination, selfgovernment, autonomy or participation through informal consensus in the decision-making process" (McColm 1990, 19-20).16

This measure is by no means ideal for our purposes. The political liberties ratings supplied by Freedom House do not include specific assessments of the freedoms of speech and press that are included in Bollen's index of democracy. ¹⁷ The indication that the political rights measure includes "freedom from domination" by unelected groups also raises the possibility that it might slightly overlap with our

measures of abuse of personal integrity. ¹⁸ Finally, the Freedom House indicators have been criticized as "highly impressionistic, being no more than an estimate by a person who has collected a lot of seemingly relevant information on all the countries of the world" (McCamant 1981, 132.)

Despite these criticisms, we use the Freedom House political rights index as one of our two primary measures of democracy in the analyses that follow, for several reasons. First, precision in the statement of the Freedom House measurement criteria has improved over the years so that criticisms based on measurements for the 1970s are less persuasive for the 1980s. Second, the Freedom House indicators have been used successfully in a number of studies of freedom-related concepts (see Poe 1991, 1992; Rummell 1983; Stohl, Carleton, and Johnson 1984). Third, the Freedom House indicator of political rights is available for the largest number of countries (153) for which we have human rights measures for the years 1980-87, thus allowing us to conduct the most extensive possible analysis. Fourth, recent research by Bollen has found the Freedom House political rights indicator to have the highest validity rating among the several he analyzed, suggesting that if one is to use a single indicator, the political rights measure is certainly a good one (1993, 1225). Finally, and most important, our extensive statistical analyses of the three available measures of democracy indicate that they are so highly correlated as to be virtually substitutable and that there are few substantively interesting differences in the pooled cross-sectional timeseries models of repression one might compute using the three indices of democracy. The seven-point ordinal scale is inverted in our analyses, so that more democratic countries achieve larger scores.

In sum, we believe the Freedom House indicator is a useful measure of democracy. Since we are employing this measure with the Vanhanen index, in parallel analyses, it is unlikely that any of its weaknesses will lead us to invalid findings. Democracy as we have now defined it is, in concept, substantially independent of human rights abuse. Furthermore, the results of preliminary analyses indicate that the two concepts are relatively independent empirically.²⁰

Population Size and Growth

Henderson develops a detailed and persuasive argument that population size and growth increase regime tendencies to use repression: "Growth in numbers of people can create scarcity—a short-fall between what people need and want and what they have. Under this pressure governments may be pushed in an authoritarian direction. . . . What is worse, government may resort to repression as a coping mechanism" (1993, 8).

A large population may increase the occurrence of state terrorism in at least two ways. First, a large number of people increases the number of occasions on which such coercive acts can occur. As a matter of simple probability, such an increase should lead to the occurrence of more instances of coercion. Second, a large population places stress on national resources and bring the threat of environmental deterioration, further reducing available resources (ibid).

Arguments made by Henderson indicate that rapid population growth may also promote resource stress, perhaps even more than population size: "The extent of scarcity varies from country to country, but in the more hard-pressed countries, burgeoning demands will keep governments off-balance and will incline them to resort to repression. Growing populations absorb any economic growth rate that may occur, thus frustrating governments' efforts" (1993, 4). Rapid population growth also increases the proportion of the population falling into the youngest age categories that require the constant creation of new jobs, new housing, and many other government services, as well as posing the greatest tendency to engage in criminal activities and other threats to public order (ibid. and the sources cited therein).

To operationalize the effects of population on the abuse of personal integrity through state terrorism, we employ two variables: the natural logarithm²¹ of total national population and the average percent increase in national population from one year to the next, over the eight-year period of the study.²²

Level of Development and Economic Growth

Despite the use of different indicators for both independent and dependent variables, the research we have summarized uniformly found a negative relationship between level of economic development or wealth and repression (Henderson 1991; McKinlay and Cohan 1975, 1976; Mitchell and McCormick 1988; Park 1987). The logic underlying these results appears straightforward: "The poorest countries, with substantial social and political tensions created by economic scarcity, could be most unstable and thus most apt to use repression in order to maintain control" (Mitchell and McCormick 1988, 478). Again, "It is only logical to think that, with a higher level of development, people will be more satisfied and, hence, less repression will be needed by the elites" (Henderson 1991, 1226). The only caveat to these musings comes from Mitchell and McCormick's interpretation of Huntington's argument concerning political instability suggesting that the use of coercive measures might rather be expected to be most serious in states in the middle, transitional levels of development, an argument for which they find little empirical evidence (Mitchell and McCormick 1988, 479, 488-90).

The presumed effects of economic growth are more problematic. On the one hand, it is logical to assume that rapid economic growth, since it expands the resource base, should reduce the economic and social stresses that lead governments to use terrorism as a policy tool. But there has also been a strong argument that rapid economic growth is most likely to be a destabilizing force that will, in fact, increase instability and a regime's temptation to resort to coercive

means to maintain control. (Olson 1963 is the seminal work.) The destabilizing effect of rapid growth may occur because it can rarely be rapid enough to outstrip the growth in expectations that is simultaneously occurring (Gurr 1970, 1986); because it increases the number of declassé individuals and groups most prone to promote instability (Olson 1963); or because rapid growth inevitably occurs unevenly, possibly even creating growth by decreasing the well-being of the nonelite. In any case, frequently sharpening class differences within the population cause the elite to promote repression to keep the nonelite quiet (Henderson 1991, 126 and the sources cited therein).

We thus expect abuse of the right to personal integrity to be decreased by level of economic development but increased by economic growth. When we turn to operationalizing economic development and growth, we find some controversy over how these variables ought to be measured. Dissatisfaction with the defects of the most traditional of indicatorsgross national product (GNP) per capita—has led to the development of several alternative measures of economic standing.²³ The major problem with these alternatives is that they do not exist-and cannot easily be created—for most of the years, or for a significant number of the countries, of our study. As a result we follow Mitchell and McCormick (1988) and McKinlay and Cohan (1975, 1976) in using GNP per capita and percentage growth in GNP per capita as our measures of economic development and economic growth. Despite the deficiencies in these measures, we have little reason to believe they produce inaccurate or misleading results compared to those achievable with other currently unavailable alternatives.

Leftist Regimes

The argument of former U.S. ambassador to the United Nations Jeane Kirkpatrick (1979) that "totalitarian" (i.e., noncompetitive, Marxist or Marxist-Leninist) regimes were the world's most repressive is reviewed and analyzed by Mitchell and McCormick (1988, 480–81, 493–95). They find that such regimes are, in fact, more repressive on at least one of their dimensions of repression than are "authoritarian" regimes of a non-Marxist nature. Such a finding is not surprising if one takes seriously the tenets of Marxist-Leninist theory about the need for a dictatorship of the proletariat, and some other communist doctrine.

Even so, critics of U.S. foreign policy have taken Kirkpatrick and the State Department to task for what the critics see as their efforts unfairly to paint all socialist regimes with the tar of repression. We test the hypothesis that leftist regimes will be more coercive than other regimes, where leftist regimes are defined quite precisely as those governed by a socialist party or coalition that does not allow effective electoral competition with nonsocialist opposition.

Military Regimes

That we expect military regimes to be more coercive than others probably surprises no one, whether they are familiar with McKinlay and Cohan's findings or not. Military juntas are based on force, and force is the key to coercion. Yet, in many of the nations in which soldiers forcibly take power, they do so alleging that the leaders they are replacing were themselves violating the constitution and, possibly, engaging in repression of the rights of the citizens. In addition, since military rule is by definition antithetical to democracy, it might be that any apparent relationship between military rule and state terrorism is spurious, a result of a failure to control for the democratic/nondemocratic nature of the regime.

For our operationalization of military-controlled regimes, we use a classification created under our direction by Madani (1992) from the Europa Yearbook. Following McKinlay and Cohan, the classification defined military regimes as those which had come to power "as a consequence of a successful coup d'etat, led by the army, navy or air force, that remained in power with a military person as the chief executive, for at least six months in a given year" (p. 61; see McKinlay and Cohen 1975, 1). The category military regimes also included a small number of mixed regimes "with either a civilian as the chief executive and several military persons in the cabinet or a military head of government who nominated a civilian as the head of government and himself worked behind the scenes" (Madani 1992, 61). All other governments were considered civilian regimes.²⁴

British Cultural Influence

Mitchell and McCormick argue that the political cultures of nations may have an important impact on the extent to which their governments engage in repression and that "one important factor that is thought to have shaped political culture for most states is the colonial experience" (1988, 479–80). Specifically, they note: "British colonial rule . . . is commonly thought to be strongly associated with the postcolonial development of democracy. The British legacy may be a relatively greater respect for human rights. By contrast, other colonial experiences (Spanish, for instance) are generally assume to have introduced a greater degree of hierarchy and authoritarianism. The legacy here may well involve higher levels of human rights violations" (p. 480).²⁵

This is certainly not the place to review the efficacy of political cultural explanations of national political behavior. At their best, they connote that certain attitudes inculcated by the culture, but not directly measured, are partially responsible for differences in the dependent behaviors of interest. That is the direct implication of Mitchell and McCormick's argument, and, following their lead, we include "British cultural influence" in our models of state terrorism. British influence is therefore represented in our model by a variable coded 1 for countries that had been territo-

ries of Great Britain at some point during their histories, which all other countries coded 0.

International War Experience

In their study of economic summitry, Putnam and Bayne (1987) draw on game theory to illustrate that when regime leaders ascend to power, they face the prospect of playing simultaneously in two distinct but nevertheless interrelated games: one is played in the domestic political arena, with the primary purpose is to keep power, whether through election or the use of terror; the other takes place in the international realm, with other major players being the leaders of other countries, their representatives, and relevant international governmental and nongovernmental organizations. We believe that this analogy is also useful to researchers interested in explaining human rights abuse. The fact that these two games are intimately intertwined suggests that leaders' actions in the domestic political realm will likely be affected when their nations are a direct participant in an international crisis situation. In fact, studies of what Stohl (1980) has called the "nexus of civil and international conflict" abound, and the results of some systematic empirical studies (e.g., Stohl 1975, 1976) tend to point to the conclusion that there is a positive linkage between participation in international war and the levels of domestic political violence in participant countries. In the only study we know that deals directly with the question of whether participation in international war affects political repression, Rasler (1986) focused on the twentiethcentury United States and found evidence that administrations did indeed increase levels of repression during wars.

We hypothesize that such a relationship applies generally in our worldwide sample. As a guide to our coding of the concept war during the 1980s we used as a guideline the criteria developed by Small and Singer (1982). These researchers coded a country as having been a participant in an interstate war when (1) there was a total of a thousand or more battle deaths suffered by all of the participants in the conflict, (2) the particular country suffered at least a hundred fatalities or had a thousand or more personnel taking part in the hostilities (pp. 50, 55). We ignored a further criterion employed by the Small and Singer study—that a participant country should be a member of the "international system" (p. 51)—because it did not fit well with the purposes of this cross-national study, which includes countries regardless of whether they are members of the international system as defined by Small and Singer.

Civil War Experience

Just as governments may employ repression when threatened on the international front, it is also a tool commonly used by governments that are faced with internal problems (see Nieburg 1969; Skocpol 1979; Tilly 1978). The most serious of threats in the domestic arena is posed by a condition of civil war, in which the authority of the regime in power is being challenged by an armed and organized resistance controlled by a shadow government. Therefore we shall propose a hypothesis that to our surprise had not yet been tested in quantitative studies of human rights and state terrorism—that regimes are more coercive when they are involved in civil conflict.²⁶

In order to operationalize the concept of civil war we again look to the criteria developed in conjunction with the Small and Singer (1982) study for guidance. First, the government, as the central authority in a country, must be involved as a direct participant in the war. Second, there must be an effective resistance, that is, either both sides must be "organized for violent conflict" or "the weaker side, although initially unprepared [must be] able to inflict upon the stronger opponents at least five percent of the number of fatalities it sustains" (p. 215).²⁷ Thus genocides and massacres are not considered to be civil wars, and this concept is kept distinct from our dependent variable.

TESTING A MODEL OF ABUSE OF PERSONAL INTEGRITY RIGHTS

In recent years studies employing the pooled crosssectional time-series (PCT) design have appeared much more frequently in political science journals. The PCT design is especially appealing because it enables researchers to test theories over both space and time simultaneously and thus to witness the interplay of two dimensions usually viewed separately in either cross-sectional or time-series studies (Clarke 1992; Sayrs 1989; Stimson 1985). With this notable advantage, however, comes some statistical difficulties. Specifically, autocorrelation and heteroscedasticity often complicate efforts to apply ordinary least squares (OLS) regression, probably the discipline's most widely used and understood statistical technique, on PCT data. Both autocorrelation (also known as serial correlation) and heteroscedasticity may lead to inaccurate estimates of the standard errors of parameter estimates, thus calling into question the results of significance tests (Ostrom 1990; Stimson 1985)

Many PCT studies have used what Beck and Katz (1994) call the feasible generalized least squares method to deal with these problems. This is essentially generalized least squares (GLS) as it is usually practiced, where an estimate of the error process is generated to evade or overlook the assumption underlying GLS, that the error process is known. But Monte Carlo trials undertaken by Beck and Katz show that there is good reason to be concerned about violating this assumption, as FGLS is shown consistently to underestimate standard errors (1994, 15). As a result this method sometimes yields extremely optimistic estimates of statistical significance. We are therefore persuaded by the recommendations of Beck

and his associates (Beck and Katz 1994; Beck et al. 1993) who favor the use of OLS regression, in tandem with a variation on White's (1980) robust-standard-errors technique that Beck and Katz developed for use on panel data, to control for heteroscedasticity. The White technique provides a consistent or "robust" estimate of the standard error by estimating a parameter covariance matrix that is consistent in the presence of heteroscedasticity. In order to control the effects of autocorrelation that we found to be a problem in our preliminary analyses, we shall employ a lagged dependent variable in the model, along with the substantive variables already outlined.²⁹

To review, our general model is as follows:

Personal Integrity Abuse $_{tj} = a$

- + B_1 Personal Integrity Abuse_{(t-1)j} + β_2 Democracy_{tj}
- + β₃Population Size_{ti} + β₄Population Change_{ti}
- + β₅Economic Standing_{ti} + β₆Economic Growth_{ti}
- + β_7 Leftist Government_{ti} + β_8 Military Control_{ti}
- + β_9 British Cultural Influence
- + β_{10} International War_{ti} + β_{11} Civil War_{ti} + e_{tj}.

Data generated from Amnesty International and U.S. State Department human rights profiles will be used to construct two different measures of the level of personal integrity abuse, and parallel analyses will be conducted on these measures. Further, we shall conduct tests with the Vanhanen and the Freedom House political rights measures of democracy with both of these dependent variables, so that in all, four models will be tested. Though we gathered data for the 1980–87 period, the addition of the lagged dependent variable meant that we had to discard 1980 from our final analyses.³⁰

Modeling Personal Integrity Ratings Derived from Amnesty International Reports

Our first set of analyses seeks to explain variations in the repression scale generated from the Amnesty International reports, using the statistical techniques described. Under model 1 and model 2 of Table 1 we present the coefficients and standard errors obtained in these tests. The statistics for model 1 are those yielded when the Freedom House political rights rating was used to operationalize the concept of democracy. Those for model 2 are the results when we substituted the Vanhanen democracy measure for that of Freedom House.

The strongest predictors of the repression scores operationalized from the Amnesty International reports are, as might be expected, the lagged dependent variables that achieved large, statistically significant coefficients in both analyses. The inclusion of the lagged dependent variable on the right side of the equation represents our means of correcting for the serial correlation present in our time-series data.

Nevertheless, the substantive meanings of the strong coefficients for the lagged endogenous variable are worth considering. Such coefficients mean that our individual country scores on state terrorism are strongly seated characteristics of their political systems that do not change easily or rapidly. In addition, these coefficients provide a mechanism through which the effects of changes in the independent variables at a given time (time_t) continue to influence the dependent variables beyond the time of the occurrence of the change. We shall use this characteristic to illustrate the dynamic effects of our independent variables on repression of personal integrity rights.

Once the effects of autocorrelation are controlled by incorporating these lagged variables, 31 several others are shown to be important determinants of levels of state terrorism. Our results indicate that level of democracy has a strong and statistically significant impact on governments' respect for personal integrity. Both the Freedom House and Vanhanen measures of democracy prove to be statistically significant determinants of national propensities to use repression. The smaller coefficient of the Vanhanen variable should not, however, be interpreted as being evidence that this variable has a smaller effect, since that variable is measured on a scale ranging from 0 to 46.4, as compared to the Freedom House variable which ranges from 1 to 7. In fact, when one considers the different scales of these two indicators, the coefficient of the Vanhanen scale is somewhat stronger than that of the Freedom House variable. We find that if a country were to achieve the least democratic score on the Vanhanen measure after having achieved the most democratic score in the previous time period, this would make a difference of about .4 in the dependent variable at time₁. While the initial impact is moderate, we can see after performing some simple calculations, considering the dynamic effect mediated by the lagged dependent variable, that the impact of a loss of democracy increases substantially over time. 32 Figure 1 shows that if the abandonment of the democratic process in our example country were to continue, other factors held equal, the human rights index would have increased by about 1.3 (on the five-point state repression scale) after five years, (i.e., at time $_{t+5}$) as a result of the lagged effect, stabilizing at 1.4 points greater than the original score a few lags thereafter. The movement from most democratic to least democratic on the Freedom House measure is associated with a .26 increase in the Amnesty index, at time, stabilizing at about a one-point rise in that index after several lags.

Relating these effects back to the scale of the dependent variable, if a democratic country with a near perfect human rights record were suddenly to abandon the democratic process, we would expect that the country would, at the very least, begin to hold some political prisoners, and that political brutality, executions, and murders might become a common feature of life, other relevant factors being equal. Is such a complete abandonment of democracy an

TABLE 1 Pooled Cross-sectional Tests of Explanations of Abuse of Personal Integrity Rights

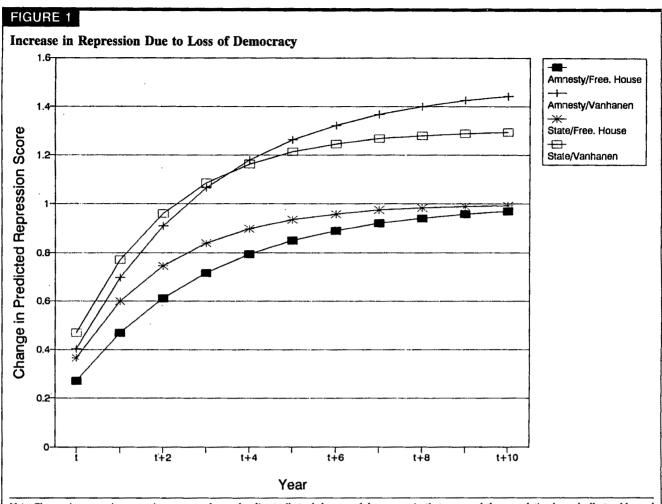
INDEPENDENT VARIABLES	AMNESTY INTERNATIONAL		STATE DEPARTMENT	
	DEMOCRACY M FREEDOM HOUSE (MODEL 1)	MEASURE: VANHANEN (MODEL 2)	DEMOCRACY M FREEDOM HOUSE (MODEL 3)	IEASURE: VANHANEN (MODEL 4)
Constant	02	.07	.11	.23
	(.14)	(.12)	(.16)	(.18)
Personal integrity abuse _{t-1}	.73**	.73**	.63**	.64**
	(.08)	(.08)	(.10)	(.10)
Democracy	05**	009**	06**	01**
	(.02)	(.003)	(.02)	(.003)
Population size	.05**	.04**	.06**	.04**
	(.02)	(.02)	(.02)	(.02)
Population change	.008	004	.01	002
	(.009)	(.01)	(.02)	(.01)
Economic standing	008*	007	02**	01**
	(.004)	(.004)	(.004)	(.004)
Economic growth	0009	0005	.0003	.00007
	(.001)	(.001)	(.001)	(.001)
Leftist government	04	03	.08*	.11**
	(.07)	(.07)	(.04)	(.04)
Military control	.05	.05	.04	.06
	(.05)	(.0 6)	(.05)	(.06)
British cultural influence	03	05	01	04
	(.05)	(.06)	(.04)	(.04)
International war	.21**	.22**	.34**	.35**
	(.07)	(.07)	(.0 9)	(.01)
Civil war	.33**	.33**	.49* *	.49**
	(.11)	(.11)	(.12)	(.12)
R ²	.77	.77	.75	.75
N	1071	994	1071	994
Average contemporaneous Correlation of Errors	.43	.43	.45	.43

Note: Main entries are unstandardized OLS coefficients, generated using RATS 386 version 4.02. A lagged dependent variable is included in each model to control the effects of autocorrelation. The robust standard errors, which were used to control heteroscedasticity (Beck et al. 1993; White 1980), are in parentheses.

unlikely scenario? Perhaps, but there are precedents for such quick and drastic changes in governmental institutions. Though it falls outside the period of our analysis, the example of the downfall of Chilean democracy with the overthrow of Salvador Allende and the installation of the extremely authoritarian Pinochet regime represents exactly this phenomenon. In 1973, Chile achieved the maximum democracy score on the Freedom House measure of democracy, while in 1975, the next year for which the Freedom House measure is available, it achieved the minimum democratic score!³³ The collapse of Weimar democracy with its catastrophic aftermath might well be another concrete example of such a drastic change.

With regard to population-related determinants of human rights abuse, the findings are mixed. In both sets of statistical analyses shown in the Table 1, the relation between population size (logged) and the repression measure derived from Amnesty International reports indicates that the larger a country's population, ceteris paribus, the greater the state's tendency to violate the integrity of its citizens through the use of repression and terror. Judgments regarding the substantive importance of the size of the coefficient would, however, vary depending on one's viewpoint. These findings are at least moderately important when viewed from the perspective of theory. One who emigrated from a country with a tiny population of about 200,000 (e.g., the Bahamas during the eighties) to one the size of the People's Republic of China would likely find, ceteris paribus, that levels of repression were much higher, since the lagged effects of population become asymptotic at around 1.4 and 1.7 in the analyses with the Freedom House and Vanhanen measures of democracy, respectively.³⁴

^{*}p < .05 (one-tailed test). **p < .01 (one-tailed test).



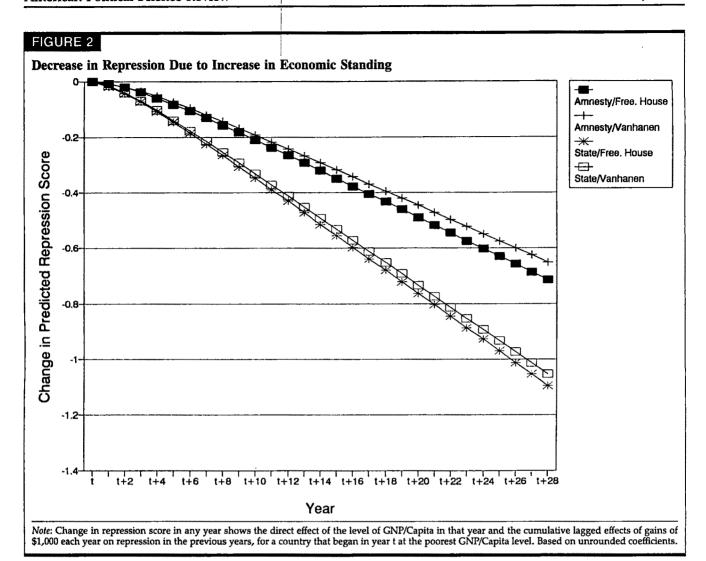
Note: Change in repression score in any year shows the direct effect of absence of democracy in that year and the cumulative lagged effects of loss of democracy on repression in the previous years, for a country that in year t changed from the most to least democratic score on the respective (Freedom House or Vanhanen) democracy index. Based on unrounded coefficients.

In contrast, the sizes of the coefficients are probably rather unimportant from the perspective of those seeking to conduct policy-relevant research, who would probably focus on changes in the dependent variable that result from achievable changes in the values of manipulable independent variables. When we examine the differences in repression that result from the addition or subtraction of population from countries of various sizes, the impacts seem rather small. For example, the impact of adding one million people to a population the size of China's in year t would result in an increase of less than .0002 in the repression index at time_{t + 10}, in both sets of analyses. In the unlikely event that one million people were added to a country about the size of the Bahamas, certeris paribus, the increase in the repression index would only be around .3 at time $_{\rm t}$ $_{+}$ $_{10}$, regardless of which measure of democracy we might use.

We find no support for the conclusion reached by Henderson (1993) that rapid rates of increase in population lead to political repression. In the set of analyses utilizing the Freedom House democracy variable (model 1), a weak and statistically insignificant positive coefficient is the result, while in the

second set of analyses, using the Vanhanen democracy variable (model 2), the coefficient is weak, statistically insignificant, and negative.

The results yielded by these analyses are also mixed with regard to the effects of economic variables on national propensities toward the violation of personal integrity rights. The hypothesis linking economic growth rates to repression is not supported by the results of either model 1 or model 2. Our findings do indicate that economic standing, as measured by per capita GNP, has a statistically significant effect on such violations in model 1 and falls just short of statistical significance in model 2. But here an examination of the magnitude of the coefficient leads us to the conclusion that the effect is rather weak, since the coefficients indicate that a relatively unlikely increase of ten thousand dollars in a country's per capita GNP from one year to the next would translate into a rather small decrease in the tendency for abuse of personal integrity of between .07 and .08 on the five-point human rights abuse index, ceteris paribus. If that country's per capita GNP remained stable at ten thousand dollars above the original value, this



would translate to a modest decrease of .24 or .22 in the repression scale at time $_{\rm t+5}$.

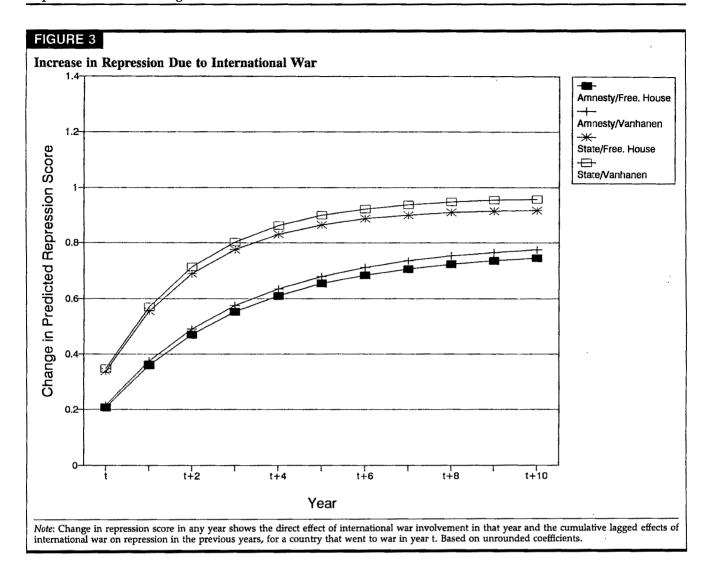
If one optimistically assumes for the world's poorest country at time, a GNP per capita increase of one thousand dollars per year for a period of time long enough to make the income of that poorest country equal that of the richest country at time, the projections are those represented in Figure 2. They show that after the 28 years this change would require, the decrease in repression due to such an impressive advance in economic standing would be expected to range from about .7 or .8, depending upon the measure of democracy used, with the personal integrity abuse index derived from Amnesty International reports. These truly long-term effects would represent important decreases in repression, in the unlikely event that a nation could sustain such increases in economic development. Nevertheless, they are much less impressive when compared with the shortterm effects of the loss of democracy or the onset of civil or international war.

The variables identifying particular types of regimes thought to have an important impact on levels of repression do not fare too well in these analyses.

While the military control variable has coefficients in the expected direction, they are statistically insignificant. And the variable identifying leftist regimes achieves a statistically insignificant coefficient in an unexpected direction.

In contrast, the dummy variables identifying levels of external and internal threat, in the form of international and civil wars, do achieve substantively important and statistically significant coefficients, indicating that as threat increases, so does a country's propensity to use repression. The variable identifying participants in ongoing international wars achieves statistically significant coefficients of .21 and .22 in models 1 and 2, respectively. These coefficients indicate that if a country were to be involved in international war for six consecutive years, we would expect an increase of about .7 in the human rights index, other factors held constant, in the sixth year (see Figure 3).

The measure of national involvement in civil war achieves a coefficient of .33, statistically significant at the .01 level, in each analysis. Once again assuming a country's ongoing involvement in civil war, its repression scale score would have increased by just



over one point in the sixth year of that war, with the impact stabilizing at about 1.2 a few lags thereafter (see Figure 4). Thus the results we obtained when using data derived from Amnesty International reports to operationalize human rights abuse strongly support the conclusion that regimes repress political opponents when faced with threats, domestically and abroad, in the form of civil and international wars.

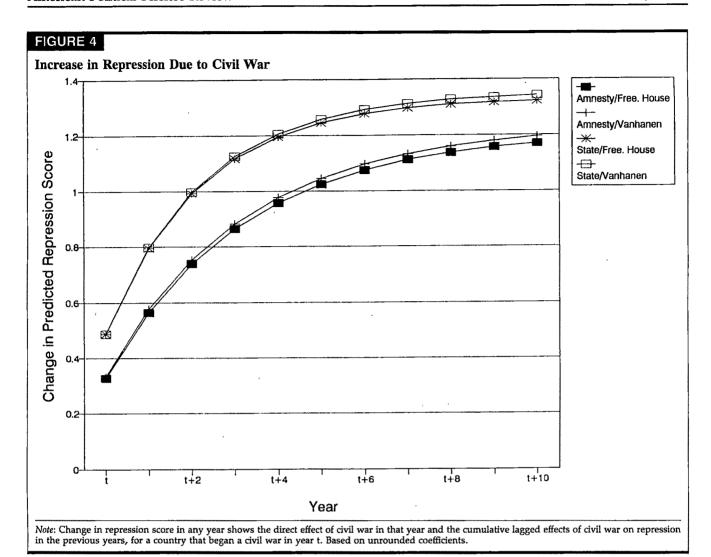
The R-squared of .77 yielded by both models 1 and 2 shows that the theoretical model achieves a quite respectable level of explanatory power. Thus while the possibility of misleading results emerging as a result of specification error is not precluded, we believe there is good reason for confidence in these findings.

A Second Model: Ratings Derived from State Department Reports

In order to check the reliability of the results obtained in our first set of analyses, we next conducted tests of similar multivariate models with an index measuring the abuse of personal integrity derived from the U.S. State Department human rights profiles. The results of these analyses are presented in Table 1: Model 3 uses the Freedom House measure of democracy, and model 4 uses Vanhanen's operationalization of that concept. The R-squared of .75 again indicate that the models achieve a respectable level of explanatory power.

The results in models 3 and 4 are very similar to those yielded when the focus was on the dependent variable generated from Amnesty International reports. The most serious divergence between this and the first set of results is that relating to the leftist government variable, which achieves strong, statistically significant coefficients in the expected direction. In our previous analyses, focusing on an index derived from Amnesty International reports, the coefficients had been extremely weak, with a negative sign. These results are precisely what one would expect if indeed the State Department's profiles are biased against leftist governments (or, alternatively, if Amnesty International's ratings are biased in favor of leftist regimes and movements).

The list of variables exhibiting statistically significant effects is otherwise about the same as in our tests of models 1 and 2. The variables identifying British



cultural influence, and military regimes again have weak and statistically insignificant coefficients. The same can be said of the population and economic growth variables, which also failed to exhibit important effects in our initial tests. But the impacts of many of the variables found to be important in models 1 and 2 appear to be somewhat larger in this set of analyses. Economic standing, democracy, international war, and civil war each have somewhat larger effects in these models, while these of the population variable were larger in one of the two sets of analyses, and identical in the other.

A Comparative Look at Dynamic Effects

Now that we have conducted four sets of analyses, we are in a better position to assess the importance of the variables we hypothesized would affect levels of repression. With regard to the variables found to have statistically significant effects in each of the four sets of analyses, it is evident that population size, economic standing or development, international war, civil war, and democracy affect levels of repression. In Figure 1 we show the impact that the abandonment of democracy would have on personal in-

tegrity abuse, assuming a change from the most democratic score at time, stabilizing at the least democratic score at time, stabilizing at the least democratic score throughout the period. While the estimates provided by the Freedom House variable are somewhat more cautious, it is clear, nevertheless, that the loss of democracy increases the abuse of personal integrity, especially when one considers its lagged effect.

The same can be said of international and civil wars, whose effects over time are plotted in Figures 3 and 4. Put in terms of the scale of the dependent variable, the results plotted in these figures show that we should expect that a country under the rule of law at the time of initiation of either an international or civil war would likely have limited political imprisonment by the end of the period, other factors held equal. A regime that already had limited political imprisonment but few more serious violations, would be expected, ceteris paribus, to hold a more extensive number of political prisoners and perhaps to torture, execute, or murder its political opponents if those wars continue.

By way of contrast, even the strongest plausible projections based on the effects of economic standing and population size seem weak when compared to

the effects of the democracy and international and civil war. Generously plausible assumptions about changes in population size yield at best only modest increases in predicted abuse of personal integrity right, ceteris paribus, when projected over time. Similarly, the most optimistic estimate of economic standing's effect is that an increase of ten thousand dollars in per capita GNP, held constant for several years thereafter, years would decrease state terrorism by .15 at time, by .39, at time, ± 5 , stabilizing at .42 a few lags thereafter. So even though these analyses do show that economic development makes a statistically significant difference in propensities to use state terror, the size of that difference is rather small. On the other hand, given the long research tradition that has found strong correlations between economic development and democracy,35 it is notable that both economic development and democracy have statistically significant effects on repression of personal integrity rights, regardless of the magnitudes of their impacts.

SUMMARY AND CONCLUSIONS

In this study we sought to explain variations in cross-national respect for the subset of human rights known as personal integrity rights, through the use of sophisticated multivariate methods, on a comprehensive pooled cross-sectional data set covering the years 1980-87. A model explaining variations in national respect for these rights was constructed to test several hypotheses drawn from a variety of theoretical perspectives on why such human rights violations might occur. Two distinct dependent variables, derived from information on human rights conditions provided by Amnesty International and the U.S. Department, and two indicators of the key concept of democracy were used in four sets of analyses. Because the tenor of the findings yielded by the four models is very similar, we can conclude with some confidence that the effects we isolated are in fact an accurate representation of reality.

Several of our hypotheses were supported in each of the four sets of analyses that we presented. As expected, democracy was shown to be associated with a decreased incidence in repression, regardless of which of the two indicators of that concept—and which of the two dependent variables—was employed. These results are important because they substantially extend the findings of Henderson (1991, 1993) with different measurements of democracy. As a result of the cumulation of findings on this linkage, then, it would now seem difficult to deny that democratization decreases governments' use of coercion to abuse the human rights of private citizens.

Following Henderson (1993), two population variable were employed in our multivariate model of human rights abuse. With regard to the logged population size variable, the results were stable and quite clear, and we can therefore conclude that population size does have a positive impact on human rights

abuse, with more populated countries having a greater propensity to abuse personal integrity rights, ceteris paribus.

Our findings regarding several other variables thought to be related to state terror were negative. None of our results supported the conclusion that military control or British cultural influence affects levels of repression.³⁶ Results for the effects of population growth were stable across all of the analyses that were conducted, indicating no statistically significant or substantively important impact on repression. This finding contradicts the conclusions of Henderson (1993), which he based on his analysis of a cross-sectional data set covering the year of 1985. However, here we should note that we dealt with only a seven-year time frame and that the population increase variable utilized in this study was an average of the population gain over this seven-year period, so that unlike other variables in the model (with the exception of British cultural influence), it did not vary across time. A study using accurate yearly estimates (if such estimates are possible) conducted over a longer time frame might yield more positive results.

Rather mixed results were yielded by the variable identifying leftist governments. Leftist regimes appeared to violate personal integrity rights more seriously than others when such rights were operationalized using information gathered from U.S. State Department sources. When our analysis focused on the measure derived from Amnesty International information, however, no such relationship appeared. Here we are inclined to give less weight to the results obtained with the index derived from State Department reports, because our findings are consistent with those that would be expected if allegations regarding the biases of the State Department reports against (or of Amnesty International in favor of) some leftist regimes are valid (e.g., Innes 1992). Thus we cannot conclude that, ceteris paribus, doctrinaire socialism is a cause of coercion that abuses human rights; but the results do perhaps provide limited empirical evidence of the different "pictures" of human rights realities painted by these two sources of human rights behavior during the 1980s.

Our study does provide very firm support for the hypotheses linking national experience of international and domestic threats, in the form of international and civil wars, to an increased tendency to abuse personal integrity rights. Variables identifying countries that were participants in both kinds of wars were found to have statistically significant and substantively important impacts on national respect for the personal integrity of citizens in each of the four sets of analyses, with civil war participation having a somewhat larger impact than participation in international war. Thus this study is the first to document the impact of involvement in violent conflicts, in both the international and domestic arenas, on levels of repression around the world.

Finally, with regard to economic explanations of human rights abuse, we found in each of our four sets of analyses that economic standing is negatively, but only rather weakly, related to regimes' propensities to abuse of personal integrity rights.

Having now nearly finished this investigation of the conditions that lead to the most serious forms of human rights abuse, we believe we would be remiss if we failed to consider the vitally important question of what we have learned that might be helpful to scholars and practitioners hoping to decrease abuses of personal integrity worldwide. While our primary focus in this study has been on theory, our findings do speak to issues of interest to practitioners. If governments, international organizations, and subnational actors are interested in improving respect for personal integrity around the world, one way for these actors to make headway toward this goal is, not surprisingly, to promote democracy. Promoting democracy is clearly not the entire answer, however. Our results indicate that if a nondemocratic country known to be a very serious violator of personal integrity rights were to change to a democratic form of government, human rights abuse would probably not disappear completely, other factors being equal. Neither should economic development be viewed as the panacea. Efforts to improve economic conditions within a country through programs like foreign aid might at times successfully promote human rights, defined more broadly, by leading to the provision for basic human needs, but our study indicates these conditions have, at best, a moderate impact on respect for personal integrity. Neither would population control efforts be likely to have a substantively important impact on repression of these rights.

Our findings do, however, reveal another course toward greater respect of personal integrity, one that would have an impact of about the same magnitude as converting autocratic regimes to democratic ones. We have shown that these basic rights can be enhanced by actors who would encourage countries to solve their political conflicts short of war, and use whatever means are at their disposal to assist them in doing so.

APPENDIX

In order to add to the already extensive data set gathered by Stohl and his colleagues (n.d.), Poe and Sirirangsi (1993, 1994) analyzed the content of the State Department and Amnesty International reports for the years 1980–87 and assigned values on a five-point ordinal scale according to the following coding rules, originally set forth in Gastil (1980):

- Countries [are] under a secure rule of law, people are not imprisoned for their views, and torture is rare or exceptional. . . . Political murders are extremely rare.
- 2. There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected, torture and beating are exceptional. . . . Political murder is rare.
- 3. There is extensive political imprisonment, or a

recent history of such imprisonment. Execution or other political murders and brutality may be common. Unlimited detention, with or without trial, for political views is accepted.

4. The practices of [level 3] are expanded to larger numbers. Murders, disappearances are a common part of life. . . . In spite of its generality, on this level terror affects primarily those who interest themselves in politics or ideas.

5. The terrors of [level 4] have been expanded to the whole population. . . . The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals. (Gastil 1980, quoted in Stohl and Carleton 1985)

Following Stohl and his colleagues (n.d.), two coders read and analyzed the content of each source and assigned a number between 1 and 5 to each country. After all coding was completed, the scores assigned by the two coders were compared. It was found that intercoder agreement was 85.6%, a percentage a bit lower than that achieved in one similar human rights data-gathering project (McCormick and Mitchell 1988, 1989) but still quite respectable. This percentage and the gamma statistics for the two variables indicate high intercoder reliability. For the measure derived from the Amnesty International reports, the gamma was .94; for that derived from the State Department profiles, it was .98.

After compiling the scores, the coders found that disagreements often occurred on three kinds of cases. First, there were cases that were categorized as a 2 by one coder and as a 3 by the other. These tended to be countries in which there was limited political imprisonment but considerable alleged torture or in which there was more torture than would ordinarily be indicated by the wording for the 2 category. A second kind of disagreement occurred on cases in which prisoners were taken after alleged demonstrations, riots, or revolution attempts but with uncertainty over whether their political activity was violent or not. Since a major concern was to make our data comparable to those gathered for use in the Stohl study, we consulted those researchers to see how they had dealt with these cases. A telephone conversation with Mark Gibney (February 1991) confirmed that those researchers gave the benefit of the doubt to the government, following the dictate that they should be "innocent until proven guilty." To remain consistent with the data provided by these researchers, we, too, followed this decision rule.

A third type of case on which coders tended to disagree involved countries that imprisoned conscientious objectors identified by Amnesty International as "prisoners of conscience." In such cases, according to Gibney, countries with large numbers of imprisoned conscientious objectors were coded as being in the 2 category as long as other human rights difficulties did not exist, but those that had only one or two such prisoners were coded as being in the 1 category. Again, following the practice of these researchers,

most of these disagreements were resolved through discussion and reconsideration of the facts of the particular cases. But if no resolution was in sight, again following the practice described by Gibney, a third coder was brought in to cast the deciding judgment.

Notes

We could not have written this article without the help of our colleagues and students. Comments and advice from Harold Clarke, Neal Beck, Jim Meernik, and Motoshi Suzuki kept us on track in our analysis of the pooled cross-sectional data set. The editorial suggestions of Vernon Van Dyke added grace and clarity to our sometimes inelegant language. Mark Gibney, Michael Stohl, David Carleton, and their coresearchers at Purdue University generously shared the human rights data they had coded from the Amnesty International and State Department reports for 1980 to 1987 and graciously answered inquiries about the coding of these data. The Interuniversity Consortium for Political and Social Research provided machine-readable versions of the data series contained in the World Bank's World Tables, in several volumes of Ruth Leger Sivard's World Military and Social Expenditures, and in the Freedom House Freedom in the World series, all of which provided portions of the data used in the analysis. These data are available to interested parties via GOPHER (gopher. unt.edu, port 70): look under "UNT Information & Resources/ Departments, Schools & Colleges Information/College of Arts and Sciences/Political Science/Repression." They are also available via anonymous FTP (ftp.unt.edu.)

We also wish to thank Rangsima Sirirangsi, Liz Thackwray, Hamed Madani, Jessie Hill, Michael Prior, Greg Pynes, Stacia Haynie, and Robb Stine for their able research assistance. Partial funding for their assistance was provided by the University of North Texas in the form of research initiation grants from the university, a Junior Faculty Research Grant, and a grant from the Department of Political Science.

1. The focus of our analysis will be the abuse of internationally recognized human rights, specifically those having to do with integrity of the person (Cingranelli and Pasquarello 1985; Henderson 1991, 1993; Mitchell and McCormick 1988; Stohl and Carleton 1985), a choice that will be discussed and justified later. We will, however, also refer to the dependent phenomenon as state terrorism. Our use of this term is compatible with that of Gurr, who states: "Regimes do many coercive things to induce compliance. They threaten, arrest and jail, fine and confiscate as well as murder. It is plausible, both analytically and psychologically to limit the concept of state terrorism to coercion that takes or grossly endangers the lives of its targets. Thus it includes imprisonment in conditions where many are worked or starved to death, and other denials of means of life, as well as outright killing. Violence by regimes is terroristic only if it is 'instrumental', which means designed to have a wider effect on some audience" (1986, 46). We choose to employ the terms inter-changeably because the two phenomena are inextricably linked: abuse of individuals is a human tragedy directly resulting from terrorist policies employed by nation-states.

2. For the linkage of U.S. foreign aid allocation to human rights issues, see Carleton and Stohl; Cingranelli and Pasquarello 1985; McCormick and Mitchell 1988, 1989; Poe 1990, 1991, 1992; Schoultz 1980, 1981; Stohl and Carleton 1985; and Stohl, Carleton, and Johnson 1984. For respect for human rights in relation to U.S. refugee policies, see Gibney, Dalton, and Vockell 1992; Gibney and Stohl 1988. For a linkage between democratic practices and national tendencies to go to war, see Rummell 1983.

3. McKinlay and Cohan analyze a set of regimes and regime—years across a 20-year time span, but they do not explicitly build time or within-unit change into their work.

4. Violations of personal integrity are usually perpetrated

directly by government officials and thus are more easily dealt with by a change in government policy, in direct contrast to violations of economic and social rights, which are often less amenable to change. It is unfortunately doubtful that economic rights (e.g., a right to food, a job, and a home) will be guaranteed in many African countries in the near future even if the government's intentions are exemplary, since the causes of these problems are oftentimes not in the government's control. Respect for social rights (e.g., social equality for women and minorities) are often based on centuries of cultural practice. For a regime to attempt a sudden change in practice before the time is right might lead to a coup, a revolution, or civil war. In such circumstances even reformminded regimes will likely be slow to change, for to change too quickly could result in a serious setback for the causes they are attempted to promote. By focusing our study on rights associated with the integrity of the person we are purposely directing our inquiry toward policies and crimes we feel can more easily be altered by national governments in the short term, in an effort to be policy-relevant. Perhaps in our future work we will deal with other definitions and dimensions of human rights.

5. For arguments that better data are needed, see Jabine and Claude 1992, esp. chapters by Banks, Bollen, and Lopez and Stohl. However, statements in some of these chapters and empirical work by some of these same researchers (e.g., Stohl and Carleton 1985; Stohl, Carleton, and Johnson 1984) support the argument that current data are sufficiently good to be used in meaningful empirical tests.

6. The measurement alternatives currently available to researchers of human rights phenomena are outlined and discussed in Lopez and Stohl 1992 and Stohl et al. 1986. Events data measures, like those reported in the World Handbook of Political and Social Indicators (Taylor and Jodice 1983), depend on newspaper accounts of different human-rights-related phenomena and use counts of state coercive behaviors (e.g., the imposition or lifting of government "sanctions" and political executions) to generate measures of human rights abuse. Research using events data to operationalize various kinds of repression has been reported by a number of scholars (Alfatooni and Allen 1991; Davenport 1992; Davis and Ward 1990; Hibbs 1973; Muller 1985; Ziegenhagen 1986).

Stohl and his colleagues (1986) cite major problems with this approach. First, the different repressive behaviors counted are often substitutable means used to reach the same end-the repression of those who are thought to pose an obstacle to the policies or continued power of the incumbent regime. For example, a decrease in reports of politically motivated arrests by a government might not be an accurate indicator of improving human rights if that decrease takes place soon after several hundred opponents of the government were executed. Second, since reports of human rights abuses are routinely suppressed by closed regimes to an extent that would not be possible in more open societies, events data measures sometimes lead to counterintuitive and invalid findings, like that reported by Stohl and colleagues indicating that the United States was a more repressive society in the years 1948-50 than the Soviet Union under Stalin. We therefore are in strong agreement with the pleas of Stohl and colleagues, who argue that "we must bring political knowledge to bear in shaping our collection of information and measurement schemes" (p. 598). So while events data may provide a useful supplement to the approach we take in this research, we choose not to use it primarily for the reasons just rehearsed. In addition, the events data we would need to operationalize our dependent variables are simply not currently available and our comprehensive international sample for our time period.

Accordingly, we see a standard-based approach as the best available measurement alternative at this time. An advantage of this approach is that it allows for a greater degree of political knowledge to be used in the development and application of standards to sources of human rights information specified by the researchers. Further, such measurement

techniques allow us to focus clearly on the integrity-of-theperson rights in which we are most interested in this study.

- 7. The ratings were derived from country profiles included in the Amnesty International Reports for 1981–88 and the State Department's Country Reports on Human Rights Practices for 1981–88.
- 8. In coding the Amnesty International profiles, the least repressive coding of 1 was assigned in only 71 cases for 1980–87, compared to 376 cases for the State Department reports. But there was little difference between the two in the number of cases their reports led us to place in the four higher categories. Further, of the 302 cases that were missing in Amnesty International, we gave 220 codes of 1 in our ratings derived from the State Department reports. Therefore, while Amnesty insists that the "omission of a country entry should not be interpreted as indicating that no human rights violations took place in that country" (Amnesty International 1987, 2), in practice, those countries that were omitted from the Amnesty International reports tended to be (but were not always) countries that appeared to adhere to the rule of law.
- 9. An issue of special concern to us are allegations of bias against the State Department reports (e.g., Carleton and Stohl 1987 and the sources cited therein). However, it is also important to note that some argue that the consistency of the U.S. State Department reports improved during the 1980s and therefore may be more valid in that period than they had been previously (e.g., Innes 1992). Furthermore, if some have argued that the State Department has been biased against leftist regimes, others have suggested that Amnesty International may have been more likely to note abuses by governments against armed leftist oppositions than by those oppositions against government and its supporters.
- 10. Closely related to these arguments are those of Howard and Donnelly (tested in the Mitchell and McCormick study) that "internationally recognized human rights require a liberal regime" because such regimes give the individual priority over the state (1986, 802). Since liberal regimes clearly fall into the subclass democracy as that term is conventionally defined, we see no reason to represent them separately in our analysis.
- 11. For example, the preface to a recent "worldwide survey" of democracy, notes that "humanitarian concern for human rights is inseparable from . . . democracy or the lack of it. Human rights are part, a good part, of democracy, and they can be assured only by legal, responsible, that is, democratic governments—at the same time affirming that "the question of democracy is broader" than the question of the status of human rights as typically assessed by Amnesty International, the State Department, and Freedom House (Wesson 1987, vii).
- 12. Bollen (1980) went on to create a variable based on his definitions, covering the years 1960 and 1965. Unfortunately, the measure has not been replicated on an annual basis and thus cannot be used in this study. For further discussion, see Bollen 1990, 1993.
- 13. The Polity II operationalization of institutional democracy omits any consideration of whether a regime respects the dignity of the person, which makes it an ideal measure for our purposes. Unfortunately, the Polity II measure is available only through the year 1986 and for only 127 of the maximum of 153 nations for which we have measures of state terrorism and our other independent variables. Since we lacked the time and resources to expand the Polity II democracy data to cover 1987 for all nations and 1980-86 for 26 additional nations, using Gurr's democracy index would have substantially reduced the N for our pooled analysis, so we explored other alternatives. However, analyses conducted with the Polity II measure on this reduced sample led us to conclusions similar to those reached in the analyses we shall present. The former are not presented in more detail due to space limitations.
- 14. A summary collection of values for this variable is reported in Freedom in the World (McColm 1990).
- 15. We do not wish to argue that the Freedom House indicators cannot be used to measure human rights, very broadly considered. But, as we have explained, the indicators derived from the Amnesty International and State Depart-

ment reports, which we analyze, focus much more narrowly on those infringements of the dignity of the person that we and others label "repression."

16. The criteria quoted from the 1989–90 edition of Freedom in the World (McColm 1990), are more elaborate than those in earlier editions. Nevertheless, the criteria, if not the clarity with which they have been expressed, appear to have been consistent over the years. Compare, for example, the following from the 1980 edition of Freedom in the World: "Political rights... allow people to participate freely and effectively in choosing their leaders or in voting directly on legislation.... The rating a nation receives for political freedom is determined by factors such as the existence of two or more competing political parties or the independence of opposition candidates from government control... Elections and legislatures have to demonstrate a significant opposition, and those elected have to be given real power" (Gastil 1980, 4–5; emphasis original).

17. These freedoms and others are a part of the Freedom House civil liberties rating. However, the civil liberties rating is not a viable measure of democracy for our use because it specifically includes "protection from unjustified political terror, imprisonment, exile or torture," which overlaps clearly with the indicators derived from the Amnesty International and State Department reports, which are our dependent variables, as well as a number of other rights that are not essential to democracy as envisioned by Bollen (1990, 20-21). Perhaps because the Freedom House civil rights indicator includes basic civil liberties that are a part of many definitions of democracy, the overall correlation between the political and civil rights ratings in our data set is very high, .935. Nevertheless, the conceptual differences between the two indicators and the explicit overlap of the civil liberties indicator with repression convinces us to consider only the political liberties rating as our measure of democracy.

18. This was also noted by Gurr. In contrasting the Polity II indicators with the Freedom House measures, Gurr noted that "the problem with the latter is that Gastil assigned annual scores partly on the basis of repression" (personal communication, 15 September 1992).

19. Bollen's (1993) article appeared too late for us to take full account of his findings and his measurement suggestions in our analysis. But we should note that despite his conclusion about the superior validity of the Freedom House measure, Bollen does not endorse the use of a single indicator. Instead, he suggests using structural equation models to construct indicators that maximize validity while minimizing systematic and random measurement error. Unfortunately, he also notes that techniques appropriate for dealing with "pooled cross-sectional and time series data for nations" are "underdeveloped in latent variable models" (p. 1224). As a secondary strategy, he suggests using factor scores from several indicators as a composite measure of democracy. We might have adopted this approach but did not because (1) the data required to construct the composite index ultimately recommended by Bollen were not available to us in appropriate time series form prior to our completion of our analyses and (2) the suspicion with which many analysts regarded the Freedom House measure mandated that we analyze it separately in order to contrast it with an indicator that represented a different and nonjudgmental approach to measuring democracy, the Vanhanen index.

20. The zero-order correlations between the Freedom House measure of democracy and indicators of human rights abuse are only -.47 for ratings derived from the Amnesty International reports and -.54 for those derived from the State Department profiles in the 1980-87 period. With the Vanhanen measure of democracy, the correlations were -.50 and -.52, for those two measures of human rights, respectively. With the Polity II measure, which limits the analysis to a smaller and temporally different number of cases the correlations were -.55 and -.52, respectively.

21. The skewed distribution of total population made it desirable to log this variable to meet the statistical assumptions of our methods.

22. Clearly, the ideal would have been to employ estimates that accurately reflect changes from year to year. When efforts were made to obtain such estimates, however, the results were disappointing. The numbers generated for many data points were simply unrealistic, due to sudden changes (sometimes unexplainable decreases) in gross population estimates for Third World nations and for countries with small populations. We therefore employed a variable indicating the average population change of a country over the eight-year period of the study as the measurement alternative that was better in practice. We did not expect—and saw no—evidence of faulty estimates causing any difficulties in estimating the gross population variable, as the few difficulties were not of a difference of magnitude.

23. For example, Henderson used energy consumption and its growth as his measures of economic growth and development, and Park used the physical-quality-of-life index created by Morris (1979) as his measure of "economic basic needs rights." In addition, Henderson used an international human suffering index to measure "socioeconomic needs," which turned out to be very highly correlated with his energy consumption measure of economic development (Population Crisis Committee 1987). The continuing dissatisfaction with GNP per capita has also led to the creation and reporting annually of a new human development index by the United

Nations Development Programme (1990, 1991).

24. Although this classification was created by a single coder, Hamed Madani, it has been subjected to a variety of tests designed to document its validity and reliability. Crossverification against the coup data set of O'Kane (1987) and against the "military control" classifications of Sivard (1989) found very few instances in which Madani's data disagreed with O'Kane's as to whether a coup had occurred in a given country and relatively few instances when Sivard coded a nation as military-controlled and Madani did not. In the latter case, such disparities appeared to result more from the use of a broader and vaguer classification scheme by Sivard than from any error on Madani's part. In the former case, O'Kane's data usually appeared to be in error. As a final test, Madani's classification was compared to two independent classifications prepared by two undergraduate research assistants, Jessie Hill and Michael Prior. Again, there proved to be few instances of disagreement, and, when there was disagreement, it was most often because the undergraduates coded a regime as civilian due to a failure to realize that it had originally come to power through a coup.

25. Bollen and Jackman (1985, 1988) provide strong empirical evidence for the effect of British colonial influence on democracy, as operationalized by Bollen's democracy index for 1965. However, readers should remember that like Mitchell and McCormick, we take pains to distinguish democracy, one of our independent variables, from repression of personal

integrity, our dependent variable.

26. Using events data in a crossnational research design, Hibbs does show a positive relationship between "internal war" and "negative sanctions" by government (1973, 182).

27. In coding international and civil wars we used as a guide information from Sivard 1991 and Brogan 1990. In cases where there were doubts as to whether the criteria were met, we sought information from other sources dealing with the situation in the particular country.

28. The White technique was adapted to the PCT design by Beck and his colleagues (1993). According to them, "if X is the variance—covariance matrix, the robust variance—covariance matrix of the errors is estimated by"

$$(X'X)^{-1}X'\,\frac{(E'E)}{T}\otimes I_TX(X'X)^{-1},$$

where **E** is the $T \times N$ matrix of residuals, T is the number of time points, N is the number of nations, **I** is the $T \times T$ identity matrix, and \otimes is the Kronecker product (p. 946). The equation

$$\frac{(E'E)}{T} = \hat{\Omega}$$

represents the estimated covariance matrix of the country errors used in feasible generalized least squares analyses of pooled cross-sectional time-series data sets, (ibid.). This procedure does not affect the OLS coefficients, but it does estimate more consistent standard errors. The RATS procedures that allowed us to use the robust-standard-errors approach on PCT data were developed and used in Beck et al. 1993 and Beck and Katz 1994 and were generously provided by Nathaniel Beck, via his FTP server.

29. To test for the possibility of autocorrelation difficulties, we computed the basic model for both the dependent variables derived from the Amnesty International and State Department reports with both democracy variables. An inspection of the residuals gained from OLS regression turned up few examples of sign changes in the residuals resulting for particular countries, a pattern indicating that positive autocorrelation is a difficulty (Ostrom 1990). The Durbin–Watson statistics ranged from .44 to .86, in the four preliminary tests of the basic model, thus also indicating that positive autocorrelation likely affects OLS results. For an argument advocating the use of lagged dependent variables to deal with autocorrelation difficulties, see Beck and Katz 1994.

30. We were reluctant to discard data from this year because it decreased our sample size by one-eighth. We therefore experimented with an estimation method that allowed us to retain the first time point while dealing with autocorrelation and heteroscedasticity-the Prais-Winsten estimation technique (Ostrom 1990, 31-41), used in a first-order autoregressive (AR(1)) model for which we calculated White's robust standard errors, using RATS 386, version 4.02. There were some divergences between the results yielded by this approach and those we rely on herein. Specifically, it appeared that military control and British cultural influence had much stronger and usually statistically significant effects on repression when the first case was retained in an autoregressive model with robust standard errors. However, when we inspected the estimates calculated for the first case for each of our variables, we became skeptical. The values of the estimates usually appeared quite atypical of their series, especially for the dichotomous variables. Also, when tests were conducted with a similar AR(1) model, with the first year discarded to ascertain the effects of these estimates, the coefficients for these variables decreased in both size and statistical significance. We took this as an indication that the strength of these variables in our AR(1) models with the Prais-Winsten estimates was likely due to faulty estimations of the first time point. In the text, therefore, we choose the more cautious path and present the analyses with the first case discarded, due to our use of an lagged endogenous variable-analyses that do not include significant effects for British cultural influence or military control.

31. Since the Durbin–Watson statistic is biased when a lagged dependent variable is entered on the right side of the equation, the autocorrelation functions were examined to determine whether the autocorrelation difficulties were solved. The autocorrelation functions for lag 1 in the four sets of analyses ranged from –.16 to –.18, with standard errors of about .03. Most of the autocorrelation functions for the remaining lags were at, or below, .10, the largest being just .18. Thus we can say with some confidence that autocorrelation does not pose a problem in our interpretation of these

sults.

32. This can be done quite simply. To find the effect of the loss of democracy at time $_{t+1}$, we simply multiply the effect of this loss at time $_t$ by the coefficient of the lagged dependent variable and add the direct effect of democracy at time $_{t+1}$. Repeating this process for each successive lag will, after several lags, fall into an asymptotic pattern if the loss of democracy continues.

33. The Vanhanen democracy index is not available for the 1970s.

34. We do not present graphs of these effects here, but we will gladly provide them on request.

35. This literature is far too voluminous to cite fully. Relevant early research includes Cutright 1963; Lerner 1958;

Lipset 1959; and the studies cited in Tate 1972. More recent work is cited and discussed in Bollen 1990, 1993; Bollen and Jackman 1985, 1988; and others.

36. However, in a set of supplementary analyses for 1981-87 using an AR(1) model to control autocorrelation and White's robust-standard-errors approach to control for heteroscedasticity, the British influence variable did exhibit a statistically significant coefficient of moderate strength (.2 to .3) in analyses using the Vanhanen measure of democracy. So perhaps this hypothesis should not be dismissed completely at this point. Other results yielded by these analyses were very similar in terms of the variables that reached statistical significance, with the exception of the international war variable, which failed to reach conventional levels of statistical significance in analyses with the dependent variable derived from Amnesty International profiles, but did achieve statistically significant coefficients of around .5 in the analyses based on ratings generated from the State Department reports. These analyses are not presented in more detail because of space limitations and because the findings are generally similar. We decided to present the OLS analyses with the lagged dependent variable because it is a more widely understood method, because of the ease in calculating the lagged effects of independent variables, and because the results were more consistent from one set of analyses to another. The results of the AR(1) models described here are available upon request from the authors.

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"THE SLOW BORING OF HARD BOARDS": METHODICAL THINKING AND THE WORK OF POLITICS

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From the perspective of quite different theoretical traditions, Simone Weil and Hannah Arendt present nearly identical and equally powerful critiques of technological determinism in modernity. Yet unlike Arendt, Weil develops a concept of work that draws a distinction between technology and instrumental action as "methodical thinking." As a result, Weil's theory of action embraces something that Arendt's theatrical politics rejects—a concept of liberatory instrumentality, or purposeful performance. I shall reassess some of the inadequacies of Arendt's concept of work and develop Simone Weil's concept of methodical thinking in order to argue for a more neighborly affinity between work:interaction and purposeful:theatrical performance than Arendtian public realm theory, for all its power, currently allows. If such an affinity is possible, then public realm theory might be more adequately equipped to deliver on what I take to be its promise as an emancipatory project in late modernity.

When we were forming the new structures, we had to hire people from the old structures. Our supporters—the people who came to rallies and street demonstrations—didn't know anything about how to run a country.

Aleksandr Sokolov, ex-commandant
 Central Committee Building, USSR

n 1994, we are just beginning to take the measure of the astonishing revolutionary events that heralded the final decade of this century and initiated the capacity of millions of citizens to act in concert for the power of democratic self-realization. For political theory, the challenge in the aftermath of 1989 is to advance understandings of democracy and political life that attempt to recover an emancipatory potential in modernity and meaningfully confront what Václev Havel calls "the practical task of organizing a better world" (1990, 136). In many respects, I think that the most promising source for this enterprise lies in public realm theory, perhaps particularly in the theories of Hannah Arendt and Jürgen Habermas. Both of these thinkers sustain a deep critical commitment to a politics of citizen interaction, reciprocal understanding, self-realization, and human dignity in a realm where, as Arendt puts it, "speech and only speech [makes] sense" (1958a, 25-26).

The compelling quality of Arendt's and Habermas' public realm conceptions of politics has much to do with their adaptations of a venerable conceptual strategy that has its roots in Aristotle and draws a palpable distinction between the human activities of "work" (techne) and "interaction" (praxis). With this strategy to hand, public realm theory inventively juxtaposes a theatrical image of the space of appearances (Arendt) or a communicative metaphor of the public sphere (Habermas) against an instrumentalist reading of manipulative work-processes. Thereby, Arendt and Habermas effectively particularize politics as a vibrant and unique human activity and develop a critique aimed at freeing the subject from

the technical domination and violence to nature that they resolutely associate with work. They also invest freedom or emancipation in expressly noninstrumental, linguistically mediated, modes of human action in the world.¹

My inclination is to support attempts to locate freedom in a politics of speech or communicative interaction. Thus I do not wish to contest the turn in public realm theory that allows intersubjective understanding to achieve, as Axel Honneth puts it, "the status in the theory of emancipation which social labor had had in Marxian theory" (1982, 45). Nevertheless, I think that the conceptual strategy that Arendt and Habermas so brilliantly employ in order to counter modern theories of action as social labor is a double-edged sword; for even as the conceptual dichtomy between work and interaction provides public realm theory with a valuable analytical device for distinguishing between human activities, it undercuts the effective theorizing of politics as a human practice. The dichotomy work:interaction seems especially to foreclose what Max Weber had in mind when he called politics "the slow boring of hard boards"—a sustained, purposeful activity that meets obstacles and undertakes acts of transformation in the world (1946, 128). This conceptual inadequacy in public realm theory is not, I think, a minor matter. If our most praxis-oriented theories cannot deliver an action-coordinating concept that appreciates the purposeful nature of human struggle as politics, we should hardly be surprised to find the "project of modernity" (Rasmussen 1990, 7-8) deviously undermined, if not defeated, by other alternatives, including the seductive, "affirmative nihilism" (Villa 1992,

719) of late-twentieth-century postmodernism.

We need, then, to rethink the action concept of politics in public realm theory. To pursue this task, I shall focus explicitly on Arendt's theory of action, not only because it presents public realm theory in what is perhaps its most exemplary anti-instrumental-

ist form but also because it is has played a role in at least one of the most dramatic emancipatory events of our time. To provide an instructive critical perspective, I turn to a contemporary of Arendt's, the French thinker Simone Weil. Compared to Arendt, Weil has received relatively little attention as a social and political theorist; but she is surely Arendt's equal as a diagnostician of modernity. Indeed, from the perspective of quite different theoretical traditions, Weil and Arendt present nearly identical and equally powerful critiques of the technological determinism of modernity.

Yet unlike Arendt, Weil draws a distinction between technology and instrumental action that refuses to reduce all forms of instrumentality to violence or purely manipulative processes. As a result, Weil's concept of work as methodical thinking embraces something that Arendt's conceptual strategy rejects—a liberatory form of instrumentality. A liberatory form of instrumentality is precisely what public realm theory must be ready to accommodate if it is to be practically prepared for the challenge posed by the world events of 1989. Arendt's public realm theory in particular needs to discover a more neighborly affinity (in Hans-Georg Gadamer's felicitous phrase) between work:interaction and instrumentality:politics than the current agonistic/discursive debates over Arendtian politics allow. With this in mind, I tentatively propose the idea of a methodical politics, in order to suggest that Weil's notion of work (instrumentality as purposeful performance), is potentially compatible with Arendt's notion of interaction (politics as theatrical performance). If this affinity is indeed possible, then public realm theory might be more adequately equipped to deliver on its promise as an emancipatory project in modernity.

AUTOMATISM AND THE SHATTERING OF THE WORLD

Simone Weil's and Hannah Arendt's very different theoretical assessments of freedom in modernity nevertheless arise out of nearly identical diagnoses of the times. In a very general sense, they were both part of a generation of twentieth-century social theorists who lived their formative intellectual lives in interwar Europe, in an epoch when the Enlightenment's summer bloom was past and modernity's polar night proceeding.⁷ Equivocal humanists and ambivalent modernists, both Weil and Arendt bear the traces of darkness before the face of universalized forces of domination. Yet in their arguments we can also find purposes that place them within a context of twentieth-century thinkers struggling to redeem the modern project of emancipation. Weil and Arendt's respective analyses are, in this sense, efforts to respond to the sterile passivity of modernity, the degradation of the human person, and the suppression of individuality by appealing to thought-action accounts of freedom that are aimed at recovering what technological forces have taken away. Consequently, Weil and Arendt share a kind of hope in their concern to identify forms of human activity that disclose the reality of the world, so that human beings as individuals may actualize "relations worthy of the greatness of humanity" (Weil 1973, 104) and guarantee that "human life can be at home on earth" (Arendt 1958a, 134).

Like many intellectuals of their time, Weil and Arendt understand the crisis of the modern age as a period of profound fragmentation and deformation accelerated by "the frenzy produced by the speed of technical progress" (Weil 1973, 54).8 Like others, they situate the modern crisis in relation to the aftermath of the Industrial Revolution, wherein the imperatives of economic conquest, mass production, routinized labor, and relentless consumption gradually usurp the activities of fabrication and workmanship (ibid., 115; Arendt 1958a, 124–25). More uniquely, however, they both confront what Arendt vividly terms the shattering of "the very purposefulness of the world" (1958a, 150) as the consequence of something they call automatism. Automatism is the third stage in human production, and the most recent feature of the productive relations of advanced, consumer capitalism (ibid., 4, 147-49; Weil 1973, 52). Automatism, Weil argues, follows a principle that "lies in the possibility of entrusting the machine not only with an operation that is invariably the same, but also with a combination of varied operations" (1973, 52). Arendt draws the implication that "we call automatic all courses of movement which are self-moving and therefore outside the range of willful and purposeful [human] interference" (1958a, 151). Assessing automatism as a complex of elements, Weil and Arendt identify the same three elements as problematic.

First, automatism (or automation) involves the appropriation of homo faber (roughly, work) activity by forms of mechanized industrial production that substitute reflexive, bodily processes for experiential work that distinguishes between ends and means, operation and product (Arendt 1958a, 149-51; Weil 1977). Under the pressure of automatic machines, Arendt argues, the defining features of homo faber are in jeopardy. The distinction between means and ends no longer makes sense; tools and instruments "lose their instrumental character"; the standards of utility and beauty are replaced by "basic functions"; the act of fabrication is subsumed in consumption; and homo faber's driving attitude—the "conscious human effort to enlarge material power"—is neutralized in a stream of never-ending processes (Arendt 1958a, 137, 145, 152). As Weil notes, under automatic machines, "thought draws back from the future" into a "perpetual recoil upon the present" in which human beings are "reduced to the condition of automata" (1973, 92; 1977, 57).

Second, automatism simultaneously supplants and recuperates the rhythms of nature, thereby invoking a form of species existence that mimics the natural life cycle, elevates basic functions, and deflates the social value of human creativity and worldly artifice. As a

result of technical developments, Weil writes, "humanity finds itself as much the plaything of the forces of nature, in the new form that technical progress as given them, as it ever was in primitive times" (1973, 83). Arendt elaborates: "The danger of future automation is less the much deplored mechanization and artificialization of natural life than that, its artificiality notwithstanding, all human productivity would be sucked into an enormously intensified life process and would follow automatically, without pain or effort, its ever-recurrent natural cycle" (1958a, 132). Automatism is not only an obstacle to individual freedom; it is also a specific organizational form of the human species that, paradoxically, liberates action from the constraints imposed by the pains and dangers inherent in earlier forms of life by mutating action into an extramundane repetitive process.

Within this situation, a third element arises in the complex that identifies the modern age-the dissolution of the world of the senses and appearances into a pseudoworld (Arendt 1958a, 153) that produces pseudo-ideas (Weil 1973, 110). Rather remarkably, both Weil and Arendt identify algebraic physics as the prime producer of a nongenuine, counterfeit world (Arendt 1958a, 264-73, 285-88; idem 1969, 265-90; Weil 1968).9 Physics proceeds by stripping away the subjective effects of the human senses with a view toward an objective theory of physical particles in fields of force. It thereby manifests a loss of confidence "in the phenomena as they reveal themselves of their own accord to human sense and reason" (Arendt 1969, 272). Weil argues that for physics even to be possible, perceptual appearances and the world as a mediating medium must be considered negligible: "The negligible is nothing other than what has to be neglected in order to construct physics. . . . What is neglected is always as large as the world" (1968, 32). The neglect is furthered by algebraic formulae and equations that dissolve all real human relationships into "logical relations between man-made symbols" (Arendt 1958a, 264-65, 284; Weil 1968, 37). Once in the world of these symbolic relations, "one inevitably loses sight of the relationship between sign and thing signified," and a form of mechanized petrification sets in (Weil 1973, 93). Hence even as algebraic formulae advance the processes of technology and automation, the formulae themselves proceed automatically and become "impenetrable to the mind," stifling the human capacity to think (ibid.; Arendt 1958a, 5).10

Weil's account of automatism, which she calls the triumph of "the blind collectivity" (une collectivité aveugle), is written in the turbulent and authoritarian climate of Europe in the early 1930s and replete with images of grinding mechanisms, vast crushing forces, and a domain of control in which "there is no means of stopping the blind trend of the social machine towards an increasing centralization" (1973, 120). Arendt's account of automatism, which she calls the victory of animal laborans, is written in the relatively placid but paranoid consumer atmosphere of the United States in the 1950s and appropriates meta-

phors of the biological life cycle to characterize "the reckless dynamism of a wholly motorized life process" that is inherently futile and worldless (1958a, 132). These temporal and metaphorical differences are more matters of degree than kind, however, given Arendt's references to motors and machines (1958a, 93, 322), and Weil's inclination to view automatism as the pressure of nature and necessity upon human action (1973, 63). What they share is an account of advanced society as a domain of endlessly repetitive, artificially natural cyclicalities.

HOW HOMO FABER WORKS

Before the deadly and dehumanizing forces of automatism, Weil and Arendt seek to identify the practical context within which human beings as individuals can actualize freedom as a mode of thought in action and thereby regain a sense of being at home in the world. In doing so, they both reject the modernist strategy—so often associated with the liberal historicist outlook of the nineteenth century—that finds freedom in the domination of nature and necessity. They also reject the romantic reaction of the early twentieth century, with its yearning for submergence within nature and necessity. The metaphysical picture behind both Weil and Arendt's accounts is that of an all-pervading necessity that forms the fabric of the world but wherein freedom, as the recognition of necessity, is nevertheless a human possibility (Arendt 1958a, 121; Weil 1973, 85).

Given their shared view of automatism as a process that destroys the experience of work as a causally efficacious relationship between human beings and the world, we might expect that both Weil and Arendt would link freedom in some way to working in the world. In fact, in her magnum opus¹¹ Weil wholly situates the practical context of freedom in "a civilization in which labour would be sufficiently transformed to exercise fully all the faculties," or in what we might call the *purposeful-performance* of work as self-determination (1973, 104). In The Human Condition, Arendt wholly situates the practical context of freedom elsewhere. Even as she pays homage to Weil's Factory Journal, which she deems "the only book in the huge literature on the labor question which deals with the problem without prejudice or sentimentality," Arendt entrusts freedom to "men's acting and speaking directly to one another," or to what we might call the theatrical performance of politics as self-revelation (1958a, 131 [n. 83], 183). These different strategic moves and their implications for the practical context of freedom are directly related to the different ways in which Weil and Arendt think about how homo faber works. The practical political inadequacy of Arendtian public realm theory originates here as well. But to see this, we need first to consider how Weil thinks homo faber works as homo methodus13 and how Arendt thinks homo faber works as homo fabricatus.

Homo Methodus

In Weil's theory of action, work is a critical concept that designates a normative and emancipatory form of human activity. Loosely following Marx, she uses the term to distinguish both a human form of reproduction and a value sphere within which the human species gains access to reality. Thus, when labor is "sufficiently transformed to exercise fully all the faculties," it forms "the human act par excellence" (1973, 104). But Weil also objects that in Marx, "the locus of the process of freedom was transferred from the individual to the species" (McLellan 1990, 72). 14 There is an insistence upon individual subjectivity in Weil's concept of work, and it takes form in the "garden contrast" (Winch 1989, 96) between work, "wherein only efforts exclusively directed by a clear intelligence" determine the expenditure of the working subject's effort, and "blind operations" (d'opérations aveugles), where "the mind is weighed down" and movements are beyond control of the intelligence (Weil 1973, 98, 94). Here Weil introduces the idea that if human beings are to escape the humiliation foisted upon them by automatism, they must recover the capacity for methodical thought (la pensée méthodique). This is the driving theme of her magnum opus, and the focus of her thought-action account of freedom as well.15

For Weil, methodical thinking is a kind of activity in which the subject independently conceives of, coordinates, and carries out physical efforts or undertakings in the external world. 16 Such action is analogous, she argues, to solving properly a problem in arithmetic or geometry. Thus, "a completely free life would be one wherein all real difficulties presented themselves as kinds of problems, wherein all successes were as solutions carried into action" (1973, 86). The analogy to mathematical problem solving posits freedom as the activity of enacting a solution (success) in response to a problem, once identified. When freedom is realized in work, then, it is as a mode of instrumental action in which the subject determines his or her own activity as homo methodus, according to his or her own problem-solving initiative. Thus, Weil writes, "the absolutely free man would be he whose every action proceeded from a preliminary judgment concerning the end which he set himself and the sequence of means suitable for attaining this end" (p. 85).

On its face, the analogy Weil constructs between methodical thinking and problem solving has its drawbacks-not the least of which involves its appeal to mathematics (Winch 1989, 96-99). There is another drawback evident here as well, for with its overt appeal to means and ends, Weil's analogy appears to connect freedom with the Weberian notion of purposive-rational (zweckrational) action that reduces life activity to specific instrumental properties. But this is not, in fact, the case. Indeed, if we view purposive-rational action as (1) geared toward the production of objects, (2) directed toward the domination of nature, (3) governed by technical mod-

els, and (4) driven by "success" in the securing of a goal or an end, then Weil's concept of methodical thinking must be taken as an action type of considerably different cognitive and ethical magnitude.

To see how this is so, we might first return to the problem orientation that Weil advances as the context within which methodical thinking takes place. Unlike concepts of work that take object production as their primary context, Weil's concept places the human appropriation of "means" within a kind of thoughtaction experience through which the subject perceives the world not as a construction site but as a domain of obstacles, challenges, or perplexities open to possible solution. The emphasis thereby shifts from the making of things to the conscious identification of, and response to, "things thrown forward" or problems (Gk. *pro-*forward + *ballein-*to throw). The kind of work activity Weil has in mind assigns a secondary status to the single end-product of work and primary status to problem identification and the undertaking of a solution within a circumscribed context (1973, 104). Only within the context of responding to challenges, Weil argues, does the subject "dispose[s] of his own capacity for action" and exercise "fully all the faculties" by bringing judgment to

bear upon circumstances (p. 85).

Weil also distinguishes the problem orientation that is methodical thinking from the purposive-rational urge to conquer or dominate nature. Here we might attend to the metaphors of high seas and sailing she often uses, in order to grasp how she distances work from an act of violent overcomingfor example, "The intelligence is powerless to get its bearings amid the innumerable eddies formed by wind and water on the high seas; but if we place in the midst of these swirling waters a boat whose sails and rudder are fixed in such and such a manner it is possible to draw up a list of the actions which they can cause it to undergo. All tools are thus, in a more or less perfect way, in the manner of instruments for defining chance events" (1973, 89; see also pp. 88, 91, 101). In this image, nature is both a zone of danger and a domain of possibility, not an inert force to be wrestled under control. The kind of voyaging spirit that Weil has in mind is not that of "the Dutch sea-captain who would go through hell for gain, even though he scorched his sails" (Weber 1958, 57). In the latter case, the boat is a ship, and the ship and its sails are nothing more than expedients for the captain's ruthless pursuit of booty. Weil's sailor is occupied differently. Apparently immune to the daring of adventure and without port or destination, the sailor brings the boat to life simply in the activity of engaging the sea, in a methodical undertaking that risks intelligence, not temerity, against chance. In this image, the tools of work are not directed toward domination, nor are they mere conveyances. They are rather instruments through which the methodical thinker initiates and attempts to maintain a tentative equilibrium within circumstances that challenge and constantly change.

Because the methodical thinker must "continually

use craft and strategy, set sails and rudder, transmute the thrust of the wind by means of a series of devices" (Weil 1973, 91), he or she cannot find freedom in a resting place or glory in victorious struggle. Indeed, Weil argues, it matters little whether actions are "crowned with success" (p. 85). Failure can bring unhappiness, but it cannot *humiliate* if a human being resolutely acts "under control of the mind" by filtering an "undefined mass of possible accidents" into a "few clearly defined series" (p. 88). This is also to say that such a person acts in full recognition of the powers of nature; hence Weil appreciatively cites Bacon's dictum, "We cannot command Nature except by obeying her" (p. 107). Only in this tentative equilibrium between body, mind, and universe, where freedom is "near the threshold of the danger of servitude" (Nietzsche 1968, 92) can one locate the ineffable skill of methodical thought. Methodical thinking, it seems, values a particular form of navigation as a mode of acting in the world. 19

When one has excluded from methodical thinking the urge to objectify, dominate, and secure success, it by no means follows that work is completely purposeless, goalless, meaningless—"a snake biting its own tail" (Nietzsche 1968, 81). Weil's exclusions invite us to comprehend work from a different aspect, but they do not erase purposefulness from the picture. The great stimuli, in fact, are problems that, once identified, arouse thoughtful attention. The more work "throws up such difficulties," Weil argues, "the more the heart is lifted" (1977, 59). The great engagement is in the conscious undertaking of a method or solution, "when wits are exercised, devices tried, obstacles cunningly eliminated" (ibid.). The great release has "nothing to do with welfare, or leisure or security"; it is actualized in the intelligent navigation of challenges and solutions carried into action (Weil 1973, 101). Only in this form of action does the subject challenge what Václev Havel calls "the diktats of automatism" (1985, 34) through work that is problem-oriented but not objectifying, masterful but not dominating, and purposeful but not preoccupied by a single "end." Not surprisingly, then, Weil argues that all human activities should "imitate the accuracy, rigor, scrupulousness which characterize the performance of work", lest they "sink into the purely arbitrary" and be submerged by forces beyond control (1973, 85). A neighborly affinity thus distinguishes the conceptual relationship she draws between work as methodical thinking and other human pursuits. This purposeful performance concept of human action is at the foundation of Weil's account of freedom.

Homo Fabricatus

When Hannah Arendt thinks about how homo faber works in the world, she, too, posits a notion of purposeful human performance against the endlessly repetitive, artificially natural processes of automatism. Thus she argues, in a seemingly Weilian spirit, "Whenever men pursue their purposes, tilling the

effortless earth, forcing the free-flowing wind into their sails, crossing the ever-rolling waves, they cut across a movement which is purposeless and turning within itself" (1969, 42). But on closer inspection, Arendt's characterization of the sailor's activity bears little resemblance to Weil's example of the boat as an instrument for defining chance events. For Arendt goes on to identify "purposeful human activities" as those which "do violence to nature" (p. 43). In this image, the sailor as seafarer is a violator who is disposed toward command and control of the natural environment, and the ship cutting across the waves is the instrument through which his domination over nature is extended. The image is powerful, but it also points to a more general problem in Arendt's thinking about how homo faber works.

In the tradition of German philosophy, and especially under the influence of Martin Heidegger, Arendt conceptualizes work as an emblem of modernity. She also designates work activity not as a material relation between humans, instruments and nature but as a kind of visionary nightmare rooted in "the utopian hope that it may be possible to treat men as one treats other material" (1958a, 188). The theoretical result of this Heideggerian maneuver is Arendt's concept of homo faber, in which she finds personified the "typical attitudes" of a (masculinist) modern age: "his instrumentalization of the world, his confidence in tools and in the productivity of the maker of artificial objects; his trust in the all-comprehensive range of the means-end category; his conviction that every issue can be solved and every human motivation reduced to the principle of utility; his sovereignty, which regards everything as material and thinks of the whole of nature as 'an immense fabric from which we can cut out whatever we want . . . '; finally, his matter-of-course identification of fabrication with action" (ibid., 305-06). With this breathtaking indictment, Arendt summons the spirit of heroic capitalism in modernity, in which all things are treated as means, all acts are oriented toward success, and man conducts himself as lord and master, a "destroyer of nature," who rips and cuts his materials "out of the womb of the earth" in relentless pursuit of the acquisition of goods (pp. 139, 153).²⁰ With the nullity that is victorious capitalism still to come, Arendt conjures up homo faber as the last blast of a ruthless epoch that arose "from the great revolution of modernity" (p. 307). But this conjurer's trick has its costs. In order to indict modernity, Arendt must make homo faber a powerful symbolic-historical category, and in order to do this, she must conceptualize the human activity of work in a constricted way. On a symbolic level, that is, Arendt's depiction of homo faber is a dramatic, expressive triumph, but on a conceptual level her analysis is a series of rigidifying moves that hammer out the normative content of work activity and destroy its emancipatory content altogether.

Arendt's first move is, perhaps, the most devastating for an emancipatory-action concept of work. In this move, the notion of work as a causally efficacious Methodical Thinking December 1994

and meaningful equilibrium between human beings and circumstances (of the sort Weil advances) is quickly resolved into nothing more than the devising of instruments and appropriation of tools for the making of things. Arendt's homo faber is simply the shell inside which we find that energetic constructor homo fabricatus (1958a, 139-44, 305-07; 1969, 215-17). Situated within a world of artifice, the activity of homo fabricatus takes on an orientation that is exclusively directed toward the object. Hence Arendt's second move is to theorize fabrication as an act "where a clearly recognizable end . . . determines and organizes everything" (1958a, 156-57; 1969, 215). Under these auspices, the actual process of engaging in fabricating activity is "secondary and derivative"; homo faber, Arendt argues, experiences processes "as mere means toward an end" (1958a, 307). The action structure of work, or what happens in between "the definite beginning and the predictable end" is thereby reduced to the negligible, while the end is granted existential priority (p. 144). In Arendt's third conceptual move, this already-stunted notion of work as an objectifying means-end orientation now becomes a thoroughly negative formulation as well. The use of tools in the production of objects is secured to "the degradation of all things into means" (1958, 156), "the limitless devaluation of everything given" (1958, 157; 1969, 216), and the "element of violation and violence" (1958, 139). This move to degradation, devaluation, and violence is decisive. With it, Arendt surrenders the ability analytically to grasp the difference between instrumental action as utilitarian objectification and instrumental action as, in Weil's sense, purposeful performance.21 As a result, she conceptualizes all forms of means-end relations as necessarily and not just contingently connected to acts of violence, manipulation, and control.

Arendt's final move should come as no surprise. In the attitudes of the modern age that she assigns to the means-end activity of homo faber, there is no room for genuine interaction oriented toward teamwork or mutual subjectivity. The only company that grows out of workmanship, she argues, "is in the need of the master for assistants or in his wish to educate others in his craft," against which she opposes "the multiheaded subject," or the teamwork of animal laborans (1958a, 161–62). Homo faber appears only in the "exchange market" and not in "the specifically political forms of being together with others, acting in concert and speaking with each other" (p. 161; Arendt 1969, 217). To appreciate the rather unimaginative and conceptually constricted nature of (both sides of) this Arendtian opposition, we might compare it to an example that Weil offers of purposeful performance as a form of interaction at work: "A team of workers on a production-line under the eye of a foreman is a sorry spectacle, whereas it is a fine sight to see a handful of workmen in the building trade, checked by some difficulty, ponder the problem each for himself, make various suggestions for dealing with it, and then apply unanimously the method conceived by one of them, who may or may

not have any official authority over the remainder. At such moments the image of a free community appears almost in its purity" (1973, 101).

This telling example is characteristically Weilian in its subjectivism (and its yearning for absolute purity). Nevertheless, it realistically differentiates and emancipates where Arendt's formulation congeals and confines. The human situation that Weil's purposeful performance account challenges as contingent and alienating (the herd-team of object producers) is one that Arendt deems intrinsic to labor (as teamwork) and to work (as fabrication). Arendt, it seems, can have no equivalent of Weil's problem-oriented, collectively methodical builders. This is not only because she reduces work to an object orientation that prohibits genuine relations with others but also because she reduces teamwork to the routinized performance of motions that prohibits speech and deliberation.

As the scenario of *The Human Condition* unfolds, then, animal laborans and homo faber emerge in conflict with each other but also allied against freedom and action (1958a, 144–53, 188, 210). Both the automatism of labor and the means—end instrumentality of work are symbolically opposed to, and normatively distinguished from, Arendt's action concept of politics. The symbolic oppositions that Arendt establishes as labor:work::action give her public realm theory much of its courageous character. But at the same time, Arendt's inability to conceptualize instrumental action within a richer situational context than homo fabricatus leaves her account of politics practically inadequate and vulnerable to what Weil calls "an unconditional surrender to caprice" (1973, 85).

THE THEATRE WHERE FREEDOM COURAGEOUSLY APPEARS

"As long as we believe that we deal with ends and means in the political realm," Hannah Arendt writes, "we shall not be able to prevent anybody's using all means to pursue recognized ends" (1958a, 229). Elsewhere she argues: "If we insist on applying the category of means and ends to action and human relationships, we shall see that everything comes to stand on its head. . . . A bad deed for a good cause instantly makes our common world a little worse" (1953, 597).

The alarm behind these words is neither trivial nor negligible. Out of the hubris of the modern age Arendt detects a conviction that has spawned a myriad of twentieth century horrors: man can know "only what he makes" (1958a, 228). The conviction can be historically located in Plato, "who was the first to design a blueprint for the making of political bodies" (pp. 227). What Plato enacted was a brilliant and devious conceptual maneuver that associated archein ("beginning") with "ruling" and eliminated prattein ("achieving"), thereby accomplishing a philosophical act of treachery against politics (pp. 188–89, 222–23). The essence of politics became knowing how

to rule. Thus, the polis became an object of order and manipulation, the perplexities of action were reduced to "problems of cognition," and the genuine plasticity of acting was hardened into the solidity of work and making. Consequently, Arendt writes, "the most elementary and authentic understanding of human freedom disappeared from political philos-

ophy" (p. 225).

When the conviction that "man is the measure," with its necessary degradation and objectification of all things, donned modern dress and invaded the realm of human affairs, politics became a means to be put to the purposes of particular ideological ends, into which history itself was drawn as consort. The prosaic dimension of a mentality "that forces one to admit that all means, provided that they are efficient, are permissible and justified to pursue something defined as an end" found inherent plausibility in the formulation of capitalistic economy (Arendt 1958a, 229). Its murderous dimension took shape in the Leninist line of argument ("You cannot make a table without killing trees, you cannot make an omelet without breaking eggs, you cannot make a republic without killing people") and reached its most virulent and horrific form in the totalitarian state (Arendt 1969, 139). This ideological orientation toward politics, with its ferreting out patterns in history, distortion of all things into means for the pursuit of allegedly higher ends, violent appeals to new orders and final solutions, and utter contempt for human personhood and individuality, is what Arendt's action concept of politics courageously attempts to overcome. Consequently, Arendtian public realm theory risks a theatrical politics that "lies altogether outside the category of means and ends" against an ideological politics as the pursuit of ends that justify all means (1953, 597; 1958a, 207).

Outside the category of means and ends, Arendt heroically stakes freedom in the condition of "plurality," or the common situation in which human beings reveal their "unique distinctiveness" through acting and speaking together (1958a, 176-77, 202). In the phenomenal space "where I appear to others as others appear to me," Arendtian politics recovers the world that the tremendous cosmos of modern automatism threatens to obliterate. The world is recovered in "the living essence of the person as it shows itself in the flux of action and speech" (Arendt 1958a, 181, 198). Daring to enter the space of appearances, every individual leaves behind the concern with life, and finds in the courage to speak a personal identity and specific uniqueness brought to light. In this condition only, one is not a what but a who—a name, a physical presence, a living essence, a character-in-voice—"distinguished from any other who is, was or will ever be" (pp. 175; see also pp. 179-81). The "intercourse of the citizens of the polis" is the theater in which mortal actors seize reality as unique and distinct speakers of words and doers of deeds (Arendt 1958a, 180; idem 1969, 222).

Consequently, politics is the space of appearances in which, as Arendt declares, the "inner affinity of

speech and action, their spontaneity and practical purposelessness" interrupt the "circular movement of biological life" and thwart the drive to calculate a trajectory of human affairs (1958a, 177, n. 1, emphasis mine; 1969, 43). In the name of this "inner affinity" that is freedom, an Arendtian politics bravely defies both the artificial, repetitive, cyclicalities of animal laborans and the durable, means—end lineations of homo faber. In Arendtian public realm theory, then, the polis is neither a nostalgic yearning nor a perverse Hellenic fixation. It represents metaphorically what "politics" constitutes literally—"a kind of theater where freedom could appear" (1969, 154).

If one of the markers of an emancipatory political theory is its capacity to speak meaningfully about individuals as social and political beings and not just isolated subjects against nature, then Arendt's theatrical performance concept of freedom has some considerable advantages over Weil's purposeful performance concept. Primarily, it carries with it a sure grasp of the emancipatory value of speech as interaction in plurality and a vivid presentation of politics as the "web of human relationships" in which such interaction is actualized through speech (Arendt 1958a, 178). In short, if "politics are wholly instrumental," as M. I. Finley insists (1983, 97), then they can only be instrumental and meaningful if they are conceptualized as a common human enterprise and against the background of something like plurality in an Arendtian sense. To the extent to which Weil bears allegiance to a post-Cartesian philosophy of consciousness, she can do none of this-neither conceptualize action intersubjectively, nor theorize speech as the activity of mutual recognition geared toward some kind of understanding, nor present politics in the context of genuine commonality. (In fact, Weil does not seem to consider politics a genuine human activity at all. It does not even appear on the list of activities she thinks should imitate work.)22 Interaction, in Weil's thinking, is at most the cooperation or the coordination of individualistic, disaggregated particulars, like her methodical builders. Speech, in Weil's thinking, seems to be at best a kind of "awkward substitute for sign language"; it is simply the vehicle through which the builders advance the solutions they have pondered in work (Arendt 1958a, 179). Consequently, Arendt's theory of action remains superior to Weil's-not only because it rescues politics within a context of interaction as speech but also because it makes freedom the raison d'être of politics itself (Arendt 1969, 154).

Yet in arriving at this powerful action concept of politics, Arendtian public realm theory also stands in danger of—in Weil's words—sinking into the "purely arbitrary" (1973, 85), for without a dimension of substantive purposefulness that finds positive expression in the vocabulary of "problem," "solution," "means," "end," and "method," Arendt's politics cannot embrace performance as the carrying out or active pursuit of purposes in the very world it strives to vitalize. Politics can encompass performance as public presentation, as in the act of performing a

play, a part, or a piece of music: the performing arts 'have indeed a strong affinity" with Arendtian politics (Arendt 1969, 154). But without a substantive purpose, Arendt's courageous political performer is constantly in danger of becoming only an actor, "concerned merely with the 'impression' he makes" (Weber 1946, 116). When this happens, the politics of the public realm threatens to become an unconditional surrender to caprice. Moreover, it leaves the theory behind it vulnerable to charges of aestheticism, sentimentalism, and self-defeat. Arendt's public realm theory is dangerously close to this surrender to the arbitrary and the capricious not because it rescues theatrical performance in the face of reductive and routinizing life processes but because it celebrates the "practical purposelessness" of speech and action and the transcendence of "mere productive activity" in politics as it does so (1958a, 177, 180).²³

By this I do not mean to redeploy the familiar claims that the Arendtian public realm is a talk-shop full of speechifying, "the plaything of an already educated elite," or a kind of game playing "set apart from the real world" (Elster 1983, 98; Schwartz 1989, 30, 41; Udovicki 1983, 56). Arendt in fact anticipates that in a society that celebrates the artifice of homo faber and the life of animal laborans, her concept of politics (and action) would probably be taken as a narcissistic tribute to "idleness, idle busybodyness and idle talk" (1958a, 208; see also pp. 159, 180). Accordingly, she is careful to assign certain a priori features to words and deeds (i.e., they are "about some worldly objective reality") and to action (i.e., it has a "specific content" and a "specific productivity") (1958a, 182, 187, 190-91). These comments obviously failed to innoculate Arendt's public realm theory against charges of irrelevance and elitism, but they indicate that she knew that politics were not simply acts of self-revelation-splendid, courageous, and immortalizing though they may be.

Still, the acknowledgements a theorist makes are one thing, and what the theory can conceptually vindicate is quite another. I am suggesting that Arendt's theory cannot conceptually vindicate either the emancipatory value of purposeful performance or the practical purposefulness of politics.24 We can trace this conceptual failure in part to Arendt's thinking about how homo faber works. Had she initially differentiated between homo fabricatus and homo methodus, she might have recognized that there is nothing necessarily violent, dangerous, or ideological about the category of means and ends in politics. Rather, everything hinges upon the action context within which that mode of thinking takes place. But by assuming that the category of means and ends is necessarily (and not just contingently) afflicted with violent objectification, Arendt has to deny (or at least evade) the normative dimension of the active pursuit of substantive purposes in the space of appearances. Thus, although means-end activity is not excluded from Arendtian politics, it is always and only a negative formulation tied to fabrication.²⁵ Ultimately, Arendt locates means-end activity in its least negative form in the "prepolitical" domain of legislation (1970, 208–10). In its most negative form, where it slides into something like automatism, it emerges in the "antipolitical" domain of administration (pp. 276–78). Accordingly, Arendtian public realm theory cedes the most important aspects of political activity as everyday life to charismatic founders on the one hand and administrative teams on the other. There must be another way to think about politics.

METHODICAL THINKING IN THE SPACE OF APPEARANCES

Earlier, in considering the means-end category in politics, I suggested that everything hinges upon the action context within which this mode of thinking takes place. I now want to suggest that there is a richer conceptual context-beyond utilitarian objectification, rational capitalist accumulation, and/or Leninism—within which to think about the category of means and ends. Weil offers this alternative in her account of methodical thinking as (1) problemoriented, (2) directed toward enacting a plan or method (solutions) in response to problems identified, (3) attuned to intelligent mastery (not domination), and (4) purposeful but not driven by a single end or success. Although Weil did not even come close to doing this herself, we might derive from her account of methodical thinking an action concept of politics. Methodical politics is equally opposed to the ideological politics Hannah Arendt deplores, but it is also distinct in important respects from the theatrical politics she defends.

Identifying a problem—or what the philosopher David Wiggins calls "the search for the best specification of what would honor or answer to relevant concerns" (1978, 145)—is where methodical politics begins. 26 It continues (to extrapolate from Weil's image of the methodical builders) in the determination of a means-end sequel, or method, directed toward a political aim. It reaches its full realization in the actual undertaking of the plan of action, or method, itself. To read any of these action aspects as falling under technical rules or blueprints (as Arendt tends to do when dealing with means and ends) is to confuse problem solving with object making and something methodical with something ideological. By designating a problem orientation to political activity, methodical politics assigns value to the activity of constantly deploying "knowing and doing" on new situations or on new understandings of old ones. This is neither an ideological exercise in repetition nor the insistent redeployment of the same pattern onto shifting circumstances and events. The problem orientation that defines methodical politics rests upon a recognition of the political domain as a matrix of obstacles where it is impossible to secure an ideological fix or a single focus.

In general, then, methodical politics is best understood from the perspective of "the fisherman battling

against wind and waves in his little boat" (Weil 1973, 101) or perhaps as Michael Oakeshott puts it: "In political activity . . . men sail a boundless and bottomless sea; there is neither harbour for shelter nor floor for anchorage, neither starting-place nor appointed destination" (1962, 127). Neither Weil's nor Oakeshott's is the perspective of the Platonist, who values chiefly the modeller who constructs his ship after pre-existing Forms or the pilot-philosopher who steers his craft to port by the light of immutable Forms fixed in a starry night. In both of the Platonic images (where the polis is either an artifact for use or a conveyance to safe harbor), a single and predictable end is already to hand. Neither Weil's nor Oakeshott's images admit any equivalent finality. The same is true of methodical politics, where political phenomena present to citizens—as the high sea presents to the sailor-challenges to be identified, demands to be met, and a context of circumstances to be engaged (without blueprints). Neither the assurance of finality nor the security of certainty attends this worldly activity.

In his adamantly instrumental reading of politics in the ancient world, M.I. Finley makes a similar point and distinguishes between a problem orientation and patterned predictability by remarking upon the "iron compulsion" the Greeks and Romans were under "to be continuously inventive, as new and often unanticipated problems or difficulties arose that had to be resolved without the aid of precedents or models" (1983, 53). With this in mind, we might appreciate methodical politics as a mode of action oriented toward problems and solutions within a context of adventure and unfamiliarity. In this sense, it is compatible with Arendt's emancipatory concept of natality (or "new beginnings") and her appreciation of openness and unpredictability in the realm of human affairs.

There are other neighborly affinities between methodical and theatrical politics as well. Both share a view of political actors as finite and fragile creatures who face an infinite range of possibilities, with only limited powers of control and imagination over the situations in which they are called upon to act. From both a methodical and a theatrical vantage point, this perpetual struggle that is politics, whatever its indeterminacy and flux, acquires meaning only when "knowing what to do and doing it" are united in the same performance (Arendt, 1958a, 223). Freedom, in other words, is realized when Plato's brilliant and devious conceptual maneuver is outwitted by a politics that opposes "the escape from action into rule" and reasserts human self-realization as the unification of thought-action in the world (pp. 223-25). In theatrical politics, however, the actual action content of citizen "knowing and doing" is upstaged by the spectacular appearance of personal identities courageously revealed in the public realm. Thus Plato's maneuver is outwitted in a bounded space where knowing what to do and doing it are disclosed in speech acts and deeds of self-revelation in the company of one's fellow citizens. In contrast, methodical politics doggedly reminds us that purposes themselves are what matter in the end, and that citizen action is as much about obstinately pursuing them as it is about the courage to speak in performance. So, in methodical politics, the Platonic split between knowing and doing is overcome in a kind of boundless navigation that is realized in purposeful acts of collective self-determination.

Spaces of appearances are indispensable in this context, but these spaces are not exactly akin to "islands in a sea or as oases in a desert" (Arendt 1970, 279). The parameters of methodical politics are more fluid than this, set less by identifiable boundaries than by the very activity through which citizens "let realities work upon" them with "inner concentration and calmness" (Weber 1946, 115). In this respect, methodical politics is not a context wherein courage takes eloquent respite from the face of life, danger (the sea, the desert), or death: it is a daily confrontation wherein obstacles or dangers (including the ultimate danger of death) are transformed into problems, problems are rendered amenable to possible action, and action is undertaken with an aim toward solution. Indeed, in these very activities, or what Arendt sometimes pejoratively calls the in order to, we might find the perpetuation of what she praises as the for the sake of which, or the perpetuation of politics itself (1958a, 154).

To appreciate the emancipatory dimension of this action concept of politics as methodical, we might now briefly return to the problem that Arendt and Weil think most vexes the modern world—the deformation of human beings and human affairs by forces of automatism. This is the complex manipulation of modern life that Havel describes as the situation in which everything "must be cossetted together as firmly as possible, predetermined, regulated and controlled" and "every aberration from the prescribed course of life is treated as error, license and anarchy" (1985, 83). Constructed against this symbolic animal laborans, Arendt's space of appearances is the agonistic opposite of the distorted counterfeit reality of automatism. The space of appearances is where individuality and personal identity are snatched from the jaws of automatic processes and recuperated in "the merciless glare" of the public realm (Arendt 1969, 86). Refigured in this fashion, Arendtian citizens counter reductive technological complexes in acts of individual speech revelation that powerfully proclaim, in collective effect, "This is who we are!"

A politics in this key does indeed dramatically defy the objectifying processes of modern life—and perhaps even narratively transcends them by delivering up what is necessary for the reification of human remembrance in the "storybook of mankind" (Arendt 1958a, 95). But these are also its limits. For whatever else it involves, Arendtian politics cannot entail the *practical* confrontation of the situation that threatens the human condition most. Within the space of appearances, Arendt's citizens can neither search for the best specification of the problem before them nor, it seems, pursue solutions to the problem once it is

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identified, for such activities involve "the pursuit of a definite aim which can be set by practical considerations," and that is *homo faber's* prerogative and so in the province of "fabrication," well outside the space of appearances where means and ends are left behind (pp. 170–71). Consequently, automatism can be conceptualized as a "danger sign" in Arendt's theory, but it cannot be designated as a problem in Arendt's politics, a problem that citizens could cognitively counter and purposefully attempt to resolve or transform (p. 322).

From the perspective of methodical politics, which begins with a problem orientation, automatism can be specified and encountered within the particular spaces or circumstances (schools, universities, hospitals, factories, corporations, prisons, laboratories, houses of finance, the home, public arenas, public agencies) upon which its technological processes intrude. Surely something like this is what Weil has in mind when she calls for "a sequence of mental efforts" in the drawing up of "an inventory of modern civilization" that begins by "refusing to subordinate one's own destiny to the course of history" (1973, 123-24). Freedom is immanent in such moments of cognitive inventory, in the collective citizenwork of "taking stock"-identifying problems and originating methods—and in the shared pursuit of purposes and objectives. This is simply what it means to think and act methodically in spaces of appearances. Nothing less, as Wiggins puts it, "can rescue and preserve civilization from the mounting irrationality of the public province, . . . from Oppression exercised in the name of Management (to borrow Simone Weil's prescient phrase)" (1978, 146).

Perhaps we have now gained some clarity on this picture of methodical politics, but a problem of value remains. Boards can be tools of violence, and so can boats. So the problem, simply stated, involves the ethical constitution of the common context within which methodical thinking takes place. What normative prerequisites are necessary in order to guarantee that methodical politics does not disintegrate after all into the ideological politics that Hannah Arendt identifies as objectifying others and rightly deplores? Against what kind of background can a public realm politics of means and ends, problems and solutions, be secured as methodical and ethical? Perhaps this quintessential problem of politics (how to judge rightly the lesser evil, the relatively best, the ends that justify the means) is, in the end, simply too much for Arendtian public realm theory to bearhence Arendt's ultimate evasion of the category of means and ends in politics altogether. Or perhaps (as some thoughtful scholars argue), Arendt's condition of plurality itself provides the ethical dimension that a truly emancipatory politics—even a politics of means and ends—requires.²⁸ I cannot answer this problem of value today. In reflecting upon it, I am drawn to the advice that Weber offers on a related problem in science: "If you remain faithful to yourself, you will necessarily come to certain final conclusions that subjectively make sense" (1946, 151). Of course, this probably does not suffice. In any case, I must simply acknowledge that for now we have come to the limits of methodical politics.

THE PLASTIC PEOPLE OF THE UNIVERSE

Even if it could be scrupulously formulated as free and ethical, a methodical politics of problems, plans, and purposes might still come to public realm theory at too great a cost. Ultimately, an Arendtian might argue, it threatens to reimprison human beings in the very existential "solidity" that theatrical politics (with its luminous sense of boundlessness, unpredictability, and irreversibility) is, in the first place, an attempt to overcome. It is true that one of Arendt's greatest accomplishments involves the rescue of politics from what Sheldon Wolin calls its "sublimation" into organization and its "absorption" into nonpolitical institutions and activities (1960, 352-53). The spatial and temporal phenomena that are set forth with atomic-age urgency in Arendt's space of appearances defy the solidity, relentless automatism, and patterned predictabilities that weigh down human existence in the modern world.²⁹ Humanness is, therefore, regained in the thrills, the highs, and the almost apocalyptic energy of spontaneous word and deed and in new beginnings (natality) that give rise to the totally unexpected.

That is why, when Arendt looks to history, she recaptures the "lost treasure" of action in those moments when politics is redeemed as the sudden, spontaneous release of numbers of people, whose "sheer momentum of acting-together" brings forth the cry for freedom: the American Revolution, the revolutionary councils that appeared in Europe in 1848, the Paris Commune in 1871, the "soviets" in Russia in 1917, the November revolutions in Germany and Austria after World War I, the revolt in Hungary in 1956, and the antiwar and Civil Rights movements in the United States (1958b, 4-5, 26-28). We can now confidently add to this list Charter 77, Worker's Defense Committee, Solidarity, Civic Forum, Public Against Violence, Tiananmen Square, a second Russian revolution, and the truly glorious spectacles of 1989: Warsaw, Prague, Budapest, Berlin. One can only wish that the curator of lost treasures had lived to witness them.

We do disservice to these moments of Arendtian action, however (and hence to politics itself) if we stake acting together solely on "sheer momentum" or the "spontaneity" of rare movements that burst out against the dark backdrop of modernity. To render as truly political only events that are public, spontaneous, and momentous is to underestimate the full complexity (and sometimes the brutality)³⁰ of human conduct in such events themselves—to see them, as Václev Havel puts it, "from the outside" and perhaps "chiefly from the vantage point of the system and its power structure" (1985, 49). Charter 77 "came as a

surprise" and appeared as a "bolt out of the blue", but, as Havel reminds us, it was neither a bolt out of the blue nor the result of a spontaneous political event. Its initial impetus was a small protest against the impending trial of the rock group The Plastic People of the Universe, whose music displeased the communist authorities. The protest began with a campaign planned in detail and with "modest, internal steps" that culminated in the signing of a petition by 70 people (Havel 1990, 130-38). The action group Charter 77 eventually emerged out of the opposition circles that the campaign for The Plastics had informally organized. As a document, the charter took form slowly during the late months of 1976, not in the merciless glare of the public but in what Havel calls "that semi-darkness where things are difficult to chart or analyse" (1985, 49). Its history has as much to do with the laborious organization of meetings, the meticulous crafting of language, the arduous collection of signatories, and the repeated drafting of copies of the original document as with the "explosion" that followed its release in the public realm. Even then, as Havel understands it, the charter was neither a "one-shot manifesto" nor by any means a prepolitical act of legislation but rather a commitment 'to participate in ongoing work" (1990, 139).

Like Havel and in the spirit of Arendt's instruction that we must "think what we are doing," I have been thinking about what it means to consider politics as a kind of ongoing, methodical work in the world. I raise the example of Charter 77 not to diminish the beauty of an Arendtian politics of spontaneity but in order to propose a public realm theory that is better able to coordinate political action as purposeful and hence open to a broader range of significance "in the whole way of life" (Weber 1946, 77). In thinking about the same sort of things, Havel warns that the global automatism of technological civilization poses a "planetary challenge" to the position of human beings in the world (1985, 90). If he is right, then those of us who take the project of emancipation seriously must do no less than face the challenge with all the means at our disposal and endeavor, in Simone Weil's words, "to introduce a little play into the cogs of the machine that is grinding us down" (1973, 121). As citizens, in other words, we must think methodically about what is to be done.

Notes

Many thanks to my colleagues Terence Ball and James Farr for their invaluable counsel and criticism on earlier drafts of this article, to Larry Biskowski for instructive discussions, and to my teacher Hanna Pitkin, who guided my studies of Hannah Arendt.

1. There are, of course, significant differences between Arendt and Habermas (see Habermas 1977; see also Benhabib 1986; Bernstein 1983; Honneth 1982; Villa 1992). Here I wish simply to underscore the distinctively modern commitments to action and an egalitarian politics of speech or communication (over and against work and instrumentalism) that Arendt and Habermas share as theorists of the public realm. None of this implies that public realm theory has nothing to gain from

numerous forms of contemporary critique, including Marxism, feminist theory, hermeneutics, poststructuralism, and studies of discourse, ethics, and culture.

- 2. It is important to note, however, that various critics of public realm theory have lodged important objections to both Habermas and Arendt's analyses of social labor and instrumental action. On Habermas, see Agger 1979; Eyerman and Shipway 1981; Giddens 1982; Honneth 1982; Keane 1975; McCarthy 1981; Winfield 1975. On Arendt, see Bakan 1979; Honneth 1982; Jay 1978; Parekh 1979.
- 3. Assessing Habermas' conceptual strategy is a more difficult proposition that I cannot address here. His early distinction between work and interaction (later reformulated into the dichotomy strategic/communicative action) is more ambiguous than Arendt's. He allows, for instance, that work can be called praxis, yet he also insists upon the irreducibility of work and interaction, or techne and praxis (1973, 158-59). Habermas has criticized Arendt for screening all "strategic" elements out of politics (1977, 16). Yet at the same time he subsumes strategic action, along with instrumental action, within the sphere of "work," or purposive-rational action, and dissociates the latter (which is based on rational decision and the instrumentally efficient implementation of technical knowledge) from communicative action (based on complementarity, reciprocity, and mutual recognition). An extended journey into this Habermasian morass must be deferred. A helpful attempt to sort out, if not resolve, some of the ambiguities can be found in McCarthy 1981, 16-40, and White 1988, 44-47.
- 4. I refer specifically to the Polish Solidarity movement, under the leadership of Adam Michnik, the great dissident and practical theorist who is now editor-in-chief of *Gazeta Wyborcza*, Poland's largest daily newspaper (personal conversation with Adam Michnik, University of Minnesota, Minneapolis, 1991). For Michnik's respectful view of Habermas, see Michnick and Habermas 1994).
- 5. Until recently, in fact, Weil's writings have been rather dimly lit in twentieth-century thought, overshadowed by those of her more luminous French colleagues and perhaps by the oddity of her own remarkable life and intractable personality, which have received much attention. Fortunately, sustained studies of her political and social thought have now begun to appear. See, among others, Blum and Seidler 1989; Dietz 1989; Nevin 1991; McLellan 1990; Winch 1989.
- 6. See Benhabib 1992; d'Entrèves 1989b; Honig 1992; Villa 1992. None of these commentators (although in some lively disagreements with each other about the "discursive," "communicative," "agonistic," and/or "dramaturgical" status of the Arendtian public realm) contest the strategy that conceptually dichotomizes work:interaction or instrumental:communicative. As a result, they collectively promote an Arendtian perspective on politics that is (1) theoretically constrained, because it presumes the inapplicability of instrumental action to an action concept of politics, and (2) practically constrained, because it leans in the direction of an expressivist (rather than a purposeful) orientation toward collective deliberation, citizen action, and democracy. Of course, Arendt's public realm theory is vulnerable to just this sort of expressivist interpretation and (mis)appropriation, and that is part of the problem.
- 7. Arendt (b. 1906) and Weil (b. 1909) both lived in Paris in the late 1930s. In the increasingly perilous atmosphere for European Jews, both emigrated to New York City (residing, at different times, on Riverside Drive). Weil stayed in the United States for only seven months, leaving for London in 1941. Despite their intellectual affinities and a shared friendship with the philosopher Jean Wahl, they traveled in different circles and apparently never met. There is no evidence to suggest that Weil, who died in 1943, knew anything of Arendt. Arendt (1958a) was familiar with some of Weil's writings. (Arendt's close friend, Anne Mendelssohn Weil, was not related to Simone Weil.)
- 8. Without question, the threat of totalitarianism, obvious to Weil in the 1930s—and its unprecedented horrors, which were to become the pressure point of much of Arendt's subsequent political thought—also shaped their shared sense

of "the process of world alienation" in the twentieth century (Arendt 1958a, 257). Yet it is also true that both of them understand totalitarianism within the context of technical progress, objectifying processes and dehumanizing institutions that distinguish late modern societies, rather than viewing these developments the other way around, as emanations of totalitarianism. Weil, for example, clearly associates the dissolution of thinking with technical domination and therefore can write, "It is quite unfair to say... that fascism annihilates free thought; in reality it is the lack of free thought which makes it possible to impose by force official doctrines entirely devoid of meaning" (1973, 119). A profoundly similar thesis informs Arendt's (1963) notion of the banality of evil in Eichmann in Jerusalem.

9. Weil's and Arendt's critiques of algebraic physics are matters of sufficient complexity to warrant study in their own right. Clearly, they share an implicit philosophy of psychology that is critical of attempts to reduce the mind to one or another aspect of a material world that can be fully described by the obvious facts of physics. What such descriptions leave out are phenomena that are irreducible to material explanations: thought and consciousness, perception, desires-in short, the intrinsically human world of the senses and appearances. Thus, a phenomenological (or at least perceptual) approach to reality informs both Weil's and Arendt's critiques of contemporary physics, and it is quite obviously carried through in their common admiration for geometry and the science of the Greeks, as well as their respective analyses of the history and social construction of science, the problem of objectivity (or the "Archimedean point"), and the violence that symbolic mathematics exerts upon "common sense" (see Arendt 1958a, 1969; Weil 1968). Thanks to one of her graduate students, Arendt was apparently familiar with Weil's essay "Reflections on Quantum Theory": she mentions it briefly in *The Human Condition* (1958a, 287–88, n. 53).

10. Philosophers refer to this operation as following the "rules of syntax," according to which someone could be trained to do something automatically, using a set of rules, tables, and symbols, without understanding the meaning (or following the "rules of semantics") of the operation as a whole. Although the implications of such syntactical operations have spawned a great deal of philosophical debate in the computer age, Weil obviously anticipated them in 1933 and addressed them within the context of social and political life. Along these same lines, see Arendt's remarks on the logical processes of "the newly invented electronic machines" and their "worldlessness" (1958a, 172) and Havel's discussion of how "a formalized language deprived of semantic contact with reality" replaces reality with pseudoreality (1985, 32–33).

11. This is how Weil characterized "Reflections Concerning the Causes of Liberty and Social Oppression" (Weil 1973), the long essay she wrote in 1934, following a year of hard labor in various French factories located within the "Red belt" outside Paris.

12. Arendt calls the theater "the political art par excellence" because it is "the only art whose sole subject is man in his relationship to others" (1958a, 188; 1969, 154). The allusion to theater calls up the importance of dramatic display in Arendtian politics. It represents the notion of a space wherein human beings appear before each other—each as a uniquely distinct character—and enact a performance that ultimately becomes a part of "the storybook of mankind," a part of theater-history (1958a, 184). In the society of animal laborans, we might imagine not only mechanized petrification but also terribly dull theater.

13. With Weil's indebtedness to Greek philosophy in mind, I use the term methodus to convey the Greek met hodos (meta-after + hodos-way) and the Oxford English Dictionary's "a way of doing anything, esp. according to a defined and regular plan; a mode of procedure" (1.3). Methodus does not correspond to OED II "a systematic arrangement, order." The latter, as Sheldon Wolin argues, is a usage that originated within the disciplines of logic and philosophy in the sixteenth century (1972, 32–38) and places more emphasis on tech-

nique-oriented activity and scientific objectivity than I wish to convev.

14. Rather surprisingly, McLellan also contends that Weil "saw work as inherently servile, . . . as permitting not an embodiment, but a transcendence, of the human condition" (1990, 72). This is quite at odds with the argument in "Reflections Concerning the Causes of Liberty and Social Oppression" (Weil 1973), where she conceives of work as an activity through which the world is disclosed, not transcended. In essays and notebooks written later, however (especially in response to an intensifying mystical turn), Weil emphasizes "the spirituality of work": "Work makes us experience in the most exhausting manner the phenomenon of finality rebounding like a ball. . . . It is when man sees himself as a squirrel turning round and round in a circular cage that, if he does not lie to himself, he is close to salvation" (1952b, 158; see also Dietz 1989, 122–25).

15. I am indebted to Winch's distinction between Weil's "thought-action account of freedom," in which "the point of the action lies in the character of the action itself," and a "desire-satisfaction account of freedom," where "the point of the action depends on its outcome" (1989, 84-85). Arendt also pursues a thought-action account of freedom and criticizes utilitarianism for its "innate incapacity to understand the distinction between utility and meaningfulness" (1958a, 154, 225). As we shall see, however, the dimensions of Arendt's account are significantly different from Weil's.

16. Weil recognizes that conceiving of a method and applying it are two different operations. Even in an emancipatory context, where the agent elaborates and executes the method or "plan of action" at issue, it may be "impossible to combine the examination of these difficulties with the accomplishment of the work" simultaneously (1973, 91–92; Arendt [1958a, 92] makes a similar point). But freedom is only in jeopardy when the elaboration of a method and its execution are decisively divided, and method transfers "its abode from the mind into the matter"—with which the subject works but over which he or she has no mental control (Weil 1973, 92).

17. Weil herself admits, for instance, that "in reality, there is nothing in common between the solution of a problem and the carrying out of an even perfectly methodical piece of work, between the sequence of ideas and the sequence of movements" (1973, 91).

18. Beginning with the seafaring Athenians, sailing becomes a commonplace reference in the discourse and rhetoric of Western political thought. The focus, however, is almost always on a ship (not a boat), which serves as an analogue for the city, or the constitution of the state. As Martha Nussbaum notes, "The city-ship, in the tradition of the image, is something safely water-tight, a barrier against imminent external dangers" (1986, 59). In Weil's image (and in another I shall consider later), the focus is on the action context of sailing and hence upon the sheer complexity of a practical task—in which the boat is not a ship, a barrier against imminent dangers, or a locus of stability in roily waters but rather the means through which dangers are encountered, with a greater or lesser measure of success.

19. In an age of electronic signals, automatic pilots, and computerized navigational instruments, this metaphor, too, loses its meaning. For an instructive parallel to a Weilian perspective on methodical thinking as a human form of navigation with mere "boards" to hand, see William Finnegan's fine essay concerning the legendary surfer "Doc" Mark Renneker of San Francisco and the "ineffable skill" of "wave judgment" in surfboarding the pounding waters of Ocean Beach off Sloat Boulevard (1992b, 46).

20. Feminist critiques of Arendt's political thought have generally overlooked her gendering of the vita activa, where we find the oppositional alliance between animal laborans (gendered female), and homo faber (gendered male) set against the public realm and the condition of plurality. Under these terms, Arendtian politics may be interpreted as (1) an overcoming of the gendered alliance rooted in being and subjectivity through the actualization of doing and interaction and (2) a destabilization of the symbolic order of feminine/mascu-

line through the proliferation of distinctive, individual voices that are not what's (i.e. representations of class, race, gender) but whos (uniquely distinct personalities). There are, I think, some potentially radical implications here for current feminist discussions concerning the politics of identity.

21. Arendt seems to grasp this differentiation when she writes, "The issue at stake is, of course, not instrumentality, the use of means to achieve an end, ... but rather the generalization of the fabrication experience [as] the ultimate standard[s] for life and the world of men" (1958a, 157). But she proceeds to characterize instrumentality as "the experiénce of ends and means, as it is present in fabrication" (ibid.). So although she initially differentiates instrumentality in work from instrumentality as a generalized phenomenon, she nevertheless reverts to a concept of instrumentality that is wholly informed by her (negative) concept of work.

22. Winch does not overstate when he argues that Weil's theory of action views "the individual and his or her thinking in uncompromisingly individualistic terms" (1989, 99). Weil argues, for example, "Several human minds cannot become united in one collective mind, and the expressions 'collective soul', 'collective thought', so commonly employed nowadays, are altogether devoid of meaning" (1973, 82). At a later stage in her work, when she turns more toward ethos, habituation, and the "need for roots" in order to formulate the content of free and ethical life, Weil's negative view of collectivities relaxes (Weil 1952a). But to say that she arrives at a concept of emancipatory dialogic interaction would be a gross exaggeration. For a discussion of Weil's antipolitics, see Dietz 1989 and Ignatieff 1990.

23. On a similar point, see Knauer, who also argues that "Arendt's account of the vita activa needs reformulation as a theory of democratic praxis" (1985, 187-90). Knauer's useful reformulation of praxis posits a dialectical relation among labor:work:action that, he argues, Arendt herself recognized but did not adequately develop. Thus "the task is to illuminate the ways in which beings gathered in political community could choose to live their lives together as workers and laborers . . . so as to nurture political community" (p. 190). By infusing an action dimension into labor and work, Knauer humanizes Arendt's categories and translocates action (politics) as "political community." The problem with this approach, however, is that it collapses action into labor and work and then calcifies Arendt's concept of politics as a communitarian normative ideal. It thereby evades precisely what needs to be examined, namely, Arendt's concept of politics as a concrete, practical activity.

24. In his thoughtful essay, Lawrence Biskowski argues that there is substantive purpose in Arendt's politics, which is "inherently connected to care for the world, not only for what the world thinks . . . but for what the world will be like in the wake of one's acting" (1993, 880). Thus we can find substantive ethical content for Arendtian politics in the doctrine of amor mundi (p. 878). In speech and action, citizens keep alive the space of appearances itself. I agree with Biskowski's claim that the concept of "world" that informs Arendt's understanding of politics carries substantive ethical content. What I am interested in here, however, is the substantive cognitive content of Arendtian politics. Since Arendt identifies cognition as "the pursuit of an aim which can be set by practical considerations" and aligns it with homo faver, fabrication, and scientific processes (1958a, 170-71), I think we have to conclude that cognition plays a very small role in Arendtian politics. Despite its ethical purpose, then, I want to maintain that Arendt's politics is inadequate as a cognitive purposeful enterprise.

25. As Knauer (1985) and d'Entrèves (1989b) correctly note in criticizing Jay (1978), Arendt does not exclude instrumentalism from politics. But my point is that instrumentality is always formulated negatively. What Arendt and her commentators seem to share is a congealed concept of instrumental action that cannot distinguish instrumentality as utilitarian objectification from instrumentality as purposeful performance. This is clear in Knauer's rhetorical query, "Why should [Arendt] emphasize the instrumental aspect of all

politics when her aim was to overcome its instrumentalization and trivialization?" (1980, 733). Also see d'Entrèves's observations that action is constrained and hampered by instrumental concerns that can never be fully "eliminated" and that politics is emancipatory only when it is able to "transcend mere instrumental concerns" (1989b, 333–35, emphasis mine).

26. Along these lines, Wiggins seeks to recover a dimension of deliberation over ends in Aristotelian practical reasoning. Aristotle, he argues, "has been incorrectly interpreted or translated . . . as saying that deliberation is not of ends. He is always prepared to describe practical reason . . . in its concern with the question what objectives a man is to form (in general, of here and now), and what particular concerns he is to put first, in the light of how the Good appears to him" (1978, 150). With this argument in mind, we might understand Weilian methodical thinking (despite her odd dislike of Aristotle) as fully in keeping with some aspects of Aristotelian phronesis, although elaborating the linkages is beyond my scope here.

27. I hasten to add that although I find this metaphor for political activity appealing, I do not share Oakeshott's commitment to a "tradition of behavior" as the locus for political life. The notion rests uneasily with the technicization of modern life that has, with tragic consequences, forged a tradition of its own and supplanted precisely those traditions that Oakeshott wants to recover. The best critique of Oakeshott's politics remains Pitkin 1974.

28. There is a wide array of views concerning the ethical dimension of Arendt's politics. Knauer (1980), Hinchman (1984), Issac (1989), and Biskowski (1993)-in various waysmake strong cases for locating an ethical commitment to human dignity, mutuality, and civility in Arendt's condition of plurality and concept of world. Kateb (1984), Jay (1978), and Villa (1992)—in various ways—make strong cases against it. I shall not attempt a consideration of this debate here. Nor is this the place to undertake the philosophical task of justifying the reconciliation of instrumental action with dialogic interaction. For a general discussion (especially as the argument relates to Habermas), see Rasmussen 1990.

29. These repetitious and relentless patterns are also the domain in which "questionnaires and motivation research" and other "gadgets in the arsenal of the social sciences" operate (Arendt 1958b, 8). Arendt's phenomenological rescue of action must, therefore, be understood as a form of resistance against a world in which the theories of behaviorism "could actually become true" (1958a, 322).

30. In a recent symposium on intellectuals and social change in Central and Eastern Europe, Doris Lessing noted revolutionary politics, the house committees, the vigilante slogans, are intoxicating drugs" (1992, 727). She also recalled one European commentator's remark about "demonstrations that seem to have little point . . . from the point of view of actually achieving something. . . . The ends are not the point. The means are the point" (ibid.) These remarks serve to remind us that the politics of revolutionary, theatrical spontaneity are as potentially susceptible to meaninglessness and excess (including murderous excess) as the politics of "ends" and ideology that Arendt powerfully condemns. Indeed, these are often elements of the same complex of experience. Compare the violence of Bucharest's revolutionary politics in 1989, for example, to the velvet revolutionary politics of Prague, or even Berlin.

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PATRICIDE AND THE PLOT OF THE PRINCE: CESARE BORGIA AND MACHIAVELLI'S ITALY

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n understanding of Machiavelli's assessment of Cesare Borgia in The Prince is essential for interpreting his view of politics, but the ambiguity of that assessment has led to vastly different conclusions about Machiavelli's political teaching and Cesare's significance. We approach Machiavelli's ultimate intentions through a consideration of his more immediate concern for Italy. Machiavelli's great interest in Cesare and his criticism of this potential hero stem from the historical context of an Italy divided due to the Church. Cesare possessed—yet squandered—an opportunity to rid Italy of the evils plaguing it by killing his father, Pope Alexander, and by eliminating the College of Cardinals. Machiavelli's suggested denouement to the plot of The Prince is an assault on the ecclesiastical power. He invites his reader to contemplate the vulnerability of the Church and to act where Cesare and others shrank. Machiavelli ultimately counsels us to break our reliance on God or fortune and thus create the conditions for a reinvigorated civil life.

achiavelli confronts his readers in *The Prince* with an ambiguous portrait of Cesare Borgia. This ambitious son of Pope Alexander VI—this ruthless new prince of the Romagna—indisputably occupies a great deal of Machiavelli's attention in the work. Indeed, Machiavelli offers Cesare as an instructive model: "I do not know what better teaching I could give to a new prince than the example of his actions" (1985, 27). Ultimately, however, Machiavelli's assessment of Cesare's achievement is mixed. Despite his outstanding *virtù*, in the end Cesare failed in his enterprise due to "an extraordinary and extreme malignity of fortune": surprised by the death of his father and his own illness, Cesare made an unfortunate choice in supporting the election of Julius II, the future warrior pope (pp. 32–33).

The prominent yet uncertain role of Cesare in *The* Prince is at the center of debates over Machiavelli's political theory. More than one reader of The Prince has been led by the relish with which its author relates the murderous deeds of the Borgias to conclude that Cesare is Machiavelli's hero, simply. "Machiavelli was full of his idol, Duke Valentino," declares Montesquieu (Spirit of the Laws 29.19). An actual prince eager to prove his humane credentials, Frederick of Prussia, indicts Machiavelli by saying Cesare is "the model on which the author forms his prince" and his "hero" (1981, 58; see also A. Gilbert 1938, 42). This conclusion requires revision in light of Machiavelli's own criticism of his exemplar (Sasso 1966, chap. 14). Some interpreters have therefore argued that Cesare, whatever his shortcomings, is Machiavelli's idealized "man of virtù" (Hulliung 1983, 192) or the model for his own method of statecraft (e.g., Butterfield 1962, 97-98, see also Skinner 1981, 8-12, 33-34). Conversely, others have seen Machiavelli's attention to Cesare as evidence that The Prince is a satire. "The choice of his execrable hero is in itself enough to make manifest his hidden intention," Rousseau states, claiming that "The Prince is the book of republicans" (Social Contract 3.6 and n.; see also Mattingly 1958, 487–91). Cesare's place in The Prince thus raises questions about Machiavelli's intentions in that work and the relationship of his handbook for princes to his apparently more republican writings, especially the Discourses.

As political scientists or historians of political thought, we are ultimately interested in Machiavelli because of the general bearing of his thought and its influence, including his status as perhaps the first truly "modern" political thinker. Nonetheless, Machiavelli did write in the context of sixteenth century Italy. Just as it is a mistake to read *The Prince* as essentially historically bound, so too is it an error to interpret it without reference to Machiavelli's surroundings and his more immediate intentions. We approach Machiavelli's ultimate intentions by concentrating on his more immediate ones in *The Prince*: his concern for an Italy overrun by "barbarians" and kept divided due to the papacy.

We argue that Machiavelli's concern with the Church's ruinous influence on Italy is the source of both his attention to Cesare and his criticism of this potential hero. Although we differ with Rousseau on the issue of whether Machiavelli's interest in Cesare is sincere, we agree with him that the Church is Machiavelli's target in *The Prince*: "The court of Rome has severely forbidden his book. I can well believe it; it is the court that he most clearly depicts" (Social Contract 3.6, n.).3 Because Cesare was perfectly situated to eliminate the power of the Church, he possessed—yet squandered—an opportunity to rid Italy of the evils plaguing it and thus to realize the grand achievement called for in the concluding chapter of The Prince. By following the development of the plot of The Prince and its protagonist, we uncover Machiavelli's advice for remedying Italy's ills. Like his play

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Mandragola (a comedy of conspiracy), The Prince has a plot.

Unfolding in a chilling drama, Machiavelli's plot involves the conquest of the ecclesiastical principality of Rome, a special type of state to which he devotes a chapter of The Prince. The court of Rome has a peculiar mode of rule where the pope's authority is derived from the cardinals and that of the cardinals from the pope. The complete acquisition of this state would require the elimination of both the pope and the College of Cardinals. Machiavelli intimates that Cesare could have done just that: had Cesare acted more prudently, "he could have kept anyone from being pope (poteva tenere che uno non fussi papa)" (p. 33 [Machiavelli 1971, 268]; emphasis added). Cesare, however, failed to do this. To employ Machiavellian terminology from a similar context, Cesare was bad, but he failed because he was not altogether bad (see Discourses 1.27). Machiavelli approves with studied coolness the crimes the Duke committed in his ascent to power. We argue that for Machiavelli, Cesare's lapse was a sin of omission: he failed to commit a simultaneous homicide and patricide. Machiavelli's suggested dénouement to the plot of The Prince represents an assault on the power of the Church and of Christianity generally. Machiavelli thereby inspires one to contemplate the vulnerability of an inimical ecclesiastical power. In countenancing the most extreme measures to eradicate it, he counsels someone who possesses both the virtue and the fortune to act where Cesare and others shrank-whether that person be the immediate addressee of The Prince, Lorenzo Medici, the nephew of the pope at that time, or the ultimate audience that Machiavelli indicates-

"whoever understands it" (p. 61).

After briefly discussing the historical context to which Machiavelli's immediate intentions in *The Prince* are a response, we follow the plot of *The Prince* through chapter 11, "Of Ecclesiastical Principalities." Our solution to the puzzle of Machiavelli's use of Cesare Borgia not only reveals the coherence of a work many readers find disjointed at first appearance (e.g., Allen 1960, 452; see also Skinner 1981, 23) and ascribes a significance to passages usually deemed insignificant, but also links the darkest, most sinister Machiavellian intent with his concern for Italy. Finally, we address how our interpretation speaks to the vexing problem of the relationship of Machiavelli's republicanism and patriotism to his role as an advisor to princes.

THE PRINCE AND MACHIAVELLI'S ITALY

The historical context in which Machiavelli wrote undoubtedly influenced his thought, but questions abound as to the manner in which and the extent to which it did so. It can be said with certainty that historical context dictated the way in which he presents his thought and provided many of the examples

he uses to expound it, notably that of Cesare Borgia. In order to understand Machiavelli's more immediate concerns in The Prince and thereby to comprehend his ultimate teaching, knowledge of Machiavelli's Italy is essential. Machiavelli is concerned with Italy's divisions and vulnerability. A number of scholars have sought to uncover Machiavelli's remedy for Italy's ills (or at least his native Florence's), sometimes with attention to how Cesare Borgia's enterprise—the creation of a large northern Italian state with the aid of his father, the pope—is in some way his model for that remedy. The interpretations offered by these scholars are, however, inadequate because they are not radical enough. Because they do not reckon with what Machiavelli indicates is the pervasive cause of Italy's woes—the papacy and its policy—these interpretations do not grasp the extremity of the remedy he prescribes. By examining these interpretations and then turning to Machiavelli's treatment of the papacy, we can establish the magnitude of the problem Machiavelli apprehends and make more plausible the extreme remedy we argue he contemplates.

Italy and Machiavelli's Prince

Numerous scholars have approached Machiavelli from a historical perspective, but in steering clear of lamentable ignorance of Machiavelli's Italy they have sometimes underestimated their author. Mary Dietz has done a great service in her own attempt to solve the problem of The Prince by reading Machiavelli with his historical situation in mind while at the same time redirecting our attention to the deceptiveness of the theorist of deception. Dietz argues that Machiavelli's work is itself a work of deception, a trap into which he intends Lorenzo Medici to fall: "Machiavelli devises a plot, a series of moves that, if followed, will lead Lorenzo to disaster" and Florence to a renewed embrace of its republican institutions (1986, 781). Dietz's application of Machiavelli's praise of the tactic of fraud to his own writing is well founded. For example, in a letter to Guicciardini of 17 May 1521, quoted by Dietz, Machiavelli declares, "For a long time I have not said what I believed, nor do I ever believe what I say, and if indeed sometimes I do happen to tell the truth, I hide it among so many lies that it is hard to find" (Machiavelli [1989, 2:973]). No longer able to play the lion, he adopts the nature of the fox. Machiavelli is indeed "outspoken, irrepressible, and fearless," as Hulliung insists (1983, 28), but his aims in *The Prince* themselves require subterfuge.

Although Dietz suspects something sinister, she identifies the wrong plot. To begin with, the scope of her investigation is too limited. Granting even that it was Machiavelli's intention to mislead the Medici to their downfall, a reconstituted Florentine republic would still have been vulnerable to the dealings of its neighbors. The republic for which Machiavelli had served as a loyal envoy had, after all, fallen in 1512 as a result of the French defeat at the hands of the Holy League, led by Pope Julius II. The viability of the

republic was therefore dependent not only on the organization of Florence but also on that of Italy. Our consideration of *The Prince* reveals that Machiavelli had precisely this problem in mind. Moreover, Dietz takes no account of Machiavelli's offer of Cesare Borgia as a model to be imitated. Surely looking into this mirror of princes would not reveal the vulnerable prince that Dietz suggests would result from Machiavelli's advice.

Attempts have been made to untangle the puzzle of The Prince with reference to its Italian context and the model of Cesare Borgia. Most notably, Hans Baron argues that Machiavelli wrote his plea in the concluding chapter of The Prince to "redeem" Italy in reaction to "a unique occasione" that offered itself to the Medici through their simultaneous hold on Florence and the papacy. Baron suggests that the request is made for the Medici to expel the barbarians "by founding, in accord with the rules established in the book, a strong new state modeled on the one built a decade earlier by Cesare Borgia as a power nucleus on the borders of north and central Italy-a historic enterprise which only misfortune had prevented from becoming the starting point for Italy's liberation" (1991, 85). Whitfield makes a similar argument and concludes that Cesare Borgia "represents the only historical parallel, the proof of how much one can build upon the favour of a pope" (1969, 28; see also Whitfield 1965, 62-64).

Baron and Whitfield mistake the occasione that Cesare's example reveals. The client state that Alexander VI was carving out through Cesare was an insufficient remedy for the illness diagnosed by Machiavelli. First, because of the brevity of the average pope's reign and the alternating creation of pontiffs from competing factions—points Machiavelli emphasizes in *The Prince* with reference to Cesare (pp. 32, 46)—any alliance of the papacy and a northern buffer state was certain to be ephemeral. As the head of the papal forces, Giuliano Medici was in the same position as Cesare, but he would be at the mercy of his uncle's successor in the same way that Cesare relied on the bad faith of his father's successor. Second,

Cesare Borgia in the manner Baron or Whitfield suggest. We shall show that Machiavelli's concern with the Church in connection with Italy's woes is present from the beginning of *The Prince* and unifies its development.

Machiavelli's Treatment of the Papacy

The most important aspect of the historical context of *The Prince* for understanding Machiavelli's more immediate intentions in *The Prince* (as well as his ultimate aims) is his view of the influence of the Church on Italy and the problem of Christianity more generally. In *The Prince*, Machiavelli writes that Italy has been "subjected to barbarous cruelties and insults" of the French and Spanish armies and the mercenary forces of the Swiss and Germans (pp. 102, 104–5). In the *Discourses*, as well as in the *Florentine Histories*, he blames the papacy for the political division of Italy, the very problem he laments at the end of *The Prince*:

The Church has kept and still keeps this region divided. . . . The reason why Italy is not in that same condition and why she too does not have one republic or one prince to govern her is the Church alone; because, though she has dwelt there and possessed temporal power, she has not been so strong or of such ability that she could grasp sole authority [occupare la tirannide] in Italy and make herself ruler of the country. Yet on the other hand she has not been so weak that, when she feared to lose dominion over her temporal possessions, she could not summon a powerful man to defend her against anyone who in Italy had become too powerful. (1.12 [Machiavelli, 1971, 96]; see also Florentine Histories 1.9)

Machiavelli discusses the temporal power of the Church in *The Prince* at several points. He explains that Louis XII of France erred "by giving aid to Pope Alexander so that the pope might seize the Romagna" and by failing to realize that he was thus "making the Church great by adding so much temporal greatness to the spiritual one that gives it so much authority" (p. 14). Later in the same work, Machiavelli observes of the Church that "before Al-

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believed, but the one I have told of" (p. 49).⁵ The Church is primarily responsible for this condition: "Since Italy had almost fallen into the hands of the Church and a few republics, and since the priests and the other citizens did not have knowledge of arms, they began to hire foreigners" (p. 52). Citizens can be armed, and Machiavelli dedicated himself to that end in Florence and throughout his writings, but the clergy is always unarmed and at the mercy of others (see p. 81).

Machiavelli expands on the Christian clergy in connection with the political corruption of the moderns in his Discourses. When he refers to those who administer to the pope as a "court" (1.12), he establishes that the members of the clergy constitute a type of noble class. Later in the same work he speaks of a type of gentleman (gentiluomo) particularly inimical to "any political life," those "who without working [oziosi] live in luxury on the returns from their landed possessions," and points his reference when he lists the Papal States (Terra di Roma) as one place where such gentlemen can be found in abundance. He ominously reports that the Germans kill these gentlemen when they find them (1.55 [Machiavelli 1971, 138]). Machiavelli challenges anyone who may disagree with his analysis of the responsibility of the "Roman court" for Italy's ills to send this court to Switzerland. He concludes in advance of the experiment that such largess on the part of the Italians would in a very short time create similar disorders even among the Swiss, who, of all modern peoples, live in a manner most akin to the ancients (1.12). Christianity has disarmed a noble class, thereby rendering it useless to life by making it dependent on other arms.

In the Discourses, Machiavelli states that "we Italians" have as "our first debt to the Church and to the priests that we have become without religion and wicked" (1.12, see also 1.27). Some readers have argued that this passage is informed by Machiavelli's firm foundation in the Christian faith and concern that the clergy be reformed (e.g., Grazia 1989, 89-90; cf. Parel 1992, 62). Nevertheless, Machiavelli indicts not just the Church for Italy's woes but Christianity for the ills of modern states generally. The Christian religion "has made the world weak and turned it over as prey to wicked men, who can in security control it, since the generality of men, in order to go to Heaven, think more about enduring their injuries than about avenging them" (Discourses 2.2). Machiavelli holds out as his utmost standard the exaltation of his earthly homeland (see Machiavelli to Vettori, 16 April 1527 [1989, 3:1010]). A Christianity reformed to his specifications would cease to be recognizably Christian (see Berlin 1980, esp. 46-50; Hulliung 1983, 67). Machiavelli's view of Christianity itself strengthens his indictment of the papacy and precludes a strong Church from being the remedy for Italy's condition.

Machiavelli's discussion in the *Discourses* of the effects of Christianity refers to "modern" peoples generally. He indicates the pervasive influence of Christianity on modern times in *The Prince* when he

relates his conversation with Rouen: "For when the cardinal of Rouen said to me that the Italians do not understand war, I replied to him that the French do not understand the state, because if they understood they would not have let the Church come to such greatness" (pp. 15-16). Louis's fault was his faith: (1) the "faith" Louis might be said to have owed Alexander "for dissolving his marriage and for the [cardinal's] hat of Rouen," faith Machiavelli explicitly urges Louis to break by referring the reader to his discussion of faith later in the work (pp. 15-16 and chap. 18, esp. p. 70); and (2) his faith more generally, which Machiavelli implies we should break. The power of Christianity over the modern mind is pervasive, and Machiavelli must make us see the religion as both inimical and vulnerable if we are to break our faith with it and adopt his "new orders" (see pp. 23-24, 61).

Healthy politics in Italy and modern times more generally would require as a first step the destruction of the power of the Church, if not its total elimination. Machiavelli does consider the possibility of the murder of a pope and the destruction of the "Roman court." In the Discourses he reports that when Pope Julius II undertook to expel the tyrants who had seized the land of the Church, he entered Perugia to remove Giovampagolo Baglioni. Machiavelli first comments on the rashness of Julius (see 1985, 100-101), who entered the city entirely unarmed while his enemy maintained troops there. But the tyrant Baglioni receives Machiavelli's harshest criticism. Baglioni's "cowardice" permitted the unarmed pope to leave Perugia with his prey. Machiavelli complains: "All the sagacious [prudenti] men with the Pope. could not reckon whence it came that [Baglioni] did not, to his everlasting fame, at one stroke [ad un tratto] put down his enemy and enrich himself with booty, since with the Pope were all the cardinals with all their precious things." Machiavelli is indeed shocked at Baglioni's uncharacteristic omission of a sin from conscience: "So Giovampagolo, who did not mind being incestuous and an open parricide, could not or, to put it better, did not dare, when he had a perfect opportunity [occasione] for it, do a deed for which everybody would have admired his courage and for which he would have left an everlasting remembrance [perpetua fama] of himself. . . . And he would have done a thing the greatness of which would have transcended every infamy, every peril that could have resulted from it" (Discourses 1.27 [Machiavelli 1971, 109-10]; see also Machiavelli 1985,

The greatness of the reward that would accrue from this terrible deed—the deed from which even Baglioni shrunk—suggests the significance of the elimination of the papacy for Machiavelli. Yet his characterization of Baglioni's potential reward does not appear to accord with an earlier remark in the Discourses. Whereas "heads and organizers [ordinatore] of religions" are "most famous" (laudati) of all human beings, "those men are infamous and detestable who have been destroyers of religions, squan-

derers of kingdoms and republics, enemies of virtue" (1.10 [Machiavelli 1971, 91]). Baglioni's "cowardice" would be indictable, but surely the murderer of a pope and his entourage would seem to merit "eternal infamy" rather than "perpetual fame"—unless, of course, the act not only destroys but also founds. Machiavelli suggests just this conclusion in his Florentine Histories when he relates Stefano Porcari's ambition: animated by a desire for glory, the Roman Porcari "judged he could do nothing else than to try to see if he could take his fatherland from the hands of prelates and restore it to its ancient way of life, hoping by this, should he succeed, to be called the new founder and second father of [Rome]" (6.29). Porcari's "mode" entailed killing the pope and calling the disaffected people to arms against the Church. Porcari failed in his conspiracy (see Discourses 3.6), and Machiavelli comments, "The intentions of this man could be praised by anyone, but his judgment will always be blamed by everyone because such undertakings, if there is some shadow of glory in thinking of them, have almost always very certain loss in their execution" (Florentine Histories 6.29). Machiavelli blames only Porcari's failure to acquire, not his aim. "Machiavelli more than hinted that the time had come for a political takeover of the church,' Hulliung comments; "Where Stefano Porcari and Cesare Borgia had failed, others could succeed" (1983, 217). Hulliung thus intimates the solution, although he fails to indicate the means to that end as it comes to light in *The Prince*.

The person who removes the religious blight from Italy would provide the possibility for the growth of healthier political institutions. As Machiavelli repeatedly emphasizes in many contexts, the very act of founding or reforming requires horrifying deeds. Most prominently, Machiavelli's first example of a prince by virtue, Moses, had to commit terrible acts and, even if Machiavelli only implies it, eliminate a former religion (Discourses 2.5, 3.30; see, esp., Exodus 32:27-28). Machiavelli's other instances of the greatest exemplars are similarly guilty. Romulus, for example, committed fratricide and consented to the death of his colleague, Titus Tatius. Romulus must be forgiven, however, because his object was to found un vivere civile (Discourses 1.9 [Machiavelli 1971, 90]; see also 1.18, 3.1).8 Cruelties "well used" are necessary to establish political order and vibrancy. Perhaps if Baglioni had had a similar end in view, his deed of papacide, like that of Romulus's fratricide (or Theseus' possible patricide), would have earned him fame rather than infamy. Borgia had the same opportunity as did Baglioni and Porcari; if he had seized it, Machiavelli would certainly rank him beside Romulus and the others as "one of the most excellent of princes."

THE PLOT OF THE PRINCE

Machiavelli discusses his greatest exemplars—new princes who acquire through their "own arms and virtue"—in chapter 6 of The Prince. Those who acquire through "others' arms and fortune"—the theme of chapter 7—pale in comparison. Yet Machiavelli offers these princes, notably Cesare Borgia, as examples to be imitated. Unlike the wholly or partially mythical exemplars of chapter 6, Cesare was of flesh and blood. Hulliung suggests that Cesare is "an abstraction Machiavelli created by taking elements from both Greek and Roman thought and compounding them into a creation all his own" (1983, 192). We insist upon the historical Cesare as the key to grasping Machiavelli's more immediate intentions in The Prince. Through Cesare's example Machiavelli instructs a prince not only not to rely on the arms and fortune of others but also how to solve Italy's political problem.

As we have shown, the politics of Italy in particular and of modern times in general was decisively affected by the Church. In order to understand Machiavelli's remedy for Italy, one has to read The Prince from the very beginning with the discussion of ecclesiastical principalities in chapter 11 in mind. Wolin is therefore incorrect when he states that "Machiavelli contended that ecclesiastical governments were irrelevant to the proper concerns of the new science" he was developing and that they were "not politic enough to warrant the attention of political thought" (1960, 198-9). The chapter on ecclesiastical principalities does indeed appear to be almost an afterthought, since the ecclesiastical state of Rome does not fit into Machiavelli's initial categorization at the outset of *The* Prince and is not included there. This appearance is not inadvertent, however, and the anomalous position of that discussion is actually the culmination of an argument that develops in the previous chapters. We shall now make the plot of The Prince manifest.

The Conquest of Italy

Chapter 1 of The Prince begins with what Machiavelli claims is an exhaustive enumeration of different sorts of states, which can be categorized generally as republics or principalities. In chapter 2, after setting aside "reasonings on republics because I have reasoned on them at length another time" (presumably in the Discourses), Machiavelli commences with a discussion of hereditary principalities. Hereditary states do not occupy Machiavelli for long since, he claims, "if such a prince is of ordinary industry, he will always maintain himself in his state unless there is an extraordinary and excessive force which deprives him of it." His example of such a prince, the duke of Ferrara (actually two dukes), already alerts us to the existence of the papacy, for according to Machiavelli, this duke did not succumb to the attack of either Venice (a republic) or Pope Julius II (pp. 6-7). This prince or these princes actually did lose their state, however, and were fortunate to be restored by others. Although Machiavelli follows tradition by calling hereditary princes "natural" in this context, there is perhaps something more "natural" about new princes who acquire states, because, as

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Machiavelli informs us, the desire to acquire is a "very natural and ordinary thing" in a world marked more by change than by stability (pp. 14-15; cf. Florentine Histories 5.1; see also Pocock 1975, 158; Strauss 1958, 57).

Machiavelli turns in chapter 3 to the epochal event that changed the landscape of Italian politics—the invasion of Italy by the French in 1494 and again in 1499. Although the chapter is entitled "Of Mixed Principalities," it is actually an account of Louis XII's failure, through his 1499 invasion, to create a mixed state by adding much of Italy to his own kingdom. Machiavelli offers Louis advice on how he could have succeeded—or rather, since Louis was dead, he counsels anyone who would conquer Italy. He offers the same advice in his Discourses (1.23), hardly an act of simple patriotism. Mansfield suggests an explanation: "Much as he loved Florence and Italy, he is not fundamentally a city or national patriot. He is a patriot on behalf of humanity, seeking to protect men against outside forces, consequently a patriot of the home of human beings, the earth" (1981, 303). While Mansfield's explanation is ultimately in accord with our own, it does not account for Machiavelli's more immediate intentions as revealed in this context. Butterfield embraces the opposite extreme, seeing Machiavelli's advice to Louis as nothing more than a compendium of maxims of statecraft (1962, 30-31, 60). Machiavelli's concern with the historical dilemma confronting Italy is in fact the unstated theme animating chapters 3 and 4 of The Prince.

Louis entered France with the aid of the Venetians and, most importantly, with the collusion of Pope Alexander. Machiavelli thus reminds us of the reason Italy was subject to such divisions and invasions—the papacy. He speaks of the division of Italy in the chapter on ecclesiastical principalities, although he does not there reveal the cause of the division: "Before Charles, king of France, came into Italy, this province was under the dominion of the pope, the Venetians, the king of Naples, the duke of Milan, and the Florentines" (pp. 45-46). As noted, chapter 3 is purportedly on "mixed" principalities but is actually about Louis's failure to conquer Italy. Italy is similar to France, where "Burgundy, Brittany, Gascony, and Normandy" have "been with France for so long a time" (p. 9). 10 Like France, Italy itself is a "mixed" or "disparate" province (see p. 11). Despite this similarity, Italy would appear to differ from France in one important respect, namely, the residence of the pope (at least after the end of the "captivity" of Avignon). If chapter 3 contains Machiavelli's advice on how to acquire a mixed province like France or Italy, he expands his plan of conquest with implicit reference to the papacy in the next chapter.

The subject of conquest continues in the apparently anomalous chapter 4, whose announced subject is, "Why the Kingdom of Darius Which Alexander Seized Did Not Rebel from His Successors after Alexander's Death." The conquest of Darius' kingdom was maintained because it was governed in a particular "mode," namely, by one prince whose

ministers owe their power to him rather than by a prince surrounded by barons with hereditary privileges and their own subjects. Machiavelli states that 'principalities of which memory remains" have been governed through one of these two modes. He presents a contemporary parallel to Darius' kingdom through the government of the Turk and contrasts it with that of the king of France. He explains: "Whoever considers the one and other of these states will find difficulty in acquiring the state of the Turk, but should it be conquered, great ease in holding it. So inversely, you will find in some respects more ease in seizing the state of France, but great difficulty in holding it" (pp. 17-18). Machiavelli advises one how to acquire both the state of France and that of the Turk. We have already seen that he considers Italy to be like France, so his remarks on the conquest of France would apply to Italy as well, with the notable

exception of the papacy.

The papacy, in turn, is similar to the government of the Turk. Machiavelli has already furnished us with the information necessary to categorize the papacy when he remarked in the previous chapter that one reason Louis kept faith with the Pontiff was that Pope Alexander had undertaken to elevate one of Louis's ministers to cardinal (p. 15). The pope makes his own ministers, who therefore have no independent source of rule, just like the ministers of the Turk. The result of this comparison would be heartening to anyone contemplating an attack on the papacy: like the dominion of the Turk, this ecclesiastical principality is difficult to conquer but easy to hold after the initial assault. Machiavelli comments that once the Turk has been defeated, "one has only to fear the blood line of the prince" (p. 18). This statement, however, points to the manner in which the papacy must be distinguished from the Turk's state and Darius' kingdom of old: the papacy does not have a (recognized) blood line. The pope's authority derives from election. Later in The Prince, Machiavelli states that the sultan's state shares this feature with "the Christian pontificate" (p. 82). (Further, in this context, Machiavelli groups the Turk and the Sultan because of their dependence on their armies rather than on the people, a feature that the Christian pontificate shares in its dependence on mercenary arms.) The Christian pontificate is similar to the states of both the Turk and

Now in the case of Pope Alexander, Cesare Borgia himself represented the blood line, and so the pontificate under Alexander was more similar to the government of the Turk. Because his father ruled from the Vatican, however, Cesare was not the apparent heir to his father's rule. Indeed, this very circumstance would necessitate that Cesare, the natural son of the pope, conquer his father's principality in order to rule it. Burckhardt, for example, detects in Cesare's actions a plan to succeed his father as pope and to secularize the lands of the Church (1958, 129-30). Machiavelli draws our attention to the peculiar situation of the pope and his son at the very end of chapter 3, when he is relating a discussion with

Rouen that took place "when Valentino (for so Cesare Borgia, son of Pope Alexander, was called by the people) was occupying Romagna" (p. 16). Immediately after making this remark about Alexander and Cesare, Machiavelli turns in chapter 4 to Alexander the Great and his successors, causing us to think of the possible successor of a different Alexander from the one mentioned (twice) in the title of chapter 4. Alexander the Great seized the kingdom of Darius, and his successors held it; Alexander Borgia attained the papacy, but his successor never gained possession of it.

Machiavelli provided a plan in chapters 3 and 4 of The Prince to conquer the whole of Italy, both the part resembling France and that resembling the sultan and the Turk. Cesare was more successful in conquering the part of Italy akin to France—eliminating the nobles and thus gaining the people to himself, "since they had begun to taste well-being" (p. 29), 12 an accomplishment to which Machiavelli appears to refer at the end of chapter 3 when he relates that Cesare was called Valentino by the people. (Actually, he was Duke Valentino, or Valentinois, as a vassal to the French king.) However, he did not succeed in acquiring his father's principality, a task that would have required overcoming those features of the papacy that make it similar to the Sultan and Turk's states. Cesare had the rare opportunity to accomplish the deed Machiavelli suggests, but he failed because he did not grasp the character of his father's realm or the deeds necessary to acquire it.

Finally, in chapter 5, Machiavelli offers advice on how to administer "cities or principalities" that lived under their own laws: "In truth there is no secure mode to possess them other than to ruin them"—or, he adds, to live in them (pp. 20–21). Such is the course Cesare would have had to take with Florence or any other republic in Italy, although we shall take up the relationship between princes and republics at greater length later. Machiavelli's plan for the conquest of Italy is complete by the end of chapter 5.

Virtue and Fortune, Crime and Opportunity

The play between virtue and fortune animates *The* Prince as a whole, and it is in chapters 6 and 7 of the work that the contrast comes to life. Speaking of "principalities that are altogether new in prince and in state," Machiavelli alerts us in chapter 6 that he will speak of "the greatest examples." He exhorts "a prudent man" to imitate such examples "so that if his own virtue does not reach that far, it is at least in the odor of it." Although the greatest examples have relied on their own virtue and arms, fortune or opportunity is also needed. Machiavelli writes of his great examples that "their excellent virtue enabled the opportunity to be recognized," but maintenance of the acquisition requires virtue above all. Among such princes, Machiavelli avers that "the most excellent are Moses, Cyrus, Romulus, Theseus, and the like" (pp. 21-23). As the founder of both a religion and a state, a combination Machiavelli esteems most highly (see Discourses 1.10), Moses is perhaps the most excellent of princes. Machiavelli initially claims to be reluctant to speak of Moses-just as he later states that "it would be the office of a presumptuous and foolhardy man to discourse" on ecclesiastical principalities (p. 45)-before proceeding to do just that. "And although one should not reason about Moses, as he was a mere executor of things that had been ordered for him by God, nonetheless he should be admired if only for that grace which made him deserving of speaking with God"-who, Machiavelli laconically remarks, was "so great a teacher" (pp. 22-23). What God taught Moses is unclear, but what Machiavelli seems to admire about Moses is his having relied on his own arms. He contrasts Moses to Savonarola, who also claimed to speak with God but was an "unarmed" prophet and therefore unsuccessful (p. 24). In order to understand Moses' accomplishment, as well as the power of the ecclesiastical dominion of Rome (a dominion founded by Jesus, a successful unarmed prophet, thus requiring us to amend Machiavelli's estimation of such prophets [see Berlin 1980, 64]), Machiavelli indicates that we must transcend our usual pieties and look at the "effectual truth of the thing.

Machiavelli provides, with chapter 7, "Of New Principalities That Are Acquired by Others' Arms and Fortune," the antistrophe to his discussion of princes who acquired through their own virtue and arms. Cesare Borgia serves as his primary example. "Those who become princes from private individual solely by fortune become so with little trouble," Machiavelli begins the chapter, "but maintain themselves with much" (p. 25). We see then that new principalities acquired by fortune are like France, which is easy to enter but difficult to hold, and, as we might expect, those acquired by virtue are like the Turk: those "who become princes by the paths of virtue, acquire their principality with difficulty but hold it with ease" (p. 23). We are reminded at the outset of the chapter concerned with Cesare Borgia of the discussion in chapter 4 of how Alexander's successors succeeded in holding the state he acquired. 13 Machiavelli briefly adduces Francesco Sforza as an example of a prince who acquired "with a great virtue of his own" but passes straightaway to Duke Valentino, so-called "by the vulgar" (thus reminding us of chapter 3). Machiavelli introduces Cesare by saying that he "acquired his state through the fortune of his father and lost it through the same, notwithstanding the fact that he made use of every deed and did all those things that should be done by a prudent and virtuous man to put his roots in the states that the arms and fortune of others had given him." Machiavelli claims, "I do not know what better teaching I could give to a new prince than the example of his actions" (pp. 26-27). Cesare is presented as an example but not one of the greatest kind. Again we are faced with the question of what "deed" Cesare omitted or what he could have done to raise himself to Machiavelli's highest

Cesare's career flourished under the sponsorship

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of his father, who "decided to make his son the duke great." By facilitating the entry of the French into Italy, Alexander upset the existing "orders" and created the "disorder" necessary for his undertaking. Cesare thus acquired the Romagna, which served as the base for his subsequent acquisitions. Machiavelli praises in particular the Duke's decision "to depend no longer on the arms and fortune of others," thus dispensing with "auxiliary arms" (the French) and then mercenary ones (the Orsini) in favor of his own (pp. 27-28, see also p. 55). In this connection Machiavelli praises Cesare's turn to deceit to eliminate those on whom he had formerly depended and savors the Duke's elimination of these "heads" at Sinigaglia, the action that also "gained all those peoples to himself since they had begun to taste well-being." ¹⁴ He points to this action as "deserving of notice and of being imitated by others" and admires in particular the manner in which Cesare brought order to the Romagna through his minister, "Messer Remirro de Orco, a cruel and ready man, to whom he gave the fullest power." After Remirro succeeded in reducing the province to peace "with the very greatest reputation for himself," Machiavelli claims that Cesare judged that such excessive authority might become "hateful," and "in order to gain [the people] entirely to himself, he wished to show that if any cruelty had been committed, this had not come from him but from the harsh nature of his minister." Cesare therefore had Remirro killed in such a way that "the ferocity of this spectacle," Machiavelli comments, "left the people at once satisfied and stupefied" (pp. 28-30). Cesare was thus able to avoid being hated while retaining the people's love and fear, in accordance with Machiavelli's advice in chapter 19 of The Prince.

Machiavelli relates the Duke's deeds admiringly, but when he comes to Cesare's plans to maintain his acquisitions, the account becomes critical. Cesare's ultimate failure was that despite his resolve, he never dispensed with his reliance on the fortune of others. Machiavelli notes that Cesare had foremost to fear "that a new successor in the Church might not be friendly to him and might seek to take away what Alexander had given him" and speaks of the four "modes" in which Cesare sought to secure himself against this: eliminating the blood lines of those he had despoiled, winning over the gentlemen in Rome to keep the pope in check, making "the College of Cardinals as much his as he could," and acquiring sufficient "empire before the pope died that he could resist a first attack on his own." He almost succeeded in these plans, according to Machiavelli, and then "would no longer have depended on the fortune and force of someone else, but on his own power and virtue." His fatal mistake was that he used his influence in the College of Cardinals to make Julius II pope (pp. 30-32). Machiavelli's discussion reveals generally that Cesare erred by continuing to rely on his alliance with the Church. He relates in his legations that Cesare supported Julius, despite the injuries he had done him, because of Julius' assurance

that he would appoint him as papal general, continuing the relationship with the papacy that he enjoyed under his father (1989, 1:155; see Guicciardini 1969, 174–6). Cesare kept faith with Julius, but according to Machiavelli, Cesare not only again relied upon the fortune of others but deceived himself (p. 33; see Discourses 3.4).

What should Cesare have done? Machiavelli indicates a more promising strategy in his relation of Cesare's failure. First, Machiavelli states of Cesare that "if he could not make pope whomever he wanted, at least it would not be someone he did not want" (p. 32). Initially, it appears that Machiavelli indicts him merely for his choice, but his restatement of Cesare's position a page later reveals another dimension: "One could only indict him in the creation of Julius as pontiff, in which he made a bad choice; for, as was said, . . . he could have kept anyone from being pope" (p. 33). Sasso notices that this is not what Machiavelli had said initially and concludes that the difference between the statements lies in the second's emphasis on the poor use Borgia actually made of his influence (1966, 147). Sasso is correct in characterizing the second as more negative; however, as we have argued, it also reveals what, in Machiavelli's view, Cesare should have done. Machiavelli says that Cesare could have kept anyone from being pope. This sentence can obviously be interpreted to mean that he could have kept any one candidate from being pope, but on another reading it signifies that Cesare could have ended the papacy altogether.

Machiavelli continues to reveal his plot in chapters 8 and 9, which parallel chapters 6 and 7, respectively. Chapter 8 concerns those who have attained a principality through "crimes," although Machiavelli equivocates on whether these "crimes" are actually acts of virtue. For instance, his first example in the chapter is Agathocles of Sicily, about whom Machiavelli says that "whoever might consider the actions and virtue of this man will see nothing or little that can be attributed to fortune," much like the greatest examples of chapter 6. Having spoken of Agathocles' virtue, Machiavelli retracts his attribution—"Yet one cannot call it virtue to kill one's citizens, betray one's friends, to be without faith, without mercy, without religion; these modes can enable one to acquire empire, but not glory"—only to grant it again: "For, if one considers the virtue of Agathocles . . . " (p. 35; emphasis added). Machiavelli later distinguishes what are "called" virtues and what virtue actually requires, with particular emphasis on keeping faith (pp. 61–62 and chap. 18, esp. 70–71).

Numerous scholars have taken Machiavelli's temporary refusal to call Agathocles virtuous to be his final opinion and have thus palliated or obscured the full force of the revolution he proposes in morality (see Pitkin 1984, 60–61; Skinner 1978, 1:137–38; see also Tarcov 1982, esp. 705–7). Machiavelli neither divorces politics from morality (as suggested by, e.g. Chabod 1964, esp. 243–55; Croce 1945, 250–56; Figgis 1960, 94–121) nor embraces half-heartedly or com-

pletely a political morality in tension with an unrejected Christian morality (as suggested by Berlin 1980), nor proposes an "economy of violence" (as suggested by Wolin 1960, chap. 7; see also Orwin 1978, 1225). His ultimate objection to Agathocles would appear to be to what he calls "his savage cruelty and inhumanity," his failure to employ necessarily violent means toward ultimately humane ends (p. 35). There are "cruelties badly used or well used," Machiavelli comments later on concerning Agathocles: "Those can be called well used," (if it is permissible to speak well of evil) that are done at a stroke [a uno tratto], out of the necessity to secure oneself" and are "turned to as much utility for the subjects as one can" (pp. 37-38 [Machiavelli 1971, 270]). He later praises Cesare for such cruelty as was actually "merciful" (p. 65). Machiavelli does indeed speak well of "evil." He announces at the beginning of his discussion of virtues, "I depart from the orders of others" (p. 61), and his departure entails a wholesale revision in the concept of virtue (see, esp., Orwin 1978; see also F. Gilbert 1977, chap. 2). When we appreciate the magnitude of his project, we are in a position to take seriously the claim that the "crimes" of Agathocles and others may potentially be acts of virtue.

Machiavelli's other primary example in his chapter on those who attain a principality through crimes is Liverotto (or Oliverotto) da Fermo, and it is Liverotto who serves as a partial pattern for what Machiavelli implies Cesare ought to have done. Machiavelli begins his account of Liverotto's history by calling attention to the parallel he will establish between Liverotto and his "father" on the one hand and Cesare and Alexander on the other: "In our times, during the reign of Alexander VI, Liverotto da Fermo, having been left a fatherless child some years before. was brought up by a maternal uncle of his" (p. 35). Machiavelli specifies that the events he relates here took place during Alexander's reign even though most of the modern events recounted in The Prince take place during that same time without his calling attention to the fact. After becoming an accomplished condottiero, Liverotto returned to Fermo and there "he held a most solemn banquet to which he invited Giovanni Fogliani," his uncle, "and all the first men of Fermo." Seemingly recalling his description of Cesare's conspiracy at Sinigaglia, Machiavelli relates, "Liverotto, with cunning, opened serious discussions, speaking of the greatness of Pope Alexander and of Cesare Borgia, his son, and of their undertakings." Again, our attention is drawn to the parallel. Adjourning to a more secret place, Liverotto had his uncle and the others killed and then seized power in the city. Machiavelli at first calls this act a "homicide," but later speaks of "the parricide he committed," and he does so when relating how Liverotto was strangled one year after his crime by Cesare at Sinigaglia (pp. 35–37; see also Guicciardini 1970, 234). Machiavelli forces us to compare Liverotto and Cesare and thereby suggests that Cesare should have seized his patrimony through patricide (see also Pitkin 1984, 60-63).

Papacide and the College of Cardinals

In Cesare's case, however, Liverotto's example alone could not suffice. Patricide would result only in the election of a new pope. The cardinals must also be eliminated at the time of his father's death. The cardinals comprise a sort of nobility surrounding the pope. Machiavelli takes up the question of how to deal with the nobility in chapter 9 of The Prince, "Of the Civil Principality." Just as chapter 8 on crimes is parallel to chapter 5 on virtue, so Machiavelli indicates at the outset of chapter 9 that it is parallel to chapter 7 on fortune: in a civil principality, "neither all virtue nor all fortune is necessary to attain it, but rather a fortunate astuteness." The chief lesson of this chapter is that in every city there are two "diverse humors," the "great" and the people: "A prince can never secure himself against a hostile people, as they are too many; against the great, he can secure himself, as they are few." Similarly, "one cannot satisfy the great with decency and without injury to others, but one can satisfy the people" (p. 39). Machiavelli thus concludes that "when a prince who founds on the people knows how to command and is a man full of heart . . . and with his spirit and his orders keeps the generality of people inspired, he will never find himself deceived by them and he will see he has laid his foundations well" (p. 41). Machiavelli has already noted several times that Cesare was founding himself on the people and opines that "his foundations were good" (p. 32). One way to deal with nobles is to destroy them. For example, in the Discourses Machiavelli relates how Clearchus, "finding himself between the arrogance of the aristocrats, whom he could in no way satisfy or control, and the rage of the citizens," took a suitable opportunity to solve his problem "at one blow" (a un tratto): "He cut to pieces all the aristocrats—to the utter satisfaction of the people" (1.16 [Machiavelli 1971, 100]; see also Langton 1987, 1280). Cesare was accomplished at eliminating the great, as Machiavelli's macabre admiration of his exploits at Sinigaglia shows. What Cesare needed to do was to eliminate another group of "heads," the cardinals. Machiavelli does not, of course, specify that the destruction of the clerical aristocracy would, in this way, result in a more stable rule for a prince. Nevertheless, it appears justified to speculate that the destruction of a class of people "who without working [oziosi] live in luxury on the returns from their landed possessions" would satisfy the people (*Discourses* 1.55 [Machiavelli 1971, 138]). 15 The elimination of the cardinals was the only way that Cesare could maintain his rule.

As the son of the pope on whom his father's ambitions were focused, Cesare had enviable access to the College of Cardinals. Indeed, Guicciardini reports that Alexander's death resulted from an accidental poisoning at a dinner that he and Cesare hosted for a number of cardinals. Alexander "had arranged a large garden party, at which he planned to poison a few cardinals so that he could then sell their offices and their benefices." Cesare and Alexander

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arrived early and asked for some wine to relieve their thirst. They were served the poisoned wine inadvertently: "That this is true is attested by the fact that he died either the same night or the next day; and by the fact the Valentino and a few others who were there fell into a long and critical illness, with signs of poisoning" (Guicciardini 1970, 240–41; see also idem 1969, 165–6). 16

Machiavelli attributes Cesare's downfall to "an extraordinary and extreme malignity of fortune." He relates that Cesare himself told him on the day that Julius II was created "that he had thought about what might happen when his father was dying, and had found a remedy for everything, except that he never thought that at this death he himself would also be on the point of dying" (pp. 27, 32). As it stands, Machiavelli's report of Cesare's comment is simply misleading (see Mattingly 1958, 488). He in fact lived another three years and died in battle! Furthermore, after the college's first choice as pope, Pius III, died only a month after his election, Cesare was vigorous enough to exert his influence with the Spanish cardinals to assure Julius's ascendancy (Machiavelli, 1989, 1:143, 149; see also Guicciardini 1970, 245). 17 According to Machiavelli's analysis, for Cesare to succeed he had to exert independence not only from his father but from the Church. Machiavelli appears to acknowledge the impossibility of Cesare's coexistence with a new pope: "He could not make a pope to suit himself" (p. 33). The lamentation that Machiavelli ascribes to Cesare appears to be his own analysis, rather than that of the historical Cesare, for in his legations, written contemporaneously with these events, Machiavelli characterizes the Duke as incapable, at the time of Julius' ascension, of the astuteness required for this insight (1989, 1:146). Only because Cesare had tied his fortune to that of his father can he be said to be "on the point of dying" at that time. Cesare deceived himself not only about Julius' faith but also about himself. Ultimately, like Baglioni, Cesare is to be censured because he lacked the insight to capitalize on his criminal character and thus to

WHAT MACHIAVELLI RENDERS UNTO CESARE

Machiavelli's criticism of Cesare Borgia's reliance on the fortune of others is related to his treatment of fortune and virtue more generally. If Guicciardini can celebrate the downfall of the Borgias by acknowledging the ultimate "justice and power of God, whose boundless might cannot be contained within the narrow limits of the present" (1969, 166), Machiavelli's interest lies elsewhere. He opposes the opinion that "worldly things are so governed by fortune and by God, that men cannot correct them with their prudence" (p. 98). Machiavelli's ultimate intention is to manage—if not to conquer—fortune through the power of virtue. Christianity has made the world weak, Machiavelli claims (Discourses 2.2). His project requires reasserting human autonomy by cutting a chain of instrumentality that is seen by Christian eyes to stretch from God through earthly powers to all people.

The case of Cesare Borgia presents both the power of the chain of dependence Machiavelli would break and the possibility of doing so. Cesare himself gave his minister, Remirro, "the fullest power" to pacify the Romagna. 18 When Remirro became dangerous Cesare had him eliminated, leaving him "one morning in the piazza at Cesena in two pieces, with a piece of wood and a bloody knife beside him" (pp. 29-30; see Machiavelli 1989, 1:141-42). In recounting this harrowing tale, Machiavelli imparts a lesson regarding instrumentality. Cesare did well in this instance by manipulating his minister. Machiavelli makes it clear that Remirro's deeds in fact belonged to Cesare. However, later we find that according to Machiavelli's analysis, Cesare is not the master manipulator but is himself manipulated. Just as Remirro's deeds belonged to Cesare, ultimately Cesare's deeds belonged to his father. Later Machiavelli clarifies Cesare's position when delineating Alexander's deeds: "With Duke Valentino as his instrument and with the invasion of the French as the opportunity, he did all the things I discussed above in the actions of the duke"

ecclesiastical principalities "subsist by superior causes" and "are exalted and maintained by God" (p. 45). Ultimately, then, God stands at the head of this chain of instrumentality. Cesare finds himself near the bottom of a rather considerable chain of instruments, and as a result he ends in a position no better than that of his own minister. As Machiavelli writes in his first Decennale, "When Alexander was slain by Heaven [ciel], the state of his Duke of Valence was broken and divided into many pieces" (1989, 3:1455 [Machiavelli 1971, 948]). The debate over the relative power of the temporal and spiritual powers that so occupied his immediate predecessors was certainly pernicious, as Machiavelli makes clear in his account of the tumult in Florence due to the struggle between the Guelfs and Ghibellines (see Florentine Histories, esp. bks. 2-3). For Machiavelli, however, this debate is ultimately strictly secondary in importance. The positions of both the spiritual and the secular powers, as well as those who would mediate them, all ultimately founded themselves on a theory that power is derived from God (see Gierke 1987, esp. 7–20, 30–32). As long as one is forced to render unto Caesar what is Caesar's and to God what is God's, human beings cannot enjoy a true civil life. Machiavelli ushers in modern politics by suggesting that a new prince break the chain of instrumentality through his own arms and virtue. Cesare Borgia had the opportunity to do just that, and Machiavelli offers him as an example to one who would not omit the necessary acts. Fortune must be broken on virtue's wheel.

MACHIAVELLI'S POLITICAL VISION

After Machiavelli divulges the last crucial elements of his plot in chapter 11 of The Prince, the theme of Italy emerges in the succeeding chapter when he informs us that the Church has disarmed the country and made it weak. Italy's fate depends on the plunderous whims of others. The Prince concludes with the stirring exhortation for the redemption of Italy from its humiliation at the hands of foreigners: "Left as if lifeless, she awaits whoever it can be that will heal her . . . of her sores that have festered now for a long time. One may see how she prays God to send her someone to redeem her from these barbarous cruelties and insults" (p. 102). In the Discourses, Machiavelli argues that it is the Church that "keeps this region divided" and adds that "truly no region is ever united or happy if all of it is not under the sway of one republic or one prince, as happened to France and to Spain" (1.12). We have seen that the same line of reasoning informs his considerations in *The Prince*. The redemption of Italy requires its unification and therefore the destruction of the Church.

So great do some commentators find the disjunction between the impassioned patriotic plea of chapter 26 and the analytic ruthlessness of the treatise's previous chapters that they conclude that the last chapter is not in fact a part of the original work (e.g., Baron 1991). Others note this disparity but claim that

this apparent gulf must be bridged: his overt immorality must be read in light of his patriotism. Now, having unraveled its plot, we see precisely what Machiavelli's patriotism must serve to forgive (see Strauss 1958, 81): the rectification of Cesare's errors would require the commission of unspeakable crimes.

If the theme of the ruinous effects of the Church on temporal affairs unites The Prince and the Discourses, what are we to make of the more general and intractable problem of Machiavelli's declared preference for republics on the one hand and his willingness to advise princes on the other? Viroli, for example, presses the division between the two works to the point where he denies that The Prince is even about politics: "Princely rule, be it hereditary or newly founded, cannot in any sense be equated with the civitas, and the art of preserving princely rule does not coincide with the art of instituting or preserving a vivere politico;" in The Prince, he claims, Machiavelli "was not writing about politics as he understood the term" (1990, 161). This position is simply too extreme. For example, in his work supposedly on republics, Machiavelli confirms that Romulus—a single man and a king—established a vivere civile ("civil life," Discourses 1.9). Machiavelli does not limit the term vivere civile to republics, for Romulus introduced kingship, not republican government, to Rome. A vivere politico can occur "by way of republic or by kingdom" (1.25 [Machiavelli 1971, 109]; see 1.55). Machiavelli thus does not preclude the possibility of a political life emerging from a state with a single head. Indeed, many scholars have paid heed to Machiavelli's claim that it is necessary to be alone to found a republic (e.g., Wolin 1960, 231-32).

Dietz is skeptical about whether this princely rule, or "heroic politics," will "somehow 'give way' to mass politics" (1986, 780–81). She neglects considerations that would, while affirming her understanding of Machiavelli's pessimistic view of human nature, nonetheless lead to another conclusion. Even from the Discourses it is unclear that Machiavelli desires the emergence of "mass politics," as such. Although he favors the Roman republic, which expanded effectively because it allowed the plebeians a voice in the regime, Machiavelli is quite careful not to expunge princely or heroic rule from his depiction of this exemplary republic. For example, he includes among the benefits of a free way of life the knowledge that one's children "by means of their abilities . . . can become princes [principi]" (Discourses 2.2 [Machiavelli 1971, 150]). Not only does he insist upon calling the leading men of the Roman republic "princes," but he shows how the devious maneuverings of these leading men kept the mass of citizens from exercising control in the regime (1.47-48, 3.11). Moreover, he shows how a regime can overcome the problem endemic and dangerous to republics, that of slowness to act (1.59, 3.6), by infusing itself with the resoluteness, even the despotic character, of princely regimes (3.1). Machiavelli does not consider princes and republics to be completely separate or even contradictory in nature.

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The deed of founding a republic need not be as altruistic an act as Dietz suggests. Machiavelli holds out the promise of fame to the one willing to undertake the travails of such a founding (1.9). The vigor of such a republic serves to exalt the memory of its original legislator. Machiavelli suggests that founding-and refounding-harnesses the tyrannical impulses of leading men. Further, the reflection that a founder of a free way of life must arrogate all authority to himself is far from unusual in political thought. Rousseau, whose credentials as a republican or democratic theorist are, to say the least, less assailable than those of Machiavelli, follows his Florentine predecessor in regarding the lone legislator as essential for the foundation of a free state. Like Machiavelli, the Citizen of Geneva holds out fame as the reward for legislators.

The notion informing much scholarship that Machiavelli's treatments of republics and of princes are analytically wholly distinct is simply incorrect and has placed unnecessary obstacles in the way of ascertaining Machiavelli's political vision. Machiavelli calls on a prince with both the virtue and fortune to remedy Italy's ills and thus to provide the conditions necessary for a reinvigorated political life in Italy and modern times more generally. Our interpretation of Machiavelli's more immediate intentions in *The Prince* is therefore also a first step needed for a consideration of his ultimate intentions.

If one needs one's own arms and virtue to establish a new principality, Machiavelli laments his fortune and his lack of arms. Yet armed with his pen he may address a virtuous prince who also possesses the opportunity and arms, whether an actual prince or those who deserve to be princes. Machiavelli is an "unarmed prophet" who unabashedly announces in The Prince, as well as in the Discourses, that he departs from the modes and orders of others (1985, 61; Discourses 1, pref.; see 1.55). The Prince contains his demand that someone with the virtue and fortune pick up the sword and do what Machiavelli is unable to complete (see Discourses, 2, pref.). He counsels another through his words to perform the deeds he cannot. Machiavelli will become a great teacher for the one who acts as his instrument, but in a fashion that will not derogate from the fame of that truly virtuous prince. Moses is Machiavelli's most outstanding example of a prince who founds a state and orders a religion through his own virtue. Moses' accomplishment required that he seize the opportunity to display his virtue, and Machiavelli explains, "He who reads the Bible intelligently [sensatamente] sees that if Moses was to put his laws and regulations [ordini] into effect, he was forced to kill countless men who, moved by nothing else than envy, were opposed to his plans" (Discourses 3.30 [Machiavelli 1971, 237]). Whoever reads Machiavelli's Prince in a similar manner and understands it will see that he too must arrange the deaths of a very great number of men. If their deaths will mean a reformation of political life, then we concur with Rousseau that The Prince is a work intended for "republicans."

Notes

We would like to thank Eduardo Velasquez, Roger D. Masters, Paul A. Rahe, and Joseph Alulis for their comments and suggestions.

1. We shall cite *The Prince* (1985) by page, and the *Discourses* on the First Decade of Titus Livius (1989) and the Florentine Histories (1988) by book and chapter. Where we cite the Italian, references are to Machiavelli 1971. All references in the text are to *The Prince* unless otherwise noted.

2. For a review of the literature on Machiavelli, see Berlin 1980, 25–39; Cochrane 1961; Geerken 1976. On Machiavelli's modernity, see, e.g., Figgis 1960, chap. 3; Mansfield 1981; Wolin 1960, chap. 7. For dissents, see A. Gilbert 1938; Parel

1992; Skinner 1978, 1:113-90; idem 1990).

- 3. Rousseau claims that Machiavelli's "hidden intention" is revealed by the contrast between the "maxims" of The Prince and those of his republican writings and says that "this profound political theorist has had only superficial or corrupt readers until now." Rousseau claims that Machiavelli was forced "to disguise his love of freedom" (Social Contract 3.6, n.). McKenzie examines Rousseau's rehabilitation of Machiavelli and concludes that he "found a way of coming to terms with The Prince but not with Machiavelli's republican theory ... without first extracting those teeth which threatened to rend the fabric of his own republican theory" (1982, 226-28). McKenzie's analysis is helpful, but he at least underestimates the similarity of Rousseau's legislator to Machiavelli's prince. Rousseau cites Machiavelli in his chapter on the legislator in the Social Contract (2.7), and his examples of legislators here and elsewhere are the same individuals Machiavelli cites as princes by their own virtue in chapter 6 of The Prince. The kinship of Rousseau's legislator to Machiavelli's prince also brings our interpretation of Machiavelli closer to Rousseau's.
- 4. Felix Gilbert follows Meinecke in arguing that *The Prince* was composed in stages, the first stage being the first 11 chapters, the "little treatise" Machiavelli announces in the famous letter to Francesco Vettori (F. Gilbert 1977, 112–14; see Machiavelli 1989, 2:929). Whatever the merit of Gilbert's thesis, it at least suggests the coherence of the first half of the work and the need to read it with the discussion of the papacy in chapter 11 in mind. In contrast, Skinner (1981) does not discuss chapter 11 in his interpretation of *The Prince*.
- 5. Machiavelli appears to refer to Savonarola as the one who blamed Italy's invasion on its "sins," but he may also have Petrarch in mind. *The Prince* ends with the conclusion to Petrarch's patriotic poem "Italia Mia" but does not include Petrarch's lament over the divisions of Italy (divisions Machiavelli attributes primarily to the Church): "It is on account of our own sins, and not a natural thing that the slow northerners should conquer us in intellect" (Petrarch 1976, 260–61).

6. This application of Machiavelli's vituperation to the clergy is not intended to derogate from his obvious disgust for gentlemen conventionally understood (see *Discourses 3.29; Florentine Histories 1.39*).

- 7. Machiavelli is astonished that Baglioni, who possesses the heart of a criminal, could not commit this deed. As evidence of this criminality Machiavelli adduces the claims that Baglioni committed incest with his sister and killed his cousins and nephews in order to rule (Discourses 1.27). Machiavelli's characterization of Baglioni shares a striking kinship with the historical Cesare Borgia. Guicciardini reports that it was believed that Cesare had had an incestuous affair with his sister, Lucrezia (1970, 197). Burckhardt relates that in order to have his father's territorial ambitions redound solely to him, Cesare murdered his brother, brother-in-law, and other relations (1958, 129).
- 8. Machiavelli's account of Romulus' founding of Rome departs from that of Augustine, who views his fratricide as an indictment of politics in a fallen world (*De civitate Dei* 15.5, see also 3.6; cf. Livy, *De urbe condita* 1.6). In addition, Machiavelli appears to exaggerate Romulus' crimes in comparison to Livy. Whereas Livy says merely that Romulus declined to go to war with those responsible for his colleague's death (1.14), Machi-

avelli declares that Romulus was "a party to the death of Titus Tatius" (Discourses 1.9). Perhaps this emphasis derives from Machiavelli's recognition of the magnitude of the crime that is necessary to found a new Rome.

9. Although Machiavelli speaks in *The Prince* of the "dispersal" of the Athenians as the occasion for Theseus to show his virtue (p. 23), it was in fact upon his return from killing the Minotaur that Theseus actually seized the occasion: he failed to raise a white flag (as promised to herald his safe return), and his father, King Aegeus, leapt to his death. Plutarch relates the story in his *Life of Theseus*, Machiavelli's source, and says of Theseus that "after the death of his father Aegeus, forming in his mind a great and wonderful design, he gathered together all the inhabitants of Attica into one town, and made them one people of one city, whereas before they lived dispersed" (n.d., 13–15, 48). Pitkin claims that "the Founder saves and protects, rather than slays, his father," discussing in this context the actual or potential parricides we mention as well: Baglioni, Liverotto da Fermo, and Cesare (1984, 60–63; see also Strauss 1958, 258).

10. Actually, several of these provinces had been added to the kingdom only quite recently. Modern scholars have tended to overstate Machiavelli's view of France when they claim that he found there a model for Italy, for France was not yet a fully unified realm, much less a nation-state (see Rubinstein 1990, 24–28; Skinner 1981, 6–7). The process of France's consolidation, which necessitated control over previously independent "barons," is described by Machiavelli in his Ritratti delle cose della francia (1971, 56). Brittany, for example, was acquired only in 1491, when Louis XII obtained an annulment from his first wife to marry Anne of Brittany, who carried the dukedom as her dowry. The annulment was granted by Pope Alexander and delivered by Cesare, who in payment obtained the duchy of the Valentinois and the hand of Charlotte d'Albret, the king's niece (Machiavelli 1985, 15, 72–28; see Guicciardini 1970, 150). A similar arrangement was not forthcoming for Henry VIII of England not long thereafter.

not forthcoming for Henry VIII of England not long thereafter.

11. Machiavelli seems to play on the fact that the names of the pope and his son replicate those of these great historical figures; hence, Cesare is both "Cesare" and "Caesar" in the scholarly literature. For example, in The Prince, in a context that clearly applies to Julius Caesar, Machiavelli claims that e Cesare era uno di quelli che voleva pervenire al principato di Roma (p. 64 [Machiavelli 1971, 281]). The original Italian, removed from its immediate context, might apply to either Borgia or Julius. Further, if Caesar aspired to a "principate," Cesare attempted to acquire an "empire" (imperio) before the pope died (p. 31 [Machiavelli 1971, 268]). Gentillet notes the possible ambiguity of this last citation and says of Cesare: "Either Ceasar [Cesar] or nothing: as though to say that he esteemed nothing less than being lord as was Julius Caesar [Cesar]" (1968, 340). See Florentine Histories: "Men too, once Caesars [Cesar] and Pompeys, have become Peters, Johns, and Mathews" (1.5 [Machiavelli 1971, 637]).

12. In the Discourses, Machiavelli notes that "Pope Alexander VI destroyed the lords who ruled" the Romagna, thus eliminating "the most wicked ways of living" (3.29). Here the author ascribes Cesare's actions to his father, a conclusion he also draws in *The Prince* in a passage we shall discuss.

also draws in *The Prince* in a passage we shall discuss.

13. Should we have missed his initial reference, Machiavelli appears to reenforce it immediately when he likens new princes who acquire by fortune to those who "were made princes by Darius" (p. 26)—though this Darius is not the Darius of chapter 4, as noted by Mansfield in his edition (Machiavelli 1985, 26).

14. For the details of Cesare's deception, see Machiavelli's "Description of the Method Used by Duke Valentino in Killing Vitellozzo Vitelli, Oliverotto da Fermo, and Others" (1989, 1:163–69).

15. At one point in the *Discourses*, Machiavelli says that the number of great in any state never exceeds 40 or 50. Because the number is so low, a prince can kill them or honor them according to their standing and thereby satisfy them for the most part (1.16). Why does Machiavelli place such a definite number on a class that surely varies according to the size of

the state? At least one body of nobles did satisfy this amount in Machiavelli's time—the College of Cardinals. Although the number of cardinals was set at 24 by the Councils of Constance (1418) and Basle (1436) (Catholic Encyclopedia s.v. "cardinal"), at least Popes Alexander and Julius disregarded the limit and raised money by selling addition cardinals' hats or sometimes by killing existing cardinals and selling their positions (Machiavelli 1985, 47; see Guicciardini 1970, 241). In 1503, 38 cardinals—the vast majority of them—convened to elect Pius III and then Julius II a month later (idem 1969, 170; idem 1970, 243).

16. The editor disputes this account and states that Alexander seems to have died of a malarial fever (Guicciardini 1970, 240). Frederick of Prussia accepts the account derived from Guicciardini, commenting of Alexander's death: "This is the prudence, wisdom, ability, and virtue that Machiavelli never tires of praising" (1981, 62). Burckhardt also accepts the account (1958, 128, 132). Whatever the validity of Guicciardini's account, Alexander and Cesare were widely known to have poisoned a great number of people, including cardinals. More importantly, Machiavelli appears to have been Guicciardini's source about Alexander and Cesare, since he repeats in his History of Italy (1969, 166) what Machiavelli relates in The Prince regarding Cesare's statement that he himself was "dying" at the same time as his father (p. 32), so that Machiavelli very likely accepted that Alexander had been accidentally poisoned.

17. Burckhardt seems to have been misled by Machiavelli's remark: "And what might not Cesare have achieved if, at the moment when his father died, he had not himself been laid upon a sick-bed! What a conclave would that have been, in which, armed with all his weapons, he had extorted his election from a college whose numbers he had judiciously reduced by poison—and this at a time when there was no French army at hand! In pursuing such an hypothesis the imagination loses itself in an abyss" (1958, 133).

18. Mansfield notes in his edition of Machiavelli's *Prince* (1985, 29) that Machiavelli's use of the term *pienissima potestà* in this context recalls the papal claim of *plenitudo potestatis*.

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RESEARCH NOTES

COMPARATIVE DEMOCRACY: THE ECONOMIC DEVELOPMENT THESIS

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In comparative politics, an established finding—that economic development fosters democratic performance—has recently come under challenge. We counter this challenge with a dynamic pooled time series analysis of a major, but neglected data set from 131 nations. The final generalized least squares—autoregressive moving averages estimates (N=2,096) appear robust and indicate strong economic development effects, dependent in part on the nation's position in the world system. For the first time, rather hard evidence is offered on the causal relationship between economics and democracy. According to Granger tests, economic development "causes" democracy, but democracy does not "cause" economic development. Overall, the various tests would seem to advance sharply the modeling of democratic performance.

Political sociology has few, if any, "iron laws." However, certain central hypotheses seem so established as to be almost beyond challenge. According to Lipset "Perhaps the most widespread generalization linking political systems to other aspects of society has been that democracy is related to the state of economic development" (1959, 75). Among the many reasons offered for this empirical connection, a common idea is that increasing economic benefits for the masses intensify demands for the political benefits of democracy. Economic development can spread authority and democratic aspirations among a variety of people, thus fostering democracy (Dahl 1989). The notion of economic development as a "requisite" to democracy (Lipset 1959) has survived increasingly sophisticated statistical tests.

Leading relevant quantitative, multivariate studies in the political science and sociology literature include Jackman (1973), Bollen (1979, 1983), Bollen and Jackman (1985) and Brunk, Caldeira, and Lewis-Beck (1987). In each of these regression-based analyses, data are gathered on a sample of nations, and a nondichotomous measure of democratic performance is predicted from economic development and, usually, other independent variables. Economic development consistently emerges as a statistically and substantively significant influence on democracy. For example, Brunk, Caldeira, and Lewis-Beck recently found that economic development alone accounts for more variance in democracy than the other independent variables taken together (1987, 468).

Given the strong results of these varied regression analyses, one might imagine that the "economic development thesis" merited little further testing. However, these studies, careful as they are, have noteworthy limitations. The samples are rather small (average = 78 nations) and sometimes explicitly nonrepresentative. (Bollen and Jackman [1985], for example, exclude communist nations.) Further, they are all based on democracy measures that are by now quite old, from either 1960 or 1965. Finally, and most importantly, they are cross-sectional, not time-series, in design (though Bollen's [1979] study contains a

measure from an earlier cross section). Of course, the absence of a temporal dimension makes it difficult to overcome the criticism that the positive economics—democracy connection represents a spurious association, occurring within a particular time-slice.

These data difficulties receive some remedy in Arat (1988) and Gonick and Rosh (1988), two more current quantitative democracy studies. Both use larger samples of nations (95 and 116, respectively), including communist ones. Moreover, they extend the measurement of democracy into the 1970s to 1977 and 1979, respectively). Of special note, in their model estimations, they explore a series of cross sections over time. Given the previous quantitative work, one might expect economic development to continue to exhibit strong, unrivaled, effects. But it does not. To quote Arat: "Only a few countries fit the models suggested by modernization theory. . . . It can be concluded that increasing levels of economic development do not necessarily lead to higher levels of democracy, even for the less developed countries" (1988, 30). The conclusions of Gonick and Rosh have a similar ring: "Economic development . . . is not the most important factor affecting the degree to which a political system can be characterized as a 'liberal democracy'.... Our application has allowed us to reject the findings of Lipset" (1988, 189, 196).

These surprisingly weak findings, based on apparently better data, force a reevaluation. Perhaps economic development actually has little influence on political democracy. Or, possibly, these analyses possess shortcomings that still leave the question open. Let us consider three potential problem areas: design, specification, and measurement. Although both studies examine cross sections over time, neither takes full advantage of the statistical leverage a pooled time-series design allows. Arat (1988) simply applies ordinary least squares (OLS) to the pool before discarding it for a country-by-country time-series examination. Gonick and Rosh apply a "variance component" model to the pool and neglect to discuss the central problem of autocorrelation from the time series (1988, 195). Nor does either explore lagged effects or Granger tests for causality. In other words,

they ignore the dynamic, analytic opportunities that the time component of the pool affords.

With regard to specification, Arat does not include any independent variables other than economic development, a modeling strategy not followed after Jackman's (1973) initial baseline work (1988, 27). While Gonick and Rosh have an equation that is more fully specified, they include independent variables (e.g., urbanization, literacy) that the other quantitative studies do not include in their "direct effects" equations (1988, 194–95). This specification difference could generate an unusual amount of collinearity, perhaps accounting for their weaker findings on economic development effects.

With respect to measurement, they both rely heavily on the cross-national time-series data of Banks (1979). Arat (1988) uses the Banks data for five of the six scales in her democracy measure. Gonick and Rosh (1988), for their democracy measure, use Banks data exclusively. Of course, the Banks data have been widely used in quantitative democracy studies. The difficulty is that key Banks democracy indicators, as they are commonly used, contain considerable measurement error, as Bollen (1993) has recently shown.

Bollen (1993) employs structural equation modeling with latent variables to arrive at the estimated "validity component" of democracy variables that have been constructed from different data sources. (The "validity component" is what remains of the measure after the random measurement error and systematic measurement error components have been extracted. A variable with no measurement error could be assigned a validity component rating of 100%.) According to Bollen (1993), the validity component ratings on the Banks democracy variables he studied are as follows: chief executive elected (14%), competitiveness of the nomination process (62%), effectiveness of legislative body (79%), freedom of group opposition (92%). These four variables, with the possible exception of the last, contain serious measurement error. Gonick and Rosh (1988, 184) use all four variables in building their democracy index, while Arat (1988) uses the first three. Given the high degree of measurement error that is thus contained in their democracy indices, it becomes difficult to have confidence in their conclusions.

We shall try to overcome the foregoing research problems. We examine data on more nations, over a longer and more recent span of time. The data set is not Banks but, rather, Gastil. The research design takes full advantage of the pooled time series structure, ending with a generalized least squares—autoregressive moving averages (GLS–ARMA) estimation, with appropriate diagnostics along the way. Model specification builds on previous work, but introduces promising theoretical innovations that at the same time guarantee a rigorous test of the economic development thesis. Additionally, the issue of measurement error (which affects Gastil data as well) is dealt with. Finally, firm conclusions are drawn about the economic development thesis.

A DEMOCRACY INDEX: THE GASTIL DATA

Democracy can have many meanings. See the current provocative philosophical exchange between Mueller (1992) and Lienesch [1992]. Here we follow the tradition of the above quantitative studies, conceiving of democracy as a continuous variable, scored numerically from low values to high. The particular democracy measure we select aims to parallel the Bollen (1979) measure. Working for Freedom House, Gastil has provided annual (1972-89) ratings of nations on two dimensions (see Appendix A). The first, here labeled "political rights," assesses the right to vote, election meaningfulness, multiple political parties, opposition power, and government independence from foreign or military control. The second, here labeled "civil liberties," covers the freedoms of speech, assembly, and religion and freedom from terrorism or blatant inequality. (Details on the items appear in Appendix A.) By adding together the ratings on these two 7-point scales (after reversing their coded order for readability), each nation acquires an overall democracy index, ordered from a low of 2 to a high of 14.

The Gastil data have received considerable use in social science research (see Bollen 1993) but not for constructing an explanatory model of democracy either in cross-sectional or time-series studies. In certain respects, these data represent a splendid opportunity. Using them, we assembled annual observations on 131 nations 1972–89, yielding a total sample size of 2,358. (The nations and their democracy scores are listed in Appendix B.) Hence, it becomes possible to incorporate simultaneously analysis of over-time, as well as cross-nation, variation in democracy. Sources of variation that would have been absent in a single cross section are now captured. In sum, a pooled methodology offers the prospect of serious gains in statistical efficiency.

MODEL SPECIFICATION

In the seven studies at hand, there is little agreement on the proper independent variables, beyond the inclusion of economic development (E) itself, which is invariably postulated to have a curvilinear effect on democracy (D). A few variables do appear in more than one model: public control of the economy, Protestant population, historical timing of development, and world-system position (W). Each of these attains statistical significance in at least one model, which suggests that other social forces (S), as well, influence democratic performance. However, the only variable to achieve statistical significance at .05 in more than one study is world-system position (Bollen 1983; Bollen and Jackman 1985; Gonick and Rosh 1988). This suggests the conceptual specification:

D = f(E, S, W)

where D = democracy, E = economic development, S = other social forces; W = position in the world-system.

We now proceed to the operationalization of these independent variables. For E, we employ energy consumption per capita (logged), the economic development measure most often used, and one correlated .9 with gross national product per capita (Bollen 1979, 578). For S, we utilize a proxy, the lagged value of democracy, Dt - 1. The rationale is straightforward. The other social forces acting on democracy are uncertain (and perhaps unmeasurable across the pool). However, to the extent they are structural processes (e.g., public control of the economy, Protestant population, historical events), they will be essentially summarized in the democratic performance of the nation during its previous year. In other words, Dt - 1 acts to control for omitted independent variables. With such a pervasive control in place, it is more difficult for spurious economic effects to be reported. (Still, Bollen (1979) found economic development significantly related to democracy in a 1965 cross section, even after including the 1960 democracy index as a predictor.)

With regard to a measure for W, a few preliminaries are in order. The notion of a world system represents a major theoretical step forward in the literature. Accordingly, a nation's chances for democracy may depend on where it is placed within the world economy. (For the theoretical inspiration, see Wallerstein 1974.) The core economies (high wages, high profits, capital-intensive), are bound in a lopsided exchange with the periphery economies (low wages, low profits, and labor-intensive). The constraints imposed by this international economic system (so the argument goes) seriously diminish the likelihood of successful democracy in the periphery.

Operationalization of the world-system position differs somewhat from study to study, but always involves dummy variables for different sets of countries (e.g., semiperiphery *M* and periphery *P*) entered additively into the estimation equation. Sometimes, expected effects have fallen short of statistical significance. Further, even when significance is attained, the form of the effect is seriously misspecified.

The last point deserves amplification. Gonick and Rosh who are leading world-system effects advocates, argue that "the effects of economic development are fundamentally shaped by the country's position vis-à-vis the World-Economy" (1988, 195). This is an unambiguous argument for the world-system position's operating nonadditively. In particular, world-system position conditions the impact of economics on democracy. To incorporate this dependency, then, the preferred specification is an interaction of the world-system position dummies with the economic development variable. This specification should actually increase the likelihood of uncovering world-system effects. (Details on construction of the world-system categories appear in Appendix B.)

These considerations lead to the model

$$Dt = a + bDt - 1 + cEt + d(M \times Et)$$

$$+ e(P \times Et) + u$$
, (1)

MODEL ESTIMATION

When OLS is applied to a pool of cross-sectional and time-series data, the violation of classical regression assumptions is highly likely. In fact, Stimson claims that autocorrelation and heteroskedasticity are virtually "inherent" in pooled data. (1985, 919). (Unfortunately, Arat [1988] relied exclusively on OLS in her pooled analysis; Gonick and Rosh [1988] ignore the autocorrelation problem.) From the time-series component, we must anticipate the former problem, from the cross-section component, the latter. Therefore, our estimation utilizes what Kmenta (assuming the same autocorrelation per unit), calls a "cross-sectionally heteroskedastic and timewise autoregressive model" (1986, 618), appropriately modified to cope with the special problem of having a lagged dependent variable, Dt - 1, as a predictor. (This condition also renders the estimates of another common technique, least squares dummy variables (LSDV), inconsistent [Hsiao 1986, 72]). We shall treat the question of the lagged dependent variable, then go on to assess heteroskedasticity. Finally, we shall develop the case for the preferred estimator, GLS-ARMA.

Because Dt-1 is on the right-hand side of the equation, usual methods (e.g., the Durbin–Watson test) for diagnosing autocorrelation are misleading. However, when the appropriate h- and m-tests were applied, we found the OLS estimates exhibited significant first-order autocorrelation (Ostrom 1990, 66). Therefore, we turned first to an instrumental variables (IV) approach. These results provided an estimate of this first-order autocorrelation (p=.90). Furthermore, these IV–OLS estimates revealed highly significant cross-sectional heteroskedasticity, LM = 1,776.86 (on this Breusch-Pagan LaGrange Multiplier statistic, see Greene 1990, 465–69).

Thus, the variables were adjusted for this first-order autocorrelation, and heteroskedasticity was corrected (with the "force homoscedastic model" option in Microcrunch). The estimation procedure was GLS-ARMA (to allow a final check on any residual autocorrelation). In addition, we evaluated the possibility of mutual unit correlation, concluding that it was not a serious threat.³

These GLS-ARMA estimates appear below. The residuals from this estimation were scrutinized in an examination of the pooled diagnostics. Following

Stimson, we paid special attention to the summary value of unit residuals, the ratio of residual variance, and any leftover nonstationarity in the autocorrelation function (1985, 939). According to these three diagnostic criteria, GLS–ARMA does edge out rival estimators: IV-OLS, GLS–error components (GLSE), or LSDV.⁴

with pseudo-R=.71 and N=2096, where all variables are defined as in equation 1 but transformed (as indicated by the ') on the basis of the instrumental variable first-order autocorrelation estimate, then submitted to GLS-ARMA estimation, under the "force homoscedastic model" in Microcrunch. The figures in parentheses are standard errors, * means statistical significance at .01 (t>2.58), the figures in brackets are absolute t-ratios, and pseudo-R is the correlation between the observed Dt and the Dt predicted from using original equation 1 variables with equation 2 coefficients. Sources for the democracy and world-system data are described in the Appendix B. The energy consumption data were gathered from United Nations publications.

On the basis of these results, economic development has a highly significant impact on democratic performance. Moreover, when that impact is conceived of as interactive, rather than additive, the interaction formulation has the edge. 5 Economic development matters most for nations in the core, where c = 2.49; it still matters, but about half as much, in the semiperiphery (2.49 - 1.33 = 1.16 net)economic effect); and for nations in the periphery, the economic effect is just a bit less (2.49 - 1.54 = .95). Taken together, economic factors, both international and domestic, appear decisive in shaping a nation's democratic future. Still, there is not economic determinism. Other important social forces, captured at least partially by past values of the democracy index itself, exercise an effect. The model, by blending economic with social explanation, manages a good fit to the data, as indicated by the pseudo-R. But to expand the implications of the model, three methodological concerns need to be addressed: measurement error, ceiling effects, and causality.

METHODOLOGICAL CONCERNS

An assumption underlying the estimates of equation 2 (as well as the estimates from other quantitative democracy studies) is that the variables are measured without error. However, common measures of de-

mocracy have considerable error. As mentioned, Bollen (1993) demonstrated substantial error in the Banks measures. He also explored Gastil measures (essentially the two components of our study), assigning political rights a validity rating of 93% and civil liberties a validity rating of 78%. Encouragingly, these validity scores have a higher average (86%) than the Banks indicators (average validity = 62%). However, they are not error-free. According to Bollen, the two Gastil components have systematic error, respectively, of 7% and 16%, induced by the Gastil rating method itself.

The question is whether this "methods factor" (as Bollen calls it), even though relatively small, seriously biases our inference about economic effects. Some critics of Gastil have argued, on the basis of their impressions, that the measures he provided Freedom House have a conservative bias. For example, nations that are staunch American allies are alleged to get more democratic ratings than they deserve (Hartman and Hsiao 1988). Using 1980 data, Bollen (1993) assesses the methods bias in the Gastil measure nation by nation, where a high standardized score implies that the judge is too favorable, a low score, not favorable enough, and zero, a neutral judgment. If the Gastil measure has a conservative bias, then that could bias the economic development parameter estimate. To test this possibility, we sequentially reestimated equation 2, progressively eliminating nations with positive scores (i.e., first greater than 2 standard deviations above the neutral value of 0, then greater than 1.5, then greater than 1.0). The economic development coefficient remained statistically significant, its magnitude virtually unchanged: 2.51, 2.52, and 2.52, respectively. Thus, there is no evidence that conservative bias in the Gastil measure, to the extent it exists, has biased the economic development coefficient. (We repeated the same test with the negative scores, in case of a possible liberal bias, and again concluded that the economic development coefficient was unaffected.)

Of course, methods factor error is not exclusively a question of conservative or liberal bias. Other issues, such as the occasional adjustment of democracy scores to reflect poor economic conditions, have been raised Freedom in the World 1978:13). What would be useful is some general way of comparing our Gastil measure to a "true" measure of democracy. Fortunately, Bollen has constructed an index for 1980 that is highly correlated with the latent variable of democracy and virtually uncorrelated with the distorting methods factors (1993, app.). These uncontaminated 1980 index scores may be rather close to "true" democracy scores. While available only for 1980, they nevertheless provide invaluable baseline data. We find that this 1980 Bollen democracy index correlates .93 with our Gastil democracy measure for the same year. (All nations were measured on both, except for one, giving N = 130.) This suggests that very little error remains in the Gastil index as constructed, since it correlates almost perfectly with Bollen's virtually error-free democracy measure. In other words, the

Gastil index, as assembled here, does an excellent job of tapping the underlying real variable of democracy.

While the Gastil index has considerable validity, it is not flawless. Like other leading democracy scales, it has limited refinement. Nations can score only so high and no higher. For instance, in the widely used 1965 political democracy index, also created by Bollen, 27 out of the 123 nations sampled score at the top, 90-100 (1980, 387-88). Of course, these scores reflect the scale "ceiling," not the attainment of ideal democracy. Our Gastil measure allows a bit more variation here, with only 20 out of the 131 nations attaining a top score of 13 or 14 across the time period. Moreover, because of the time-series component of the design, nations obviously can attain more than one score. Overall, the added variance on the dependent variable afforded by this pooled design makes for more efficient statistical inference.

A last methodological concern is over causality, an issue that plagues this literature. Does economic development "cause" democracy? Or does democracy really "cause" economic development? The cross-sectional work has had to assume causality runs from economics to democracy, while the limited-time-series work has not fully utilized the testing opportunities that temporal observation allows. Fortunately, Granger (1969, 1988) offers some straightforward causality tests applicable to time series. Accordingly, if X causes Y, then past values of X should aid in the prediction of Y (even after controlling for past values of Y). Further, Y should not aid in the prediction of X.

To carry out the test (here extended to these pooled time-series data), we first established the optimal number of lags for the independent variables, using the final prediction error test (see Mahdavi and Sohrabian 1991, 44–45). With regard to predicting democracy (Y), optimal lagged variables turn out to be Dt-1 and Et-1. With regard to predicting economics (X), the optimal lagged variables are more elaborate, Et-1... Et-6, Dt-1. The block F-tests on the unrestricted versus restricted OLS equations on the pool are, for democracy, F (1,2224) = 18.18 (significant at .05), and for economics, F (1,1572) = .00 (not significant at .05).

According to the first test, the lagged economic development variable is a significant predictor of democracy even after controlling for past values on democracy. Therefore, we reject the hypothesis that economic development does not cause democracy. Following the second test, the lagged democracy variable is not a significant predictor of economic development after controlling on past values of economic development. Therefore, we accept the hypothesis that democracy does not cause economic development. These tests allow, for the first time, a rather firm conclusion on this causal relationship: economic development causes (or at least Grangercauses) democracy, but democracy does not cause economic development. (These Granger results seem especially robust, even surviving controls on worldsystem position.)6

CONCLUSION

The economic development thesis, long a staple of comparative democracy studies, has recently come under challenge. The pooled-time-series studies of Arat (1988) and Gonick and Rosh (1988), while in some ways the most advanced quantitative pieces in this critical literature, appear to be characterized by five flaws: (1) they measure democracy with errorladen data (2) the data do not extend past the 1970s, (3) the models are misspecified, in particular with regard to world-system-position interaction effects, (4) estimation procedures are not dynamic and fail to utilize more efficient estimation techniques such as GLS-ARMA, and (5) causality tests are not administered. In response, we have analyzed a very large sample of nations (131) over a long period of time (1972-89), employing a relatively neglected data set on democracy. After refining the model specification, a pooled design was applied, with final parameter estimates provided by the GLS-ARMA technique. The results were demonstrated to be methodologically robust. In particular, the Gastil democracy index has considerable measurement validity and promises to be increasingly used in future work.

We established that the causal arrow most probably runs from economic development to democracy, rather than vice versa. Further, that effect is highly significant statistically. Substantively, the economic development coefficient of equation 2 implies that for every tenfold increase in per capita energy consumption, the nation could expect about a two-and-a-halfpoint rise on the democracy scale. How important an effect is this? A standard deviation increase in per capita energy consumption (logged) is predicted to yield, on average, a .48 standard deviation change in the democracy score. By this measure, the economic impact is not small, and as effects cumulate over time, the impact can grow. A simple measure of importance comes from correlating average (over nations) world energy consumption with the average (over nations) world democracy score over time (1972-89), r = .82

On balance, it is clear that economic development substantially improves a nation's democratic prospects. However, the full magnitude of that effect depends on the location of the nation in the world system. As the nation moves from the core, to the semiperphery, to the periphery, the effect diminishes. Even in the periphery, however, the effect remains statistically and substantively significant. Thus, around the world, economic development works to foster democracy. Indeed, our Granger results indicate that the relationship works in that direction but not the other. To the extent that this finding holds for nations currently in democratic transition, the implication is that democratic reform by itself cannot be counted on to bring about the needed economic development. However, this is no counsel to dictatorship. Just as clearly, we found that democracy, while not apparently a direct cause of economic development, certainly does it no harm.

Moreover, as the lag pattern of the structural model shows, past democratic performance breeds future democratic performance. Democracy, then, can be furthered for its own sake, without sacrificing economic development.

APPENDIX A: THE GASTIL DEMOCRACY DATA

The democracy data are compiled from Raymond D. Gastil's Freedom in the World: Political Rights and Civil Liberties 1989: 50-61 for the years 1972-88 and 1990: 312-13 for the year 1989. (Freedom House assumed responsibility for the scoring after the 1989 issue.) Gastil ranks each nation on separate 7-point scales for "political rights" and "civil liberties." Before assigning a score, Gastil carries out "library research," including the consultation of the New York Times, Christian Science Monitor, Banks's Political Handbook of the World, regional journals, international press association publications, Amnesty International, State Department reports, and specific country publications (Freedom in the World 1989:26; see also Bollen, 1986: 583-84). The categories for each of his checklists follows. (Freedom in the World 1989:9). Note that he avoids a simple one-to-one correspondence of a checklist item and a scale point, in arriving at his overall score.

Checklist for Political Rights

- Chief authority recently elected by a meaningful process
- Legislature recently elected by a meaningful process
- 3. Fair election laws, campaigning opportunity
- 4. Fair reflection of voter preference in distribution of power
- 5. Multiple political parties
- 6. Recent shifts in power through elections
- 7. Significant opposition vote
- 8. Freedom from military or foreign control
- 9. Major groups allowed reasonable self-determina-
- 10. Decentralized political power
- 11. Informal consensus, de facto opposition power

Checklist for Civil Liberties

- Media/literature free of political censorship
- 2. Open public discussion
- 3. Freedom of assembly and demonstrations
- 4. Freedom of political organization
- Nondiscriminatory rule of law in politically relevant cases
- Free from unjustified political terror or imprisonment
- 7. Free trade unions or peasant organizations
- 8. Free businesses or cooperatives
- 9. Free professional or other private organizations
- 10. Free religious institutions
- 11. Personal social rights

APPENDIX B: THE SAMPLE OF NATIONS

Nations are listed with their range of democracy scores constructed as described, from the material in Appendix A (2–14 being the widest possible range) and their world-system Position (c = core, m =semiperiphery, p = periphery). To construct the world-system-position dummies, we combined the ratings in nine studies, following a "panel of experts" strategy (Arrighi and Drangel 1986; Bollen 1983; Chase-Dunn 1983; Chirot 1977; Frank 1969; Gonick and Rosh 1988; Nemeth and Smith 1985; Smith and White 1992; Snyder and Kick 1979). There was high agreement among the judgments. However, when discrepancies occurred, more weight was give to the more recent evaluations. As can be seen, the classification has high "face validity"; and when we experimented with alternate classifications, the results did not change substantively.

Afghanistan 2-7 p, Albania 2-2 p, Algeria 3-6 p, Argentina 4-13 m, Australia 14-14 c, Austria 14-14 c, Bahamas 11–13 p, Bahrain 5–8 p, Bangladesh 4–10 p, Barbados 13-14 p, Belgium 14-14 c, Benin 2-4 p, Bolivia 4-11 p, Brazil 6-12 m, Bulgaria 2-4 m, Burkina Faso 3-11 p, Burma 2-4 p, Burundi 2-4 p, Cambodia 2-5 p, Cameroon 3-6 p, Canada 14-14 c, Central African Republic 2-4 p, Chad 2-4 p, Chile 4-13 m, China 2-4 p, Colombia 9-12 m, Congo 2-5 p, Costa Rica 14-14 p, Cote d'Ivoire 4-6 p, Cuba 2-4 p, Czechoslovakia 2-4 m, Denmark 14-14 c, Dominican Republic 9-13 p, Ecuador 4-12 p, Egypt 4-8 p, El Salvador 6-11 p, Equatorial Guinea 2-4 p, Ethiopia 2-5 p, Fiji 5-12 p, Finland 12-14 m, France 13-14 c, Gabon 4-5 p, Ghana 3-11 p, Greece 4-13 m, Guatemala 4-12 p, Guinea 2-4 p, Guyana 6-12 p, Haiti 3-7 p, Honduras 6-11 p, Hungary 4-9 m, Iceland 14-14 m, India 9–12 m, Indonesia 4–6 p, Iran 4–6 m, Iraq 2–3 m, Ireland 13-14 m, Israel 11-12 m, Italy 12-14 c, Jamaica 11–13 p, Japan 13–14 c, Jordan 4–6 p, Kenya 4-7 p, Kuwait 5-9 m, Laos 2-6 p, Lebanon 5-12 p, Liberia 4-7 p, Libya 2-4 s, Luxembourg 13-14 c, Madagascar 4-8 p, Malawi 3-3 p, Malaysia 5-11 m, Mali 2-4 p, Malta 10-14 p, Mauritania 3-5 p, Mauritius 10-12 p, Mexico 8-9 m, Mongolia 2-2 p, Morocco 6-9 p, Nauru 12-13 p, Nepal 5-9 p, Netherlands 14-14 c, New Zealand 14-14 c, Nicaragua 4-9 p, Niger 3-4 p, Nigeria 4-11 p, North Korea 2-2 p, North Yemen 4-8 p, Norway 14-14 c, Oman 3-4 p, Pakistan 4-10 p, Panama 3-9 p, Paraguay 4-9 p, Peru 4-11 p, Philippines 6-12 p, Poland 4-9 m, Portugal 5-13 m, Qatar 4-6 m, Romania 2-3 m, Rwanda 3-4 p, Saudi Arabia 3-4 m, Senagal 4-9 p, Sierra Leone 5-7 p, Singapore 6-8 m, Somalia 2-3 p, South Africa 5-7 m, South Korea 5-11 m, Spain 5-14 m, Sri Lanka 7-12 p, Sudan 2-7 p, Sweden 13-14 c, Switzerland 14-14 c, Syria 2-5 p, Tanzania 4-4 p, Thailand 4-11 p, Togo 2-4 p, Tonga 8-10 p, Trinidad 11-14 p, Tunisia 5-8 p, Turkey 6-11 m, Uganda 2-7 p, United Kingdom 14-14 c, United Arab Emirates 4-6 m, Uruguay 4-13 p, United States 14-14 c, USSR 2-5 m, Venezuela

12-13 m, Yugoslavia 4-7 m, Zaire 2-4 p, Zambia 5-7 p, Zimbabwe 5-8 p.

Notes

We express special appreciation to B. Dan Wood for his advice on estimation for this pooled time series design.

1. The LSDV technique is also less preferred here because it is a "fixed effects" model, which implies that the observations cannot be treated as a random sample from a larger population. In contrast, these data compose a large, apparently representative, sample from a population (of nations and of time). Thus, country dummies should not enter our analysis wholesale. However, we do include selective introduction of geographic dummies (i.e., the periphery, semiperiphery variables). These variables, in their interaction form, explicitly allow for slope heterogeneity of economic effects across nations but do so in a statistically efficient and theoretically defined way. (To confirm our economic effects results, another treatment for differing slopes or intercepts across nations—the random effects model of GLS-error components-was eventually applied. See n. 4.)

2. First, OLS was applied to the pool of variables in equation 1. The Durbin's h = 7.87 indicated the need to reject at the .05 level the hypothesis of no first-order autocorrelation. The m test also showed significant first-order autocorrelation but no significant second-order autocorrelation. Finally, more general Breusch-Godfrey testing revealed this significant AR(1) process but uncovered no other processes of any order. To estimate this first-order autocorrelation, we used an instrumental variables approach (see Ostrom 1990, 65-71). An instrumental variable, Dt - 1", was constructed (from the exogenous variables of equation 1, lagged to t-1). (The instrument appears a good proxy, Dt-1" correlating .64 with Dt - 1, for which it is a stand-in.) This Dt - 1 was substituted for Dt - 1 in equation 1, and OLS was applied to achieve consistent estimates of the parameters and the residuals. Using these residuals, we arrived at an estimated pooled rho (the first-order autocorrelation) of .90.

Having thus obtained a desirable estimate of the troublesome first-order autocorrelation, we moved on to correct the original equation for it. Following a Cochrane-Orcutt-type procedure, each original variable of equation 1 was transformed (e.g., [Dt-1] - [.90Dt-2] = D't - 1). These transformed variables were then submitted to the comparative

pooled estimations (see n. 4).

3. Besides homoskedasticity and no autocorrelation, the assumption of the mutual independence of the units can also be a concern with analysis of pooled data. Here we are dealing with a sample of independent nations, rather than a population of more homogeneous geographic units like the American states (Kmenta 1986, 622-25). Therefore, we did not suspect a mutual unit correlation problem. Nevertheless, we attempted to correct for it, utilizing SHAZAM but were initially thwarted by the very high correlation between the error vectors of certain nations. One solution-exploration of the phi matrix to identify the offending high correlations—poses its own set of problems, involving a search through about 8,500 correlations (from our 131 × 131 country matrix). Another solution-eliminating countries by trial and error and reestimating—not only raises practical difficulties but would call into question the integrity of the remaining sample of nations. In the end, we chose to stick with our GLS-ARMA estimates, which have been corrected for autocorrelation and heteroskedasticity. We are quite comfortable with this because, in general, a correction for mutual unit correlation increases efficiency and makes for higher levels of statistical significance. The reported GLS-ARMA results of equation 2, then, can actually be viewed as a conservative test

4. For purposes of statistical comparison, OLS, LSDV and GLSE were applied to the variables of equation 1, corrected for the autocorrelation described in n. 2. (Recall that Arat [1988] relied on OLS, Gonick and Rosh [1988] on GLSE estimation.) The OLS results indicated significant autocorrelation remaining, according to the autocorrelation function (back five lags). The autocorrelation function of the LSDV estimator also revealed a slight amount of nonstationarity remaining, with a statistically significant first-order autocorrelation estimate of .06. GLSE makes no correction for serial correlation (or heteroskedasticity) but might be considered more efficient than LSDV for taking into account any intercept differences (which OLS ignores). GLSE in fact yielded a parameter estimate (of 2.54) for Et, almost identical in value to that in equation 2. However, it yielded a standard error of .30, clearly indicating the efficiency gains of the GLS-ARMA (heteroskedasticity-corrected) estimate of equation 2, with its lower standard error of .22.

Further, other residual diagnostics for these GLS-ARMA results are favorable. With GLS-ARMA, the standard deviation of the residual means is only .34, compared to .37 for GLSE. (The residual variance ratios show essentially the same performance level with, for example, only 9 of 131 scores exceeding 3.0 in GLSE or GLS-ARMA.) These residual departures seem small and bolster our confidence in the GLS-ARMA model estimation and specification. (A healthy pattern of residual diagnostics from a GLS-ARMA analysis led Stimson to a similar conclusion regarding his party issue polarization model [1985, 943-44, tbl. 7].) However, pooled diagnostics provide no sharp boundaries for deciding "right" and "wrong." As Stimson goes on to note, at some point it becomes "a judgment call" (p. 943). We experimented with treating the few departing cases as outliers, either eliminating them from the estimation equation or introducing dummy independent variables. We could not improve on the rather robust diagnosis already obtained with the GLS-ARMA estimates of equation 2.

5. Ideally, we would have a fully specified model, including the additive terms for world-system position (M and P) in equation 2 along with the interaction terms. We estimated this fully specified model, but severe collinearity (e.g., Et regressed on the other independent variables yields an R-squared = .994) rendered the coefficients nonsensical. Because this problem sometimes occurs in the presence of interaction terms, we applied the possible solution of centering the variables, but the results failed to improve (Jaccard, Turrisi, and Wan 1990, 31). As an alternative, we estimated a main-effects model, for comparison to the interaction effects model of equation 2. Using the same variable definitions, statistics, corrections, and estimation methods (GLS-ARMA, "force homoscedastic model" option) as in eq. 2, the results are as follows:

$$D't = .97^* + .08^*D't - 1 + .89^*E't - 4.86^*M' - 6.02^*P',$$
(3)
(.10) (.02) (.21) (.69) (.68)
[9.54] [3.80] [4.29] [7.06] [8.80]

with pseudo-R = .70 and N = 2,096.

In comparing these results to equation 2, we observe that the coefficient for the key economics variable, Et, is much less significant than under the interation specification. To appreciate this, first use the standard errors to construct confidence intervals (95%, two-tailed) around the Et coefficient in each equation. For equation 2, this confidence interval equals 2.49 +/- (1.96 \times .22) = 2.06–2.92; for equation 3, this confidence interval equals .89 +/- (1.96 \times .21) = .48–1.30. One observes that for the interaction specification of equation 2, the lower bound of the confidence interval is much farther from zero, giving much more confidence in rejecting the null hypothesis. The greater statistical security of the Et coefficient in the original interaction specification (equation 2) is summarized by comparison of the t-ratios, 11.43 and 4.29, respectively. Further, as the other t-ratios show, the precision of the other coefficients is not lessened.

6. The Granger approach, by only allowing an X to account for Y after past values of Y itself have accounted for all they can is a very conservative test. Hence, the survival of economic effects in such a test gives us some confidence in their existence. Because its purpose is to offer a demanding "test for causality," rather than to estimate a structural model, comparisons of Granger results to structural model estimates such as those of equation 2 can be misleading. However, it is

possible that even strong Granger findings, such as these, are spurious because of the operation of a third variable (Pindyck and Rubinfeld 1991, 217). The obvious "third variable" candidate is world-system position. Therefore, as a further test, we included the world-system interaction terms in the unrestricted Granger regression:

with adjusted $R^2 = .953$, N = 2,227, and Box-Ljung Q (distrb. chi-squared, 5 df) = 5.77, where the variables are defined and measured as in equation 1. The figures in parentheses are standard errors, the figures in brackets are absolute t-ratios, adjusted R^2 is the adjusted coefficient of multiple determination, and the Box-Ljung Q is a measure of autocorrelation.

These results make clear how demanding the Granger-test is. We observe that if we impose no theoretical structure on the model and operate by statistical brute force, past values of democracy appear almost totally predictive of democracy. Nevertheless, even under this extreme (and unrealistic) stricture, economic development manages statistically significant main effects, as well as statistically significant world-system interaction effects. The t-ratios are helpful here. For a one-tailed test, significance at .05 requires t > 1.64, and significance at .10 requires t > 1.28. Also, the block F-test is still significant at .05, F (3,2222) = 11.22 (a number slightly lower than the original because of different degrees of freedom). Again, we find that economic development is a determinant of democracy, irrespective of world-system position.

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THE DETERMINANTS OF INDUSTRY POLITICAL ACTIVITY, 1978–1986

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Thile the allocation of interest group monies to specific politicians has been extensively studied, little is known about the factors that determine of the overall level of political activity across groups. We study total contributions by corporate political action committees at the industry level. We create a large data set on industry political activity, covering 124 industries across five election cycles from 1978 to 1986 and sketch out a simple benefit—cost model to predict total corporate PAC contributions in each industry. The few previous studies of this phenomenon use relatively small samples and employ statistical techniques that are either biased or impose untested restrictions. The selectivity-corrected regression technique used here solves these problems. We find that industries with greater potential benefits from government assistance contribute systematically more but that the ability to realize these benefits is constrained by collective action problems facing firms in each industry.

ampaign contributions made by corporate political action committees (PACs) are increasingly important—and controversial—in U.S. elections. "Influence purchasing" is often alleged by pundits, and campaign finance reform is continually debated in Congress. In the academy, social scientists have analyzed the allocation of interest group monies and have looked for possible impacts on legislation. Few, however, have tried to explain the total contributions of economic interest groups. We shall analyze variations in contributions across industries and over time with an empirical model based on organizationally constrained profit-maximizing behavior. We assume that corporate PAC contributions in an industry are raised to enhance industry profits subject to the constraints of organization costs and freeriding incentives. Our empirical work covers 124 industries over five election cycles from 1978 through

Both the costs and benefits of political activity vary across industries. We argue that the benefits of political action are determined by direct contact with the government as a regulator or purchaser of industry output, by government's ability to ameliorate adverse market conditions, and by the industry's ability to solve collective action problems without government assistance. The costs of industry political action arise mainly from collective action problems, because effective political activity often requires concerted action by the constituent firms in each industry.

We also make a methodological contribution. Previous studies use statistical techniques that suffer from sample selectivity bias or else force the probability of acting politically to be explained by the same set of coefficients as is the amount of action taken. We explain and employ a technique that accounts for both problems. The results are consistent with the theses that industries follow investment-oriented goals in deciding the amount of political action to

undertake, and that collective action problems are important constraints on industry political activity. We can explain between 60% and 80% of the variation in contributions in our sample.

First, we review existing work on corporate political activity, particularly papers examining patterns of aggregate PAC spending by industry. Then we present an organization-cost-constrained industry-profit-maximizing model, based on a comparison between an idealized zero-organization-cost industry and the more realistic case where organizing collective action is difficult. The independent variables used in the analysis are also discussed and defined. Next, we discuss the dependent variable and total industry contributions and explain our statistical method. Then the results are presented, followed by a discussion of industry structure and political activity. To make clear how our results can explain different contribution patterns, we give examples using five actual industries.

PREVIOUS WORK ON CORPORATE POLITICAL ACTIVITY

Most work on PACs examines where contributions go and what they accomplish, not where the money comes from. Gopoian (1984), Poole and Romer (1985), Snyder (1990), and Stratmann (1992) all explicitly study the allocation patterns of PACs. Grier and Munger (1991, 1993) find that corporate, union, and trade association PACs target incumbents who win by moderate margins, have voted sympathetically, and (at least in the U.S. House) have seats on committees with jurisdiction over the PACs area of interest. Studies of the effects of contributions show more mixed results (e.g., Evans 1988; Grenzke 1989; Hall and Wayman 1990; Langbein 1986; Salamon and

Siegfried 1977; Stratmann 1991; Wright 1989, 1990). Research on specific policies finds some measurable effects. Work on broader measures of influence (e.g., general voting patterns and behavior) finds negligible impacts.¹

Turning to the sources of money, Andres (1985), Masters and Keim (1985), Grier, Munger, and Roberts (1991), Humphries (1991), and McKeown (1994) all study a binary choice: Does a corporation have a PAC?² This question is important, but if we are really to understand the pattern of political action of industries we must focus on the amounts that industries contribute as a collection of firms. Unfortunately, the work on total contributions (our dependent variable of interest) has focused on industry structure (particularly concentration) rather than offering a general model.³ Pittman (1976) argues that more contributions will be generated by concentrated industries (i.e., a few large firms). Esty and Caves (1982) and Zardkoohi (1985) find ambiguous effects for industry structure. None of these models has worked from an overarching conception of the goals of the firm and the industry and how these goals might be realized through political action.

The data that previous researchers use also vary widely. Pittman (1976) uses a Common Cause data set of large individual contributions to Nixon's reelection campaign, aggregated into industry-level observations. Esty and Caves (1982) aggregate 300 firms from the Fortune 1,000 into 35 industry-level observations. Their dependent variable is total PAC receipts plus estimated lobbying expenditures by industry, summed over the years 1976–78. Zardkoohi's sample is 412 firms with PACs that made contributions in 1980. Boies (1989) examines PAC contributions by the Fortune 500 firms in 1976 and 1980. He begins with 18 independent variables, then considers the 10 that are correlated with contributions in either year.

Though each of these papers makes a useful contribution, they each have problems in research design. The first kind of problem is the choice of a sample to analyze. Pittman, Zardkoohi, and Esty and Caves all consider only industries (in Zardkoohi's case, firms) that are politically active. Though this does allow the use of least squares estimation, using only active PACs is clearly a nonrandom criterion for inclusion in the sample. As Heckman (1976, 1979) shows, if variables, that determine the sample are correlated with variables used to test hypotheses in the sample, ordinary least squares coefficient estimates are biased and inconsistent. It is quite likely that variables influencing whether firms in an industry establish a PAC correlate with variables affecting how much money that PAC then spends. (We show later that this is exactly the case.) Boies's sample includes noncontributing firms, but he chooses his specification with ordinary least squares and then reestimates the "best" model using TOBIT. TOBIT regressions force the model for predicting existence of PACs, and the model predicting the spending of PACs, to have the same set of coefficients (see Appendix A).

The second research design problem is the size of the samples used in these studies. Esty and Caves study 300 firms aggregated into just 35 industry-level observations. Zardkoohi and Boies consider 415 and 500 firms respectively. Pittman does not report the number of industries included in his sample. Further, each paper analyzes only one or two cross sections of contributions. In cases where more than one cross section is examined, regressions are run separately, with no attempt to exploit the time-series property of the data for increased efficiency of coefficient estimates.

We address these research design issues in our empirical work, aggregating thousands of firms into 124 different industries, over five election cycles. We test for, and then reject, the implicit restrictions built into the TOBIT model. The alternate technique first proposed by Heckman (1976) is shown to be a better means of accounting for sample selection.

INVESTMENT IN POLITICAL ACTION BY INDUSTRY

It will be useful to lay out the theoretical perspective that informs the subsequent empirical work.

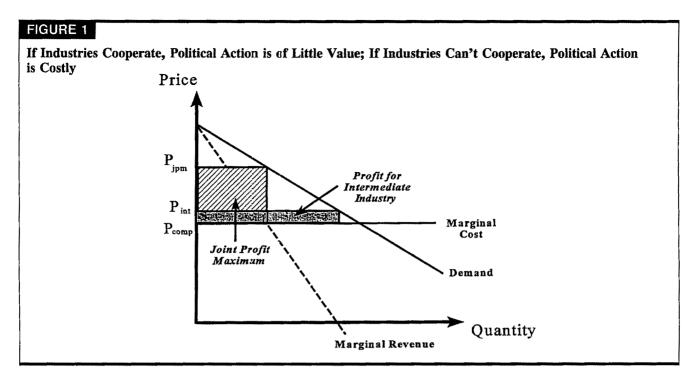
Theoretical Perspective

The following model captures variations in the likely gains to political action and the costs in achieving those gains, across industries. The maintained hypothesis underlying our work is that in a world of no organization costs, industries would maximize total profits earned by all producers. But as Olson (1965) points out, groups that might form profitably may be prevented from doing so by the free-rider problem. Consequently, the explanatory variables we shall discuss are mostly controls for obstacles to collective action.

We shall analyze corporate PACs as if they were directly controlled by their sponsors in cooperation with other corporations in the industry, to see how much explanatory power this reductionist approach has. Under the assumption that there are no impediments to agreeing on or enforcing collective action, industries would achieve the joint profit maximum. In this idealized setting, we can write the following profit function for industry i.⁵

$$\Pi_{i} = P_{i}(Q_{i}, I_{i})Q_{i}(L_{i}, K_{i}) - r_{i}(I_{i}, K_{i})K_{i} - w_{i}(I_{i}, L_{i})L_{i} - \phi I(\phi)_{i}, \quad (1)$$

where Π = joint industry profits; P = price of output for whole industry (no cheating), partly politically determined; Q = quantity of output produced, given production technology and amounts of K and L; I = political influence of the industry, an industry-specific function of ϕ ; 6 L = quantity of labor used for



production; K = quantity of capital used for production; r = price of capital (interest rate), partly politically determined; and w = price of labor (wages), partly politically determined. The firm invests in all activities with a rate of return greater than the riskadjusted cost of borrowing. Political activity will become attractive if its net present value is greater than zero. It is worth noting, however, that unlike labor and somewhat like capital investments, political activity may be "lumpy." Though many firms have established PACs, many more have not. As Eismeier and Pollock note, the costs of establishing and running a PAC (including reporting and accounting requirements), are significant, so that the minimum investment required to make contributions is substantial (1988, 101). In the empirical work that follows, we estimate the activity decision first as a PROBIT. We assume that labor and capital markets are segmented and that the industry can affect P, r, and w through government influence. For example, the auto industry can affect P by supporting protectionist legislation, w by gaining power over unions, and r through regulation of financial markets and monetary policy.

We must account for an important additional factor to make the model useful for application to actual corporate political activity. The industries with the lowest costs (in terms of acting collectively) of using political action are also the industries for which political action holds the lowest marginal benefits. Industries that can act as a unit are quite likely able to achieve most benefits of cartelization on their own. As Posner points out: "The demand for regulation . . . is greater among industries for which private cartelization is an unfeasible or very costly alternative—industries that lack high concentration and other characteristics favorable to cartelizing. They

lack good substitutes for regulation" (1974, 345). Figure 1 illustrates this trade-off. If the industry can act perfectly collusively, privately, it can charge $P_{\rm JPM}$ and achieve the private, joint profit maximum. Political influence might increase profits further, by protecting the industry's output for competition, increasing government purchases, or making cheaper loans or labor contracts available. But these gains may be smaller than the potential gains of more competitive industries: the cost of increasing profits rises as the industry nears the joint profit maximum (Denzau and Munger 1986; Peltzman 1976).

A purely noncooperative industry, with no private collective agreement on price and output, charges price equal to marginal cost (the flat line in Figure 1) and makes zero economic profits. This industry serves a similar market to-and faces the same input costs as-the joint profit-maximizing industry discussed above, but $\Pi = 0$, because of collective action problems. The benefits to political action for a competitive industry are large, but the costs of overcoming the collective action problem if firms are atomistic are larger still. Finally, the "intermediate" industry charges P_{INT} . Political action is more costly for intermediate industries than for perfectly organized industries, but the benefits are far larger than for privately colluding groups because intermediate industries cannot reach P_{IPM} on their own.

To summarize, we claim that if the marginal costs and benefits of political activity are accounted for, the pattern of contributions can be explained. The null hypothesis against which our theory is tested is that corporate PAC contributions by an industry are explained by idiosyncratic internal imperatives, not external investment-oriented goals. This view is mostly associated with the interview results of Bauer, Pool, and Dexter (1963); Handler and Mulkern (1982);

and the work of Wright (1985) on trade associations. Rejection of the null will not imply that American business is a monolith and acts only on its collective interests, of course. Rather, if the null hypothesis is rejected, it simply means that corporations in industries in our sample respond systematically and predictably to political incentives in choosing their levels of political activity.

Finally, it is useful to distinguish PACs sponsored by corporations and the trade association PACs that represent the same industry. Previous work on trade association PACs (Grenzke 1989; Wright 1985) or work that has mixed trade association and corporate PACs (Wright 1989, 1990) is useful as research on interest groups generally but tells us little about the general pattern of corporate political action. Corporate PAC contributions at the industry level are important in their own right, for at least three reasons.

First, trade associations are subject to the statutory limit of \$10,000 per election cycle (primary and general) per candidate, while PACs representing individual corporations in the same industry can make many contributions of up to \$10,000. Consequently, trade associations often represent groups with many diffuse members. For example, in 1988, the National Association of Realtors spent \$3 million, the American Medical Association \$2.7 million, and the Association of Trial Lawyers \$1.9 million. Trade associations are less important in industries with a few large firms, however: the "motor vehicles and equipment" industry gave more than \$1.8 million, with just the Ford, Chrysler, and General Motors PACs spending \$1.2 million. Second, collective action by corporations is a substantive and interesting problem in its own right. Umbrella organizations such as trade associations are one means by which collective action problems can be overcome, but only because the costs of organizing and raising funds are internalized. We address collective action directly, by explaining differences in industries' ability to overcome free-rider problems and induce corporations to act. Finally, government actions often affect the entire industry. The Environmental Protection Agency regulates use of chemicals by dry-cleaning firms, the National Highway Traffic Safety Administration requires all autos to meet safety standards, and an increase in the oil depletion allowance affects the net profits of all oil producers. Firms that can act as an industry will be much more effective at deflecting or diluting unfavorable regulation. Since the pattern of contributions observed just one firm at a time could be consistent with either pure cooperation or purely atomistic behavior, we use the *industry* as the unit of analysis.

Definition of Industry

The intuitive definition of an industry is just "all firms that produce similar products." Of course, this requires a definition of *similar*. The Census of Manufactures has a range of industry definitions, or standard industrial classifications (SICs), from two-digit (very broad) to five-digit (extremely specific). For

example, SIC 20 is "food and kindred products," SIC 202 is "dairy products," and SIC 2024 is "ice cream and frozen desserts." Similarly, SIC 50 is "durable goods—wholesale," 504 is "professional and commercial equipment supply—Wholesale," and 5045 is "computers and software—wholesale." We need to use a definition of industry that includes enough observations that the sample can be analyzed statistically yet ensures that the firms we will call an industry produce similar outputs. To strike a balance between these two concerns, we have chosen the three-digit SIC level as the definition of industry.

The variables we propose to use to explain industry political activity will be defined and discussed. These variables are designed to capture the determinants of variations in political activity across industries, that is, the marginal benefits of political action to the industry, the marginal costs of political action to the industry, and the collective action costs faced by the firms in the industry in organizing politically.

Empirical Model

To specify an empirical model embodying these ideas, we shall consider each of three sets of explanations in turn: direct political influences on demand for the industry's output, political influences on underlying market conditions, and the costs of collective action. A brief description of the variables we use to measure these concepts is given here. (For a complete description of the definitions and sources of the variables, see Appendix B.)

Direct Political Influence on Demand. The federal government can affect the demand for the industry's output directly by being a consumer and indirectly by being a regulator. The size of average sales to the government and whether the industry is subject to regulation (a dummy variable, equal to 1 for 17 industries in the sample subject to specific federal regulation) are the most important indicators of direct government impact. Our measure of regulated industry is taken from Pittman (1977). Average government sales is the mean (in millions of dollars) amount of sales by firms in each industry and is taken from Bureau of the Census (1985a-d). These variables represent the degree that prices are determined by a political process and may be sensitive to political influence. Consequently, both regulation and government sales should cause higher contributions.

Amelioration of Adverse Market Conditions. As noted, for an industry to profit from favorable regulation, both its input and output markets should be protected from unexpected shocks and from potential competitors. Government can also indirectly affect both the level and variability of demand for the industry's product and competition from imports. Our measure of variability of demand that the industry faces is standard deviation of industry profits, based on time-series data on real corporate profits (again, in 1988 dollars) provided by the Bureau of Economic

Analysis at the two-digit SIC level. The variable used is the standard deviation of profits over the four years preceding each election in the sample. We expect this variable, which measures variations in the riskiness of profits across industries, to be positively associated with political activity. The reason is that risky industries may try to get government either to underwrite their losses (as was the case in the bailout of Chrysler and the savings-and-loan industry) or to pursue policies that will support demand. Examples of the latter include price floors on commodifies or the extension of "most favored" trade status to nations with many consumers and many large human rights violations. Import share is a measure of the industry's exposure to foreign competition, which might be moderated by government intervention. Ceteris paribus, industries with higher import shares will use the political process more heavily, at least up to some critical level of imports. The source for import share is Clark, Kaserman, and Mayo 1990.

If the industry uses specialized labor, political influence on Congress and the National Labor Relations Board can affect wages industry by industry. The federal government also influences the riskadjusted price of borrowing funds through management of the money supply, but no good industryspecific measures of interest rates are available. Consequently, our measures of input prices are real wage rate and BAA bond rate. Wage is the annual inflation-adjusted wage rate for production workers in the industry; the data come from the CITIBASE economic data bank. This variable is available only at the two-digit SIC level and only for industries 20-49. Bond rate is available annually and is the average rate charged on BAA-rated bonds as reported by CITIBASE. All else equal, high-wage industries should be more politically active, and all industries should be more politically active in years when interest rates are high.

Collective-Action Problems. Although, for the sake of exposition, we assume that the industry faces no collective-action problems and can achieve the joint profit maximum of a pure cartel, differences in the costs of collective action are the key variables in the choice to seek political influence. There are two general categories of measures to be included: measures of the extent to which the industry has common interests, and measures of the industry's ability to overcome the free-rider problem.

We include two measures of the extent to which the industry has common interests: geographic concentration and diversity of the products that the industry produces and sells. *Geographic concentration* is a measure of whether an industry has good alternatives to contributions as means of gaining political influence (e.g., lobbying, direct appeals to voters). If an industry is located entirely in a single state, its employees are an important voting bloc for legislators in that state when compared to an industry with the same number of employees spread uniformly over all 50 states. We use a Herfindahl index to measure

dispersion of industry sales across states; greater geographic concentration produces a higher index. The expected sign for the coefficient on this variable is negative, since votes can substitute for contributions. Diverse industry is a dummy variable selecting heterogeneous industries, in terms of number of different products. The variable equals 1.0 for the 30 industries that have more products than two standard deviations above the mean. To the extent that industries with greater product diversity have weaker common interests among firms, we expect contributions to be lower. Both geographic concentration and diversity are adapted from COMPUSTAT.

We account for the ability of the industry to overcome the free-rider problem in three ways: firm size, industry structure, and government reaction to collusive arrangements. The first of these is the most obvious; in Olson's (1965) framework, the key element is the absolute size of benefits to the individual. Since in our analysis the "individual" is the firm, we use average private sales (measured in millions of 1988 dollars) as a measure of firm size. The relative cost of raising funds falls with larger firms, at least over a certain size range. Political action committee money has to be raised from employees in sufficient quantities to have some effect; larger firms have more employees and are better able to pay the fixed start-up and accounting costs for establishing a PAC. The data on mean firm size within industries is from COMPUSTAT. The industry structure variable is the four-firm concentration ratio (hereafter, concentration). Concentration is the proportion of total industry sales accounted for by the four largest firms (the data used here are from the Bureau of the Census 1985a). In more concentrated industries, the smaller number of similar-sized firms simplifies formation and enforcement of agreements over contribution strategies and raise total contributions, at least over some range. As industries become highly concentrated, firms presumably have sufficient market power to earn maximal profits with less and less political help. Cumulative antitrust indictments measures the ability of the industry to solve problems of collective action privately without attracting attention from the Justice Department and Federal Trade Commissions, which try to ensure private collective action problems stay unsolved. Other factors equal, antitrust actions make firms more sensitive to the political environment and raise contributions. Our measure comes from Miller 1989 and Miller, Shughart, and Tollison 1990.

The variables described above account for several different effects. Industry structure for example, proxies for both the costs and the benefits of political action. Sales to the government measure both the dependence on political action and the existence of direct ties with specific agencies or departments. Our goal has been not to discriminate among theoretical models of political investment (which do not yet exist) but to specify a simple theoretical model consistent with the empirical model we estimate. Such an approach allows us to test the value of an organiza-

Corporate PAC Contributions to Congressional Candidates in 124 Industries (Three-Digit SIC), 1978-86								
	TOTAL	LARGEST SINGLE-INDUSTRY	INDUSTRIES MAKING NO	AVERAGE NONZERO				
YEAR	CONTRIBUTIONS (\$)	CONTRIBUTION (\$)	CONTRIBUTION	CONTRIBUTION (\$				
1978	8,846,904	1,409,900	57	132,043				
1980	15,268,120	1,873,400	41	183,935				
1982	17,690,584	2,045,600	39	208,124				
1984	20,281,440	2,480,500	. 39	238,605				
1986	24,721,880	2,715,400	40	294,308				

tionally constrained investment conception of corporate political action.

DATA AND TECHNIQUE

Our dependent variable is corporate campaign contributions to all U.S. House candidates, taking industries as the unit of observation. These data are available by individual PAC from the Federal Election Commission. We merged individual corporate PAC contributions with firm data from COMPUSTAT for the five election cycles 1978–86 and then aggregated up to the SIC three-digit industry level. The data collection process captures 50%-60% of the corporate PACs (listed separately from the trade association records, there being no overlap between the two) listed in the Federal Election Commission data for each election and about 80% of the dollar amount of contributions. The 124 industries in the sample are mainly in the 200-399 (manufacturing) range along with a few 100-level (agricultural and mining) and 400-level (services) industries. Industries in the sample and their contributions levels are listed in Appendix C.

Table 1 shows the growth of political activity in the 124 industries comprising the sample. Total contributions almost triple in real terms (all dollar figures are given in 1988-dollar-equivalent units) over the sample, from around \$8.8 million in 1978 to almost \$25 million in 1986. Both the intensity of participation by active industries, and the number of industries making contributions, grows significantly. The largest total contribution by a single industry almost doubles, from \$1.4 million in 1978 to \$2.7 million in 1986, and the average nonzero contribution rises from \$132,000 to \$294,000. Further, the number of industries making no contributions falls from 57 to 40.

The values of the variables are calculated for two different samples: the full 124-industry sample and the subsample of 110 industries where complete data for the full model are available. The two samples appear generally similar (see Appendix B for summary statistics and precise variable definitions), though industries with the largest average government sales, most antitrust indictments, and most variable profit streams are excluded in the smaller

sample, reducing the sample variation of these regressors.

The methodological problem is that the dependent variable is limited below, at zero. Industries where it is not profitable to make political contributions or where the collective-action problem cannot be solved make no contributions. Zero contributions occur in 216 (35%) of the 620 observations in the full 124-industry-and-five-election sample. Such a probability mass at a single point implies biased and inconsistent ordinary least squares estimates. A possible solution is to throw out the zero-contribution observations and then estimate an equation on the rest of the sample with OLS. This approach eliminates the masspoint problem but introduces sample selectivity bias, again yielding biased and inconsistent coefficient estimates.

We can write the HECKIT (Heckman 1976, 1979) model as follows:

$$z_i^* = X_{1i}\beta_1 + \epsilon_{1i} \tag{2}$$

$$y_i = X_{2i}\beta_2 + \epsilon_{2i}. \tag{3}$$

Equation 2 is the sample selection equation where z_i^* is an unobserved index of the propensity to undertake political action. We do not observe this index but do observe an indicator variable z_i that equals 1 if $z_i > 1$ 0 and equals 0 otherwise (in our case, $z_i = 1$ if any firms in the industry have active PACs). Equation 3 is the contributions equation where y_i is observed only when $z_i = 1$. If $X_1 = X_2$, $B_1 = B_2$ and $e_1 = e_2$, then the model is identical to the TOBIT model. On the other hand, if these conditions do not hold, then TOBIT imposes invalid restrictions on the data. We show in Appendix A that the TOBIT restrictions are rejected in our sample. Consequently, we use the HECKIT estimator to allow B_1 to differ from B_2 , while continuing to assume $X_1 = X_2$. We assume that e_1 and e_2 are bivariate normal random variables with correlation coefficient ρ .

The basic problem with just estimating equation 3 with OLS is that the expected value of the error term is both nonzero and correlated with the independent variables; that is,

$$E[y_i \mid z_i = 1] = X_{2i} B_2 + E[e_{2i} \mid z_i = 1].$$
 (4)

TABLE 2				
Determinants of Corporate PAC Contributions to	House Candidates	by 124	Industries.	1978-86

	EQ. 1: P	ROBIT	EQ. 2: HE	CKMAN ^a
VARIABLE	COEF.	S.E.	COEF.	S.E.
Constant	-1.167	.45*	-241,199	103,076*
Trend	.117	.47*	40,915	9,741**
Private Sales	.227	.03**	21,198	2,924**
(Private sales) ²	001	.0002**	-64	16**
Government sales	.708	.35*	132,302	15,402**
(Government sales) ²	012	.01	-2,320	358**
Regulated industry	.260	.26	167,292	39,831**
Diverse industry	-1.172	.38**	-394,612	92,199**
Concentration ratio	.037	.017*	3,549	3,697
(CR) ²	00053	.00021	_ 38	38
Cumulative antitrust	.010	.017	9,316	3,682*
S.D. of profit	3.105	.61**	222,127	71,195**
Geog. spread	002	.000034	-390	89**
IMR			182,087	60,294**
Dependent variable	0/	1	dolla	ars
Sample	62		40-	4
Log-likelihood/R ²	-25	1	.60	1

"Selectivity-bias-corrected OLS with corrected standard errors (Heckman). Data are on 124 three-digit industries for five elections expressed in 1988 dollars; 404 nonzero observations and 216 at 0. To allow more significant digits, the sales data used in these regressions are in one hundred's of millions of 1988 dollars.

Greene gives the expectation of the error term in equation 4 as

$$E[e_{2i} | z_i = 1] = \rho \sigma_y \{ f(Z_i)/1 - F(Z_i) \},$$

where $Z_i = (X_{1i}B_1/\sigma_{e1})$ and f and F are the normal and cumulative normal densities, respectively (1993, theorem 22.4).

Intuitively, accounting for the sample selection process generates another regressor, $f(Z_i)/1 - F(Z_i)$, which needs to be incorporated into the contributions equation. This variable is generally called the inverse Mills ratio (IMR), or the Hazard rate. The HECKIT technique first estimates a PROBIT model on equation 2 (using the (0,1) indicator variable z) to calculate an estimate of the IMR for each data point. Then this equation is estimated using OLS:

$$y_i = X_{2i} B_2 + g I M R + v_{2i}$$
.

HECKIT provides consistent estimates of B_2 , though the OLS standard errors are biased. We compute the consistent standard errors as derived by Greene (1993, 713).

RESULTS

The sample is a pooled time-series cross section, so our first task is to find out whether the time-series pooling assumption is valid. In all the regressions reported here, there is a trend variable (trend = 1 in 1978 and goes up by 1 in each period to 5.0 in the 1986 election) included as a regressor. The variable imposes the restriction that the intercept shifts an equal

amount between each election. This restriction can be tested by replacing the trend variable with four dummy variables that let the intercept shift freely and then comparing the fit of the two equations with a likelihood ratio test. In all cases, the appropriateness of the trend variable cannot be rejected, even at the .10 confidence level.

We also test for the stability of the other coefficients in the regression over time using likelihood ratio tests. The hypothesis that a pooled sample is appropriate at the .05 level, once the trend intercept shift is allowed, cannot be rejected. Thus the annual estimates do pool over the years in our sample, and treating these data as a single sample is legitimate.

Equations 1 and 2 carry out the Heckman procedure (Table 2). The PROBIT model from equation 2 provides an estimate of the IMR that is used as an additional regressor in the reported OLS regression. The selectivity-corrected OLS model shows all variables correctly signed and significant at the .01 level except concentration. 12 This regression explains 60% of the variation in contributions across politically active industries. Also, the coefficient on the IMR is positive and significant in equation 3, suggesting that selectivity is a factor in these data. In other words, OLS estimations on this sample would produce biased estimates of the true coefficients. The PROBIT model (equation 1) correctly classifies 80% of the cases in the sample. The misclassified cases are split almost evenly between inactive industries predicted to be making contributions (61 cases) and contributing industries predicted to be inactive (64 cases).

Our technique allows a comparison of the effects of

 $p \le .05$. $p \le .01$.

TABLES

ı	TABLE 3			
l	Adding Import Cor	npetition. Interest Ra	ites, and Wage Rates	in a 110-Industry Subsample

	EQ. 1: I	PROBIT	EQ. 2: HE	CKMAN ^a	EQ. 3: 1	PROBIT	EQ. 4: HE	CKMAN
VARIABLE	COEFF.	S.E.	COEFF.	S.E.	COEFF.	S.E.	COEFF.	S.E.
Constant	-1.818	.53**	-326,652	87,574**	-2.078	821*	-700,220	128,954**
Trend	.099	.51	29,002	7,287**	.072	.052	29,187	7,136**
Private sales	.230	.031**	18,786	2,108**	.238	.031**	17,779	1,991**
(Private sales) ²	002	.000042**	90	11.6**	001	.00018**	-89	11**
Gvt. sales	1.108	.42**	160,011	40,024**	1.190	.43**	148,410	39,055**
(Gvt. sales) ²	231	.12*	28,875	8,134**	242	.12*	29,964	7,885**
Regulated industry	.243	.33	137,784	33,606**	.187	.35	103,870	36,318**
Diverse industry	-1.315	.41**	-192,803	65,357**	-1.453	.44**	-168,490	58,913**
Conc. ratio	.067	.022**	9,529	3,145**	.072	.023**	7,459	3,161*
(CR) ²	00083	.00025**	-119	30.6**	-8.6E-4	.00026**	-105	32**
Cum. antitrust	.084	.031**	9.063	2.75**	.099	.034**	7,185	2,681**
S.D. of profit	2.295	.65**	257,523	84,712**	2.637	.83**	-7,445	106,357
Geog. spread.	002	.00036**	-179	73*	002	.00037**	-12	7.2
Import share	<u> </u>	_	_		040	.02	9,134	2,828**
(Imp. share) ²	· —		_		8.6E-4	.00041*	-199	67**
Real wage	_	_		***	040	.055	33,469	8,223**
BAA bond rate	_	_		-	.062	.034	7,052	4,670
IMR	_		96,197	44,742*		_	91,362	44,136*
Dependent variable	0/	/1	dolla	ırs	O,	/1	dolla	ars
Sample	55	50	357	7	55	50	35	7
χ^2/R^2	27	76	.68	3	28	34	.7	8

"Selectivity-bias-corrected OLS with corrected standard errors (Heckman). Data are on 110 three-digit industries for five elections expressed in 1988 dollars; 357 nonzero observations and 193 at 0. In order to generate coefficients with more significant digits, the sales data used in these regressions are entered as one hundred's of millions of 1988 dollars.

the explanatory variables on two different decisions by industries: the decision to form a PAC and then the decision on the amount that the PAC will spend. The PROBIT equation estimates the probability that the industry has at least one active PAC, and the selectivity-corrected ols contribution equation predicts the total amount contributed. This approach allows the separate impacts of the independent variables to be identified at each stage. For example, concentration is strongly related to the decision to create a PAC but turns out to have no influence on the amount contributed. The significant concentration coefficients in the PROBIT equation show that higher industrial concentration raises the marginal probability of positive political activity in any industry, up to a concentration ratio of 35%. In contrast, regulation and antitrust indictments are insignificant in the PROBIT equation but positive and significant in the selectivity-corrected contribution model. Regulated industries contribute almost \$170,000 more than an (otherwise similar) unregulated industry, and each antitrust indictment raises industry contributions by \$9,300, other factors held constant.

Consider now the rest of the results in equation 2. The significant trend coefficient implies that average industry contributions are increasing about \$41,000 per year, after controlling for the effect of our independent variables. Private and government sales both increase contributions at a decreasing rate (i.e., coefficients on squared terms are negative), but the

effect of government sales is dramatically larger compared with the results from the earlier method. Fifty million dollars in private sales produces about \$10,600 in contributions, while an equal amount of sales to the government is associated with \$65,500 of industry PAC money. 13 The industries with the most extreme heterogeneity of product lines contribute almost \$400,000 less than average. Geographically concentrated industries also contribute significantly less, suggesting that they can use alternate avenues of exercising political influence. Each one-standarddeviation increase in geographic concentration lowers contributions by over \$85,000. Finally, industries with greater variability in past profit streams contribute significantly more to political campaigns. Every one-standard-deviation increase in this risk-proxy raises contributions about \$40,000.

These results show that the simple organizationally constrained investment model of political activity has considerable explanatory power. We shall add the import, wage, and interest-rate variables to the model and consider the results in more detail. These variables are available only for the 110-industry, 550-observation subsample.

Table 3 adds import share, import share squared, real wages, and the BAA bond rate to the regressors from Table 2, with the sample reduced by 14 industries because of missing data. Equations 1 and 2 reestimate the two-equation selectivity model using the original variable set from the previous analysis.

 $p \leq .05$. $p \leq .01$.

The only differences from the Table 2 results are that concentration now significantly affects both contribution levels and the probability of participation, and the coefficient on the square of government sales is now positive and significant. The other variables have the same signs as before and are statistically significant.

Equations 3 and 4 in Table 3 contain our results with the four new variables added. Again, compare the standard errors on variables between the PROBIT equation estimating the probability of forming a PAC and the selectivity bias corrected OLS equation estimating the amount of contributions. Regulation and wages are insignificant in the participation decision yet positive and significant for explaining variations in the level of political contributions across industries. On the other hand, geographic concentration and the variability of industry profits are highly significant in the participation equation but much less important in the contribution regression. The BAA bond rate is significant at the .10 level in the PROBIT model, but not in the contribution model. All other variables are significant, at least at the .05 level, in both equations.

We can use the estimates in equation 4 to describe the effect that changes in the variables have on industry political contributions in this subsample. The average level of private sales in this subsample is 834 (meaning the average firm in the average industry has sales of \$834 million), which implies contributions of \$142,000. An increase of one standard deviation raises contributions to \$397,400, an increase of more than a quarter of a million dollars. The contribution-maximizing level of sales is \$9.9 billion (only one sample industry has larger average firm sales), implying \$888,000 in contributions.

The average level of government sales is \$19 million, which is associated with \$29,300 in marginal political contributions. A one-standard-deviation increase in government sales raises predicted contributions to \$153,800, an increase of more than 400%. These results suggest that at the margin, having government as a customer increases industry political activity more than having private-sector customers. The hypothesis that the two types of sales have identical effects on contributions can be rejected at the .01 level. Regulation also raises contributions more than \$100,000 per election, and every antitrust indictment increases industry contributions by \$7,200. This last variable ranges from 0 to 35 in this subsample. Greater government involvement with firms either greatly increases the return to political activity or dramatically lowers the costs of collective action by having politicians readily available to coordinate industry activities.

The effect of industry concentration on contributions is increasing up to 35%, just below the sample mean of 41%. At 35% concentration, predicted marginal contributions are about \$132,000. A one-standard-deviation increase above 35% (to 53%) reduces the predicted effect of concentration on contributions to only \$99,500. At levels of concentration above 70%,

the marginal effect of concentration on contributions is negative. ¹⁴

Import share also significantly affects industry PAC contributions. At the sample mean penetration of 10.3%, contributions due to imports are predicted to be about \$73,000. A one-standard-deviation increase (to 21.6%) in import penetration raises contributions to about \$104,000. Beyond the 23% import level, the marginal effect of import competition on industry contributions begins to fall, reaching zero at around 46%

Industries that pay higher wages also make greater political contributions. Every one-dollar increase in industry wages increases PAC contributions by around \$33,000. Every percentage point increase in interest rates raises each industry's contribution about \$7,000, though this effect is not strongly significant. In general, however, our prediction that higher input prices increase political activity is strongly supported in these data.

Geographic concentration is still negative, though only marginally significant in this sample. Each one-standard-deviation increase in geographic concentration lowers contributions about \$26,000. The diverse industry dummy is still negative and significant, with a coefficient of -\$168,000. These results continue to show the importance of organizational factors on industry political activities.

Finally, the coefficient on the IMR is still positive and significant, showing the importance of our technique's correction for sample selection bias. The general fit of the model is quite good. The R-squared of .79 in the contribution equations means that we are explaining about 80% of the variation in contributions across both industries and time. The PROBIT model predicting the existence of political activity correctly classifies 81% of the 550 cases in the sample. Industry political activity can be explained quite well with a basic model accounting for profit opportunities and free-riding costs.

EMPIRICAL EXAMPLES FROM 1986

We shall consider in more detail the contribution patterns of some selected industries. First, Table 4 contains summary statistics on the independent variables grouped by amount of contributions in 1986 into four categories: no contributions (40 cases), positive contributions less than \$100,000 (45 cases), contributions between \$100,001 and \$500,000 (23 cases), and contributions greater than \$500,000 (16 cases). The results show that industries making very similar contributions are often quite heterogeneous. While average sales rises as contributions rise, the standard deviations in each cell for both private and government sales are very large. There is little evidence of a trend across contribution categories for any of the other variables, though both real wages and percentage regulated jump in the highest contribution category. There are however, regulated industries that

TABLE 4			
Average Industry Ch	aracteristics b	y Contribution	Group, 1986

VARIABLE	NO CONTRIBUTIONS	\$1-\$100,000	\$100,000- \$500,000	>\$500,000
No. of industries	40	45	23	16
Private sales	207	460	1,012	2,788
	(220)	(453)	(810)	(3,033)
Government sales	5.7	7.0	15.0	406
	(23)	(11)	(33)	(1,259)
Regulated indust. (% of group)	2.5	11	17	50
	(15)	(31)	(39)	(51)
Diverse indust. (% of group)	5.0	2.2	4.3	12.0
	(22)	(15)	(21)	(34)
Concentration ratio	·37.5	39.4	44.1	48.1
	(16)	(15)	(16)	(23)
Antitrust actions	3.05	3.11	6.17	4.56
	(7)	(4)	(7)	(6)
Geographic concentration	870	1,100	3,400	1,350
	(1,000)	(1,000)	(6,000)	(1,000)
Import share ^a	12.29	10.14	10.27	5.93
	(13)	(12.5)	(7.5)	(5.9)
Real wage ^a	10.16	10.48	10.62	12.42
	(2.25)	(1.75)	(1.46)	(1.55)

Note: Numbers in parentheses are the standard deviations of the group averages. "Available for the 110-industry sample only.

make few contributions and low-wage industries that make large ones.

Second, we provide five examples of how our model works to predict industry contributions in Table 5. We examine the predicted contributions for the cigarette, aircraft, motor vehicle, trucking, and petroleum-refining industries made by equation 4 in Table 3; that is, the entries give the dollar amount of predicted contributions attributed to each individual independent variable. That number is expressed in thousands of 1988 dollars and is computed by multiplying the value of each variable for each industry by the relevant regression coefficient. We have rounded off the figures to the nearest thousand dollars.

In the cigarette industry (Table 5, col. 1) there are a few, large, geographically dense firms. Actual contributions from this industry in 1986 are \$603,000; our regression predicts contributions of \$642,000. Private

TABLE 5

Sources of Predicted Contributions (thousands of dollars) in Five Selected Industries, 1986

VARIABLE	CIGARETTES	AIRCRAFT	MOTOR VEHICLES	TRUCKING	PETROLEUM REFINING
Private sales	877	279	484	85	870
Government sales	.0	2,165	171	23	867
Regulated	.0	.0	.0	104	.0
Concentration	-126	52	86	131	130
Antitrust	28	28	163	129	86
Import share	.0	49	115	.0	64
Real wage	461	455	438	418	506
Geographic conc.	-90	-17	-93	-21	-36
Fixed factors ^a	-508	508	~508	-508	508
Total predicted contributions	642	2,523	856	374 ⁶	2,083
Actual contributions	603	2,486	1,092	402	2,715

Note: The technique of using specific industries as examples is drawn from Bartels (1991). The predictions here come from the coefficients estimated earlier in Model 4, Table 3.

"Sum of those effects that do not vary cross-sectionally and are therefore fixed for all industries. These include intercept, trend, and interest rate variable. In this case, the value of the IMR is large enough to raise the predicted contribution significantly (\$13,000) above what the reported variables predict, an indication of the importance of what would otherwise be selectivity bias in the estimation procedure.

sales account for \$877,000, concentration for -\$126,000, and geographic concentration for -\$90,000. The aircraft industry, contributed \$2,486,000 in 1986, while our model's predicted contribution is 2,523,000. Here, sales to the government is the driving factor, accounting for over 2 million dollars in campaign contributions. Note that neither of these industries is significantly regulated by Pittman's 1977 standards.

The third industry in Table 5 is motor vehicles, or SIC category 371 (vehicle bodies assembly, not final sales). Actual 1986 contributions by this industry are \$1,092,000; our predicted contribution is \$856,000. In this industry, antitrust trouble and import penetration account for about \$280,000 of the total contribution. Sales concentration and geographic concentration offset each other, and government sales explains most of the rest of the prediction. Trucking and courier services is examined in Table 5, column 4. Regulation, sales concentration and antitrust trouble are the main factors driving our predicted contribution of \$374,000. The actual industry total is \$402,000. Trucking is geographically dispersed, has low average sales to the government, faces no import competition, and has very small average firm size compared to the other industries in our example. Yet trucking contributions approach those of the cigarette industry

Finally, consider the petroleum refining industry. Petroleum refiners contributed \$2,715,000 in 1986, the largest industry contribution in our data. Our model "only" predicts a contribution of about \$2.1 million. A large average firm size (\$870,000) and significant sales to the government (\$867,000) are the largest factors affecting contributions here. However, sales concentration, antitrust trouble, import competition, and high wages also have a significant predicted effect. Our \$700,000 underprediction may be indirect evidence supporting Evans's claim that "oil PACs [are] the most ideological" of all corporate PACs (1988, 1048). While our simple cost-benefit model does predict a very large contribution, petroleum refiners contributed even more.

The tenacious reader will have noticed that industry wages are associated with large contribution numbers but have received little emphasis in our exposition of Table 5. That is because there is not much variation in wages across the five chosen industries. Consider a case of two industries similar in many respects but with different average wages. The sawmill industry (SIC 242) contributed \$262,000 in 1986, the papermill industry, (SIC 262) \$583,000. Both are made up small firms with few sales to the federal government, and neither has much history with the antitrust authorities. Yet there exists (and our model predicts) about a \$300,000 difference in their political contributions. Of the difference, \$104,000 is due to higher wages in the papermill industry, and another \$104,000 is attributed to the fact that papermills are significantly regulated (according to Pittman) and sawmills are not. The rest of the difference is attributed to the greater concentration of the papermill industry.

CONCLUSION

We have investigated the industrial organization of corporate PACs over the period 1978-86. Though the allocation of PAC monies among candidates for political office has been extensively studied, we are the first to present even a simple empirical model of corporate political activity and then investigate which industries contribute and how much they give. Our empirical results support two theoretical propositions: (1) the evidence is consistent with the notion that industries follow investment-oriented goals in political activity, and (2) the ability of industries to achieve these goals is conditioned by both the benefits that political action brings and the costs of achieving cooperation and organizing collective action. Between 60% and 80% of the variation in contributions is explained in the empirical models. Coefficient signs and significance levels are robust, showing general consistency across time, changes in the sample, and inclusion or omission of independent variables.

We feel our evidence is sufficient to reject the null hypothesis that variations in total PAC contributions are based on idiosyncratic internal goals, at least at the corporate PAC level. Wright's (1985) influential work only directly addresses the behavior of five large trade associations. We argue that attaining real political leverage requires concerted contributions by a group of PACs and therefore study corporate PACs at the industry level. However, it is still an open question whether the factors we use to explain corporate PAC contribution levels can explain total trade association PAC activities. We plan in future work to study the relationships between corporate PACs and industry trade associations.

In closing, one further caveat seems in order. Because our focus has been on organization and collective action problems, we have taken the industry as the appropriate unit of observation. The definition of industry, however, has been quite narrowthe collection of corporations within a three-digit SIC code. A more appropriate definition of industry might well include unions and trade associations drawn from the same SIC, reflecting a functional, rather than class, definition. A corporation manufacturing automobiles may share more political goals (trade restrictions on imports, blocking more stringent emission standards, etc.) with the United Auto Workers than with corporations that export semiconductors. If this (currently untested) claim is correct, the traditional division of corporations and unions into political adversaries is not a useful one. The industry, more broadly construed to include all economic actors with an interest in the industry's fortunes, may ultimately prove to be the more useful unit of analysis.

APPENDIX A: THE TROUBLE WITH TOBIT

This appendix discusses the TOBIT technique, which is widely used in political science to solve the problem of a censored sample (particularly in the study of campaign finance; see, e.g., Grier and Munger 1993). Though there are alternate approaches (see Greene 1990, chap. 21 for a good discussion), TOBIT is the most common choice. The problems with TOBIT are not widely understood, however. In particular, TOBIT requires (in our case) that the determinants of both the participation and contribution decisions have exactly the same effects.

The classic TOBIT model (Tobin 1958) assumes that there is an index variable y^* , linearly related to the independent variables,

$$y_i^* = X_i \beta + \epsilon_i. \tag{A1}$$

The observed dependent variable y_i is generated as follows:

$$y_i = y_i^* \text{ if } X_i \beta + \epsilon_i > 0; \text{ otherwise } y_i = 0.$$
 (A2)

Given that the errors are independently and identically distributed normal, the coefficients of this model can be estimated using maximum likelihood.

As we note in the text, TOBIT models force a single set of coefficients to explain both whether the dependent variable will be observed and its value conditional on being observed. Cragg (1971) points out that it is not obvious that the occurrence of limit

observations and the regression model for the observed data should be so closely related. Cragg assumes that the probability of limit observations is independent of the regression model. Greene (1993) shows that this restriction in the TOBIT model can be tested by comparing the fit of the TOBIT model with the combined fit of (1) a PROBIT model predicting the probability of a nonlimit observation and (2) a truncated normal regression on the nonlimit data.

Table A-1 presents some preliminary results, and Greene's test of the TOBIT model versus Cragg's variant. The ordinary least squares regression in equation 1 and the TOBIT in equation 2 have the same signs and significance levels. Equations 3 and 4 (Table A-2) decompose the participation decision and the contribution level into distinct parts. A PROBIT equation is used to predict political activity and then a truncated regression on the 404 positive contributions. The results of these separate procedures can now be compared with the TOBIT model, which assumes the two components are determined in a single equation. Summing the loglikelihood functions of equations 3 and 4 and comparing that sum to the TOBIT log-likelihood produces a chi-squared statistic of 444, which strongly rejects the TOBIT assumption. In the truncated contributions regression, all variables are correctly signed and significant at the .01 level. While none of the substantive conclusions in our empirical work would change if we used the TOBIT model throughout, the model is clearly rejected by the data. The PROBIT and selectivity-corrected least squares regressions used in the text provide better information.

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Testing	the	Tobit	Restrictions

TADIE A 1

INDEPENDENT		EQUATION					
VARIABLES	1 (OLS)	2 (TOBIT)	3 (PROBIT)	4 (TRUNCATED)			
Constant	-68,020	-151,609	216	-1,760,330			
	(21,801)**	(47,675)**	(.26)	(271,656)**			
Trend	23,709	33,685	.120	106,914			
	(6,425)**	(8,772)**	(.047)*	(10,586)**			
Private sales	11,568	14,069	.196	19,452			
	(639)**	(832)**	(.026)**	(1,926)**			
Government sales	27,601	33,470	.641	61,368			
	(2,763)**	(3,630)**	(.33)*	(8,754)**			
Regulated industry	156,234	168,418	.327	698,545			
	(26,891)**	(35,160)**	(.26)	(127,240)**			
Diverse industry	-151,056	330,420	1.157	-785,631			
	(50,690)**	(79,428)**	(.38)**	(262,752)**			
Concentration ratio	1,134	1,685	008	12,335			
	(545)*	(736)**	(.0053)**	(2,452)**			
Cumulative Antitrust	5,205	8,160	.009	38,440			
	(2,303)*	(3,022)	(.17)	(9,636)**			
S.D. of profit	149,138	287,391	3.132	529,299			
	(47,800)**	(60,631)**	(.6)**	(150,797)**			
Geographic spread	-169	−454	002	-857			
	(40)**	(62)**	(.0003)**	(271)**			
Dep. var.	dollars	dollars	0/1	dollars			
Sample size	620	620	620	404			
R²/log-likelihood	.56	5,728	265	5,241			

Note: Data are on 124 three-digit industries for five elections expressed in 1988 dollars. There are 404 nonzero observations and 216 at 0. Numbers in parentheses are standard errors. To generate coefficients with more significant digits, the sales data used in these regressions are entered as one hundred's of millions of 1988 dollars.

 $p \leq .05$

^{••*} $p \le .01$.

APPENDIX B: DESCRIPTIONS AND SOURCES FOR VARIABLES

The industrial concentration, regulation, and import share variables do not vary over time, and the interest rate variable does not vary cross-sectionally. Wages and profits only vary at the two-digit level; that is, in any year all three-digit industries with a common two-digit home are assigned the same wage and profit numbers.

Total Industry Contributions (Dependent Variable). Each corporation on the COMPUSTAT data base was matched against the Federal Election Commission master list of corporate PACs (Nonparty Spread File, Reports on Financial Activity) for each election year in the sample (1978, 1980, 1982, 1984, 1986). If a corporation was not identified as having a PAC, the contributions for that firm were listed as zero; otherwise, the total contributions of the PAC to House candidates were listed. Many PACs were neither identified as having corporate sponsors, nor had an identifiable company name revealed in their registered PAC title. These PACs were dropped from the sample. The firm-level data were then aggregated up to the three-digit SIC industry level, using the SIC codes listed on COMPUSTAT, by adding up all the contributions within an industry.

Average Private Sales. Obtained from COMPUSTAT; item no. 12 on industrial database. Gross sales (the amount of actual billings to customers for regular sales completed during the period) reduced by cash discounts, trade discounts, and returned sales and allowances for which credit is given to customers. Value is in millions of dollars and is annual (as are all COMPUSTAT variables). Our variable represents the average of sales over firms in the three-digit industry.

Average Government Sales. Data on proportion of industry total sales that went to the federal government were obtained from Russell Pittman; original data came from Bureau of the Census 1985a–d. This proportion was multiplied by industry total sales to yield an estimate of dollar sales to the federal government.

Regulated Industry. Dummy variable, equal 1 if industry is regulated. Obtained from Pittman 1977 for four-digit industries. If the three-digit industry contained at least one regulated four-digit industry, it was considered regulated.

Concentration Ratio. Data were obtained from Russell Pittman; actual values represent corrected (by Department of Justice) concentration ratios for 1982, with original data coming from Bureau of the Census 1985a.

Cumulative Antitrust Indictments. Data were obtained from Gerry Miller and used in Miller 1989 and Miller, Shughart, and Tollison 1990. These data were originally compiled by Miller under a Freedom of Information Act request from the Department of Justice and Federal Trade Commission. Miller tracked indictments of all firms and made the data available to us at the four-digit SIC level. We aggregated up to the three-digit level and created a moving (cumulative) sum of industry violations.

Geographic Concentration. The variable is a Herfindahl index, calculated for each industry j as Σi (Sales_i/Sales)², where sales in the ith state are calculated using the COMPUSTAT state IDs for each corporation in the sample, and the denominator of the ratio is total industry j sales.

Industry Profits. The variable is "corporate profits without adjustments" contained in the Bureau of Economic Analysis Income and Wealth Division's gross product originating (GPO) by industry, table 6.1F. These data are annual, available in an industry grouping very similar to the two-digit SIC. We adjusted these data for inflation using industry-specific price indices and gave the appropriate two-digit profit figure to each three-digit industry in the sample.

Diverse Industry. Source data come from COMPUSTAT Industrial Segment File, where the number of segments (roughly, products) are reported by each firm, up to a maximum of 10. We averaged these data by industry (mean = 1.7, standard deviation = .65) and then defined a "diverse industry" as one with mean number of products more than two standard deviations above the sample average; that is, all industries whose firms average more than three product lines are coded 1.0 and the others .00.

Import Share. Data were obtained from Don Clark and used in Clark, Kaserman, and Mayo 1990. Import share is defined as total

TABLE B-1				
Data Summary				
VARIABLE AND NO.			MINI-	
INDUSTRIES	MEAN	S.D.	MUM	MUM
Contributions				
124		326,940		
110	130,110	313,250	.00	2,715,400
Avg. private				
sales (millions)	796.37	1.646	2.81	18.519
110	834.32	1,729		18,519
Avg. government	007.0L	1,720		10,010
sales (millions)				
124	49.82	363	.00	5,087
110	18.94	69	.00	629
Concentration				
Ratio (4-firm)				
124	40.81	17.08		93.00
110	41.08	16.20	10.50	86.00
Cum. antitrust	0.54	4.00	00	44.00
124	2.54 2.56	4.03 3.55	.00.	44.00 35.00
110 S.D. of Profit	2.50	3.33	.00	35.00
124	.151	.18	.007	2.24
110	.138	.12	.007	.57
Regulated indust.	.,,,,			10.
124	.14	.35	.00	1.00
110	.12	.32	.00	1.00
Diverse indust.				
124	.048	.214		1.00
110	.045	.208	.00	1.00
Geographic Herf.				
index				
124	3,929			9,925
110 Wave rate	3,868	2,222	636	9,925
110	10.57	1.91	6.23	15.14
Import share	10.57	1.31	0.23	15.14
110	10.28	11.33	.00	56.10
BAA bond rate				55.10
110	12.55	2.13	9.45	15.34

imports divided by total shipments for the industry. Original data were four-digit SIC level, aggregated up to the three-digit level.

Wage Rate. From CITIBASE data bank. Variables are "average hourly earnings of production workers" (LE6M20-LE6M39), corresponding to two-digit SICs. We adjusted for inflation using the gross national product deflator and gave the appropriate two-digit "real wage" figure to each three-digit industry in the sample.

BAA Bond Rate. From CITIBASE data bank. Values are the annual average rate charged on BAA rating bonds, from 1978 through 1986.

APPENDIX C

This appendix gives information about the industries used in the sample and their total contributions over the period 1978–1986. The industry definitions and descriptions are taken from the Department of Commerce Standard Industrial Classifications, using 3-digit industries. The contribution totals are the sum of contributions reported to the Federal Election Commission, by corporate PAC, aggregated into these industry definitions. The dollar amounts were all transformed to real (1988 dollar) values, and then summed from 1978–86.

Miscellaneous Metal Ores (0) 131 Petroleum & Natural Gas Extraction (1,608) 138 Oil & Gas Field Services (978) Heavy Construction, Except Highway (0) 162 Meat Products (531) 201 202 Dairy Products (157) 203 Preserved Fruit & Vegetables (0) 204 Grain Mill Products (838) 205 Bakery Products (1,225) Sugar & Confectionary Products (70) Fats & Oils (505) Beverages (2,879) Cigarettes (1,358) 208 209 Misc. Foods & Kindred Products (25) 211 221 222 Broadwoven Textile Mills (cotton) (483) Broadwoven Textile Mills (manmade) (70) Knitting Mills (26) 227 Carpets & Rugs (42) Men's & Boy's Furnishings (142) Women's & Misses' Outerwear (0) Women's & Children's Undergarments (0) Sawmills & Planing Mills (863) 239 Misc. Fabricated Textile Products (3) 242 243 Millwork, Plywood & Structural (906) 245 Wood Buildings & Mobile Homes (0) 251 Household Furniture (0) 252 Office Furniture (7) 253 Public Building & Related Furniture (0) Misc. Furniture & Fixtures (0) 261 Pulp Mills (0) Paper Mills (2,817)
Paperboard Containers & Boxes (10) 262 Paperboard Mills (76) 263 265 271 Newspapers (0) Periodicals (109) 273 Books (0) Commercial Printing (214) 276 Manifold Business Forms (9) 277 Greeting Cards (0) 278 Blankbooks & Bookbinding (11) Printing Trade Services (0) 279 281 Industrial Inorganic Chemicals (586) 282 Plastics Materials & Synthetics (1,144) 283 Drugs (4,580) Soap, Cleaners & Toilet Goods (328) Paints & Allied Products (355) Industrial Organic Chemicals (35) 287 Agricultural Chemicals (516) Misc. Chemical Products (184) 291 Petroleum Refining (10,525) 295 Asphalt Paving & Roofing Materials (0) 301 Tires & Inner Tubes (167) 302 Rubber & Plastic Footwear (117) 306 Fabricated Rubber Products (46) 314 Nonrubber Footwear (1) 321 Flat Glass (0) Glass & Glassware, Pressed/Blown (640) 322 323 Products of Purchased Glass (0) 324 Cement, Hydraulic (228) 325 Structural Clay Products (109) Pottery & Related Products (0) 327 Concrete, Gypsum & Plaster Products (164) Misc. Nonmetallic Mineral Products (109) Blast Furnace & Basic Steel Products (1,654) 332 Iron & Steel Foundries (92) 333 Primary Nonferrous Metals (1,349) Nonferrous Rolling & Drawing (149) 334 Nonferrous Products 335 336 Nonferrous Foundries (8) 339 Misc. Primary Metal Products (0) Metal Cans & Shipping Containers (215) Plumbing & Heating (Nonelectric) (21) Cutlery, Hand Tools & Hardware (18) 342 343 345 Screw Machine Products (0) 346 Metal Forgings & Stampings (33) 347 Metal Services (51) 348 Ordnance & Accessories (0) 349 Misc. Fabricated Metal Products (0) 351 Engines & Turbines (134) 352 Farm & Garden Machinery (2,627) Construction & Related Equipment (915) Metalworking Machinery (48) Special Industry Machinery (22) Computer & Office Equipment (711) 355 356 General Industrial Machinery (736) Refrigeration & Service Machinery (148) 358 357 359 Industrial Machinery (137) 361 Electric Distribution Equipment (990) Household Appliances (434) Household Audio & Video Equipment (267) Electrical Industrial Apparatus (261) 363 Electric Wiring & Lighting Equipment (0) Communications Equipment (996) 364 365 366 367 Electronic Components & Accessories (450) 371 Motor Vehicles & Equipment (4,013) Misc. Electrical Equipment & Supplies (145) 369 372 Aircraft & Parts (8,505) 373 Ship & Boat Building & Repair (0) Railroad Equipment (16)
Guided Missiles, Space Vehicles & Parts (4,233) 375 Motorcycles, Bicycles & Parts (25) Misc. Transportation Equipment (0) 379 Measuring & Controlling Devices (472) 382 381 Search & Navigation Equipment (1,934) 385 Ophthalmic Goods (0) Medical Instruments & Supplies (47) Photographic Equipment & Supplies (200) 387 Watches, Clocks, Watchcases & Parts (0) Toys & Sporting Goods (153) Costume Jewelry & Notions (0) Jewelry, Silverware & Plated Ware (40) 394 Pens, Pencils, Office & Art Supplies (0) Misc. Manufactures (0) 396 399 401 Railroads (2,636) Public Warehousing & Storage (0) Trucking & Courier Services (Nonair) (1,026) 421 422 Deep Sea Foreign Freight Transportation (171) 451 Air Transportation, Scheduled (2,992) Pipelines, Except Natural Gas (69) Radio & Television Broadcasting (48) Telephone Communications (5,116) Electric Services (2,580) Combination Utility Services (1,355) Gas Production & Distribution (2,492)

Notes

The authors thank David Austen-Smith, Dennis Coates, Tyler Cowen, James Friedman, Russell Pittman, Robert Tollison, and Richard Wagner for helpful comments on earlier drafts. Don Clark, Gerry Miller, John Mayo and Russell Pittman provided useful data.

 An exception is Hall and Wayman 1990. They find that the use of time by committee members is strongly correlated with contributions.

- 2. Andres (1985) examines the Fortune 500 firms for 1980 (though his sample inexplicably contains only 426 observations). Masters and Keim (1985) and Humphries (1991) examine the Fortune 1000 manufacturing firms in the year 1982. Masters and Keim add about 150 nonmanufacturing firms, as well. Grier, Munger, and Roberts (1991) examine the percentage of firms with PACs in 96 manufacturing industries (regardless of size, provided the firms are listed in the Compustat database) in 1984. McKeown (1994) examines 118 firms at three different points in time.
 - 3. There are a variety of measures of industry structure in

the economics-industrial organization literature, with the most common being the four-firm concentration ratio. This ratio can be defined as the sum of total sales of the largest four firms in the industry, divided by the total sales of the entire industry. Obviously, a concentration ratio of one means that there are only four (or fewer) firms. A concentration ratio of .10 would mean that the largest four firms account for only 10% of the total sales (and hence market power) of the industry.

- 4. Boies chooses 10 variables by looking at pairwise coefficients, then derives a four-variable model from stepwise OLS regressions, in effect using two pretest estimators, each of which are biased and inconsistent.
- 5. It is worth pointing out that the empirical model we will estimate based on this abstract profit function is a reduced form, capturing the aggregate effects of variables that may effect both the costs and the benefits of political action. The main reason we specify a formal profit function at all is to justify inclusion of the input price variables, which are then implied as direct comparative statics results of the model.
- 6. We subscript $I(\phi)_i$ because different industries will exercise very different levels of influence depending on the political support among voters—and other industries—for the policy or service in question. This function is designed to focus just on the marginal impact of PAC contributions by the
- 7. The exact mechanism by which influence is obtained through contributions is left unspecified here. Two possible modeling approaches are Denzau and Munger 1986 and Baron 1994.
- 8. The evidence on corporate PACs (Eismeier and Pollock 1988; Handler and Mulkern 1982, Matasar 1986, Morrison 1992), based on both perceptions of researchers and interviews with company executives and lobbyists, would appear to indicate that corporate PACs are fairly directly controlled by the sponsoring organization. This top-down control is quite different from the looser and more anarchic setting in which trade associations work (Bauer, Pool, and Dexter 1963; Wright 1985). Further, as Grier and Munger (1991) show, the allocations of corporate and trade association PAC contributions are statistically distinct and must be analyzed separately.
- 9. The general form of the Herfindahl index is the sum of the squares of some fractions that sum to one. It is widely used to measure market structure (the sum of the squares of the market shares of all firms). In this case, the "share" variable is the proportion of total sales that each state represents for the industry.
- 10. As Snyder (1989) shows, the issue of optimal dispersion of "power" is a difficult one. The issue depends in part on the ability of a group (e.g., in this case, of employees at manufacturing plants) to control a single district or to spread among several districts. The probability of being pivotal in multiple districts is higher if the workers are spread out; the probability of being pivotal in at least one district is obviously higher if all the workers are in one district. It may not be true, therefore, that geographic diversity has a monotonic effect in either direction.
- 11. The average number of product lines per industry is 1.7. The cut-off point for being considered a "diverse" industry is 3.0. We ran regressions using product lines directly. In a linear specification, it is negative but insignificant; adding the square turns the linear coefficient positive; and the square coefficient is negative and significant. Since it seems strange to estimate a quadratic function for a variable that ranges from 1 to 4.1, we used the dummy variable described in the text.
- 12. Standard OLS estimates are inconsistent, and the standard errors are incorrect (see Greene 1993; Heckman 1979). The standard errors reported here are corrected, using the procedure Greene derives from Heckman's consistent estimator. All our regressions were estimated with LIMDEP version 5.1 and checked with SHAZAM version 7.0.
- 13. The average level of sales to the federal government in the full sample is \$50 million.
- 14. These results are broadly similar to those of Grier, Munger, and Roberts (1991), who find that the influence of

concentration on the number of PACs in an industry is nonlinear, first increasing and then decreasing, for the year 1984.

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REPORT FROM THE LABORATORY: THE INFLUENCE OF INSTITUTIONS ON POLITICAL ELITES' DEMOCRATIC VALUES IN GERMANY

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he unification of Germany revives several questions about the future of Germany's democracy. Given the socialist—authoritarian background, how supportive are East Germany's elites of liberal democratic rights? Has the socialist—democratic experience instilled into elites a social egalitarian conception of democracies? In what ways, if at all, do elites support direct democracy procedures? I examine political elites' conceptions of democracies in the united Germany in 1991, using a survey of 168 parliamentarians from the united parliament in Berlin. I find that the socialist and parliamentary institutions in the East and the West, respectively, have substantially influenced elites' conceptions of democracies in Germany, leading to a value divergence across the East—West boundary. Yet the findings also suggest that a partial value convergence in terms of liberal democratic rights among postwar elites has taken place. The results support an institutional learning theory, but they also suggest that support for liberal democratic values has been diffused into East Germany.

I think entirely in dialectical terms.

—an East Berlin CDU parliamentarian on evaluating political issues

The unification of Germany revives several questions about the future of Germany's democracy, most of which analysts of the West German political system had thought were resolved. Especially throughout the 1980s, a consensus evolved among scholars that the West German mass public and elites had developed a democratic political culture (Baker, Dalton, and Hildebrandt 1981; Hoffmann-Lange 1985; Roth 1976; Weidenfeld and Korte 1991). However, the socialist East German system in many ways represented a continuation of Germany's antidemocratic past-one-party-controlled, with few opportunities for elites and the public alike to express their opinions freely (Childs, Baylis, and Rueschemeyer 1989; Krisch 1985; Ludz 1972; Schweigler 1975). Hence, given the importance of political elites in the political reconstruction of the unified Germany, I shall analyze the democratic values of the German political elite at the end of 1991.

There is no agreement on one fixed blend of democratic values necessary to make a political elite democratic. But the extent of elites' commitment to basic democratic rights, their views on social egalitarian principles, and the extent to which plebiscitarian procedures are favored by elites partly define the character that a democratic system develops (Dahl 1989; Pennock 1979). These three core values (also referred to as elites' conceptions of democracies)1 constitute important elements in elites' ideology (Collier and Mahon 1993; Putnam 1973). Thus, given the 50year history of experience in an authoritarian regime, how supportive are East German elites of liberal democratic rights (e.g., free elections, freedom of discussion)? Has the socialist experience instilled into East German elites a social egalitarian conception of democracy? Given the extensive involvement of ordinary citizens in the 1989 revolution and the emphasis of socialist ideology on direct democracy, how do East German elites view plebiscitarian procedures? Using a survey of East and West German parliamentarians from the united parliament in Berlin, I shall attempt to address these questions.

Although my focus is on legislative elites' conceptions of democracies in Berlin shortly after unification, the theoretical significance reaches beyond Berlin and, indeed, beyond Germany; for it addresses the perennial question of whether political institutions can reshape elites' core values. Previous elite studies remain inconclusive on this issue because they focus either on individual-level or systemic source of elites' political values. For example, individual-level studies highlight elites' professional norms as sources of parliamentary behavior (Herzog et al. 1990; Searing 1982, 1985) and elite attitudes (Aberbach, Putnam, and Rockman 1981; Roth 1976); or else they document the influence of elites' generational experience on elite polarization over such New Politics issues as environmentalism (Dalton 1987).2 Systemic studies, in contrast, assume that the unique regime experience of elites within a given polity (e.g., political institutions, level of economic development) contributes to a similar outlook on core political values (Di Palma 1990; Higley and Burton 1989; Higley and Gunther 1992).3

Despite the recognition that both individual-level and systemic factors are important sources of elite values, these two perspectives have not been combined into a single study. Individual-level studies generally do not incorporate national-level factors into their empirical analyses of elites' values and frequently treat core values as an "uncaused first cause" (Putnam 1973, 149). In contrast, systemic approaches cannot generate the detailed empirical data of survey studies about elites' values because

such information for longer time periods (frequently centuries) is not available.⁴ The main reason for this gap is undoubtedly the absence of proper cases where both types of factors can systematically be varied, measured, and controlled for. I suggest that the conditions created by Germany's division and unification offer the opportunity for a synthesis by examining the influence of political institutions (controlling for individual-level East–West differences) on elites' conceptions of democracies.

Thus one of my theoretical objectives is to analyze the diverging impact of socialist and parliamentary institutions on elites' democratic values. But a second is to consider the possibility that democratic ideals have spread to East Germany. In fact, there are good reasons to argue that East and West Germans have not merely drifted apart during the past four decades. Analysts of democratic transitions in Southern and East-Central Europe have argued that support for liberal democratic values may develop even within authoritarian states (Di Palma 1990; McDonough, Barnes, and Lopez-Pina 1986; Weil 1989). While these studies point to a broad range of potential sources of value diffusion (e.g., mass media, economic development, education), the diffusion argument predicts that democratic values have developed in East Germany despite the authoritarian political environment. I will therefore also examine the possibility of a value convergence in terms of elites' conceptions of democracies.

In seeking to address these issues, I interviewed 168 parliamentarians from the united Berlin parliament (79 from East Berlin, 89 from West Berlin) between September 1991 and June 1992. I view Berlin's division and unification as a quasi laboratory, where the effect of socialist and parliamentary-democratic systems on elites' conceptions of democracies can be studied. Although a focus on the Berlin parliament lacks the external validity of a national elite study, it has the advantage of maximizing the range of background factors that are being kept constant (e.g., regional differences in political traditions within Germany between South-West and North-East). Further, where available, I will present evidence that the Berlin results are generalizable to the national legislative elite. I therefore believe that the Berlin study provides a first important step toward assessing systematically the influence of political institutions on elites' understanding of a democ-

First, I argue that a process of institutional learning may have substantially shaped elites' democratic ideals in East and West Germany. Based on this discussion, I shall propose a series of testable hypothesis. Next, I shall present evidence that corroborates several (though not all) institutional-learning hypotheses. Finally, I shall examine the extent to which democratic values have been diffused into East Germany and highlight the main implications of this study.

INSTITUTIONAL LEARNING AND DEMOCRACY IN GERMANY

The establishment of a parliamentary democracy in West Germany and a socialist system in East Germany in 1949 marks the beginning of a monumental social experiment; for in both systems, the political culture had to be reconstructed to conform to the new regimes. In West Germany, public opinion and elite studies documented that the undemocratic forces during the 1950s had to be reckoned with (Almond and Verba 1963; Edinger 1960; Meritt and Meritt 1970). But the longevity of democratic institutions and the economic success during the 1950s and 1960s also began to transform, slowly but steadily, mass and elite political values. By the mid-1970s, analysts of public opinion and political elites alike began to document the growing acceptance of democratic values, especially among citizens born during the postwar years (Baker, Dalton, and Hildebrandt 1981; Boynton and Loewenberg 1973, 1974; Hoffmann-Lange 1985; Kaase 1971; Kuechler 1992). In East Germany, while Communist party elites could partly benefit from the authoritarian traditions in German society (Dahrendorf 1967), the task was to imbue East Germans with socialist ideals (Glaessner 1989). Although the developments in East Germany are less well documented than those in West Germany, initial analyses seem to indicate that the East German system was at least partially successful in achieving this goal (Bauer 1991; Friedrich 1990; Noelle-Neumann 1991).5

Theoretically, the division-and-unification of Germany allows analysts to examine whether a political system, if established for a sufficient time period, can reshape a nation's political culture. Almond and Verba's (1963) seminal Civic Culture analyzes the culture-institution nexus by primarily examining the value prerequisites of democratic systems. But Verba, in an important essay, explicitly suggests that institutions may also have a significant influence on individuals' values because "what an individual believes about the political process is learned from observation of that process" (1965, 533). The central supposition is that elites' (and publics') exposure to a particular institutional configuration substantially influences the basic political values of individuals, a theme that became increasingly important in the wake of the success of West German democratic institutions (Almond and Verba 1980; Conradt 1974; Rogowski 1974). From this perspective, individuals' regime experience may be viewed as a combination of elites' exposure to the regime ideology (the official values and norms) and regime practices (the operating norms). At the center of this perspective, then, is the premise that the process of institutional learning molds individuals' democratic values.6.

The effect of newly established institutions on elites' political values is an especially important issue in contexts where abrupt and fundamental changes in political systems occur, such as the ones in Central Europe after World War II or in 1989. Are new institutional arrangements able to imbue citizens with those ideological values that institutions need to endure? Political culture theory predicts that under these conditions individuals' existing value predispositions will initially delay the impact of new institu-tional learning (Almond and Verba 1963).⁷ Yet, one would also expect that individuals will slowly but steadily incorporate the new institutional values and norms into individuals' preexisting values. In particular, generational experience may become a "major basis for subcultural differentiation" (Eckstein 1988, 798). That is, cohorts born after a regime transition are only exposed to the new institutional configuration and should provide less resistance to the new institutional values and norms than generations that were also shaped by pretransition institutions (Sigel 1989). This perspective therefore anticipates that elites' core values in West and East Germany should be influenced by the democratic and socialist regime experience because at least two elite generations have been socialized under two fundamentally different systems since about 1949. In short, the institutional learning perspective, combined with the notion of generational change, predicts that the East German socialist system, despite its collapse, shaped elites' ideological predispositions.8

Democratic Ideals in East and West Germany

Based on these considerations, the division of Germany after World War II and the 1989 unification can be used to study the effect of parliamentary-democratic and socialist institutions on elites' political values under quasi experimental conditions. Since a diverse range of factors (e.g., social culture, historical traditions) that usually varies across nations remains constant, these systemic factors can be largely ruled out as sources of East–West variations in elites' democratic values. Further, all parliamentarians represent constituencies within the city of Berlin, which means that variations of regional interests (e.g., between cities and more rural areas) are unlikely to enter parliamentarians' views on the topics discussed with them (a "most–most" similar systems design).

Undoubtedly, there are other national-level variations between East and West Germany that our study design cannot adjust. For example, the West German economy has clearly outperformed East Germany's command economy. However, although this East-West difference may influence elites' views on specific economic decisions (e.g., where to invest resources for a particular project), it is less plausible to view differences in economic capacity as sources of ideological differences across the East-West boundary. Recognizing that the design only approximates but does not duplicate—experimental conditions, I submit that variations of elites' ideological values, if they exist, must be attributed to the different institutional configuration that elites from East and West Berlin were exposed to.

Following the work of political theorists (e.g., Dahl

1989; Held 1987; Pennock 1979), I examine elites' democratic values from three different angles and propose six hypotheses about the expected East-West and generational differences. Briefly, liberal parliamentary democracies emphasize individual political rights in their conceptions of democracies, such as the right to participate in politics and the right to form an opposition (what Dahl terms polyarchy). Second, socialist democracies emphasize the egalitarian distribution of economic goods and link the ideal-typical democracy to the egalitarian provision of economic goods (Held 1987). Finally, a direct democracy model stresses the development of plebiscitarian procedures to expand the direct involvement of citizens (Barber 1984; Dahl 1989). These three different conceptions are not mutually exclusive, but the particular emphasis on these values defines elites' idealtypical democracy.

Based on these variants and the institutional learning argument, the first expectation concerns elites' support for liberal democratic rights. While the ideological and institutional foundations of the German Democratic Republic (GDR) differed from previous authoritarian regimes, several analysts maintain that the authoritarian structures of the socialist East German state resemble past authoritarian institutions (Dahrendorf 1967; McCauley 1983). For example, East German elites were socialized in a political environment that penalized the open and regime-critical exchange of political ideas. In addition, the official rhetoric of the GDR regime did not nurture political competition or independent mass involvement in politics. In contrast, West German elites have been socialized into a political system that encourages political competition and citizens' participation in politics. Thus, the first proposition focuses on elites' support for liberal democratic rights:

Hypothesis 1. East German elites are less supportive of liberal democratic rights than West German elites.

Furthermore, postwar elites in West Germany were socialized under democratic conditions, whereas prewar elites also experienced the authoritarian influence of the Third Reich. In contrast, prewar and postwar elites in East Germany were primarily exposed to authoritarian systems. Therefore, the effect of institutional learning should reflect generational differences within West Germany but not in East Germany (Eckstein 1988):

HYPOTHESIS 2. Postwar elites are more committed to democratic rights than prewar elites in West Germany, but not in East Germany.

The socialist system in East Germany permanently exposed East German citizens to an ideology that closely linked a democracy to the egalitarian distribution of economic goods. This ideological message was reinforced in schools or through the minimal opportunity to own private property (Glaessner 1989; Rueschemeyer and Lemke 1989):

HYPOTHESIS 3. East, but not West, elites' conceptions of democracies stress social egalitarian elements.

The logic of generational experience implies a further proposition about the anticipated cohort differences regarding the understanding of democracy by members of parliament (MPs):

HYPOTHESIS 4. Postwar elites in East Germany, but not in West Germany, are more likely than prewar elites to emphasize the social egalitarian conception of democracies.

Hypotheses 5 and 6 concern East elites' views on direct democracy procedures. There are two reasons to believe that East Germans are more likely than West Germans to support plebiscitarian procedures. First, the extensive involvement of citizens during the 1989 revolution undoubtedly sensitized East elites to the importance of citizens' involvement in politics. Had it not been for the so-called Monday demonstrations of the East German public, the upheavals in East Germany during 1989 would likely have taken a different course. Second, while socialist systems did not endorse independent involvement of masses in politics, socialist ideology did endorse the direct mobilization of masses for socialist goals (Held 1987). While I will not be able to disentangle the relative importance of these processes, the logic of these arguments provides the basis for the fifth hypothesis:

Hypothesis 5. East German elites are more supportive of plebiscitarian components than West Germans.

Finally, while all citizens in East Germany experienced the collapse of their political regime, postwar elites should be particularly affected by this experience because younger citizens were disproportionately involved in bringing down the socialist system. In addition, the postwar cohort in East Germany was exclusively exposed to the mobilization tactics of the socialist regime. In West Germany, the New Politics literature suggests that postwar elites in West Germany should be more supportive than prewar elites of plebiscitarian procedures (Dalton 1992; Inglehart 1990):

HYPOTHESIS 6. Postwar elites in East and West Germany are more likely than prewar elites to endorse procedures that increase the direct involvement of citizens in politics.

These six hypotheses make the institutional-learning argument testable, but they also define the benchmark expectations to uncover value domains shaped by converging forces. That is, if I do not find differences where, on the basis of institutional learning, I expect them, I must explain their absence.

THE DATA

The data stem from a survey conducted between September 1991 and June 1992 with 168 (about 70%) parliamentarians of the united Berlin parliament. West MPs have been socialized under the West

German system, 10 and they have spent an average of about seven years in the Berlin parliament. In addition, there is no reason to believe that West MPs are unrepresentative in terms of their democratic conceptions of the general political elite in West Berlin. However, I suspect that the nature of the selection process of East Berlin MPs may have biased the composition of East Berlin MPs. Most importantly, one criterion in selecting candidates for the Berlin parliament was the absence of close ties to the old GDR regime. Individuals who were politically active in the GDR system were unlikely to be nominated for a legislative seat within the new political system. Even within the reformed communist party (PDS), it furthered a candidate's career after the transition not to hold visible political or social positions in the GDR. Undoubtedly, several lower-level functionaries of the GDR have joined the West German parties. Still, the contemporary elite in East Germany, on average, probably overrepresents the most democratic forces within the East. Although this bias suggests caution in generalizing my findings to East Germany's old political elite, it actually stacks the case against confirming the hypotheses because old GDR elites were most intensely exposed to the socialist learning process. Thus any East-West difference that this study produces might actually be larger if old GDR elites were represented in adequate proportions in the Berlin parliament.

The interviews are based on a structured questionnaire containing a combination of open-ended and closed-ended questions. The Appendix contains detailed information on the data collection process and discusses evidence that the interviewed MPs closely mirror key characteristics of the entire Berlin parliament.

RESULTS

I employed several measures in order to investigate elites' support for liberal democratic, egalitarian, and plebiscitarian principles. To measure MPs' support for liberal democratic rights, I asked MPs to indicate their agreement or disagreement with items reflecting basic principles of liberal democracies (Table 1). I first present the bivariate results, followed by multivariate analyses, because the bivariate results convey a detailed profile of East and West elites' democratic values which the multivariate analyses does not convey. The first three items (A, B, C) measure individuals' commitment to general democratic procedures, while the next three indicators present respondents with a trade-off between conflict and order (D, E, F) (Kaase 1971; Putnam 1973). There is virtually unanimous support for the most basic democratic rights (A, B, C). Yet the level of support for democratic rights is significantly reduced when they conflict with another goal-for example, moral or ethical considerations (D) or the stability of the political order (E). Furthermore, there is some evidence that elites in the East are somewhat more willing to curtail individuals'

MP	MPs' Support for Democratic Rights							
ME	MEASURES		WEST	GAMMA				
Α.	Every democracy requires a political opposition.	100.0	100.0	(a)				
В.	It is the primary duty of the political opposition to support the government, and not to criticize it.	8.9	13.5	.16				
C.	Every citizen has the right to demonstrate.	100.0	97.8	(a)				
D.	Freedom of opinion and discussion must be limited by moral and ethical consideration.	57.0	42.7	27				
E.	A citizen forfeits the right to demonstrate and to strike when he or she threatens the political order.	31.6	30.3	09				
F.	The freedom of political propaganda is not an absolute freedom, and the state should carefully regulate its use.	29.1	23.6	22				

Note: Entries are percentages of respondents who agree with statements. See the Appendix for question wording and recodes. N = 79 for East Berlin, 87 for West Berlin.

(a)Too little variation to compute gamma.

rights when order-related objectives conflict with liberal democratic rights. On the whole, while East German MPs are somewhat less supportive of these rights than West German MPs, both elite sectors evidence similar levels of support for liberal democratic rights.

Despite the agreement among East and West elites over basic political rights, it would be premature to conclude that East and West German elites are essentially alike. One shortcoming of closed-ended items is that they may mask considerable differences in elites' conception of an ideal-typical democracy. Accordingly, I asked respondents to define the core elements in their own words: "The term democracy is frequently used without further specifications these days. What seem to you personally the essentials of a democracy?"

This open-ended question elicited a wide range of responses, but the basic patterns can be summarized as indicated in Table 2. (The Appendix contains the original code categories and recodes used for this table). Since the later multivariate analyses use this question only to measure egalitarian ideals, this table also displays cohort differences within the East and West.

A large number of MPs mention at least one civil right (e.g., liberty, freedom of expression) as an important component of a democratic system, both in the East (64.6%) and the West (73.0%). Within the East, prewar generations are more likely to mention civil rights (77.1%) than postwar generations (55.9%), whereas there are no clear generational differences among MPs from West Berlin. Similarly, a quarter of East and West MPs mention at least one "government-by-the-people" item (e.g., popular control). Consistent with the results from the analysis of closed-ended indicators, both East and West MPs evidently value basic civil rights and popular control as important ingredients of a democracy.

Yet differences between East and West MPs emerge in regard to the social equality component. East MPs are substantially more likely than West MPs to mention the necessity for governments to provide social security to individuals. Over a third of East Berlin MPs argue that guaranteeing a just standard of living

TABLE 2							
MPs' Conceptions of Democracy, by Generation							
DEMOCRATIC							
COMPONENT	EAST	WEST					
Government by the people	26.6	25.8					
Prewar	24.4	26.1					
Postwar	29.4	25.6					
Active participation	17.7	12.4					
Prewar	13.3	13.0					
Postwar	23.5	11.0					
Direct democracy	25.3	2.2					
Prewar	13.3	.0					
Postwar	. 41.2	4.7					
Social equality	35.4	7.9					
Prewar	28.9	10.9					
Postwar	44.1	4.7					
Equality of opportunity	3.8	7.9					
Prewar	2.2	4.3					
Postwar	5.9	11.6					
Civil rights/limited government	64.6	73.0					
Prewar	77.1	69.6					
Postwar	55.9	76.7					
Institutions	40.5	60.7					
Prewar	51.1	58.7					
Postwar	26.5	62.8					
Political competition	20.3	30.3					
Prewar	22.2	32.6					
Postwar	17.6	27.9					
Societal competition	17.7	19.1					
Prewar	17.8	15.2					
Postwar	17.6	23.3					
Citizens' responsibility	8.9	7.9					
Prewar	6.2	13.0					
Postwar	11.8	2.3					
N, both generations	79	87					
Prewar	45 04	44					
Postwar	34	43					

Note: Entries are percentages mentioning one or more democratic component. Multiple responses were allowed. See the Appendix for details on the question wording and recodes.

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Support for Direct Democracy Procedures

	_	EAST	7	GAMMA		
REFERENDUM INITIATED	BERLIN	VOLKSKAMMER	BERLIN	BUNDESTAG		
A. By the parliament and legally nonbinding	35.4	35.0	18.0	12.0	.41	
B. By the executive and legally nonbinding	19.0	22.0	10.1	5.0	.31	
C. By a minority in parliament	49.4	26.0	24.7	10.0	.45	
D. By a majority in parliament	60.8	67.0	33.7	16.0	.46	
E. By the executive	45.6	35.0	19.1	6.0	.51	
F. By the people	78.5	71.0	64.0	40.0	.34	
Support C-F	32.0	(a)	9.0	(<i>a</i>)	.66	

Sources: Berlin data is from the Berlin survey; Bundestag data is from Herzog et al. 1990, 130; Volkskammer data is from Werner 1991, 431. (a)Entries are not available.

Note: Entries are percentages viewing each procedure as meaningful. See the Appendix for question wording and exact response categories. N = 79 for East Berlin, 87 for West Berlin.

or providing social security for everybody ought to be an important obligation of a democracy. In contrast, only 7.9% in the West mention this component. Again, the substantial generational differences within the East attest to the fact that while all East German elites were exposed to socialist regime messages, postwar elites were particularly receptive to them (hypothesis 4).

Differences between East and West MPs also emerge over the degree to which they support the use of plebiscitarian procedures. About a quarter (25.3%) of MPs from the East mention at least one direct-democracy procedure (e.g., referenda for all important decisions) whereas only 2.2% in the West mention such element. Within the East, the generational differences attest to the cumulative learning of individuals: both pre- and postwar generations are influenced by the people's revolution and the socialist emphasis on direct democracy, but younger elites who are less resistant to new experiences than older ones, are more strongly influenced by these factors (hypothesis 6).

Within the West, the weak generational differences regarding the plebiscitarian procedures are surprising, especially in light of the postmaterialism literature (Inglehart 1990). One possible explanation for the absence of generational effects may be seen in the powerful socialization effect on MPs of representative institutions, which leads even those MPs who initially favor plebiscitarian procedures to abandon these views as a result of their parliamentary work. As one Green parliamentarian put it when direct democratic procedures were discussed during the interview: "If you had asked me this ten years earlier, I would have been for it." This MP then discussed how his/her initial enthusiasm for plebiscitarian procedures faded as (s)he became confronted with "unreasonable" demands from the constituency. Although this must remain a tentative attempt to explain this unexpected finding, it is also consistent with the result that more West Germans (60.7%) than East Germans (40.5%), including the postwar cohort, mention the existent (representative) institutions of the West German constitution (elections, majority rule, etc.). Combined, the Green MP's conversion and the general focus of West elites on representative institutions point to the ability of these institutions to imbue all MPs with representative-democratic values.

East MPs' support for plebiscitarian procedures is equally pronounced when MPs evaluate a series of closed-ended questions about citizens' direct involvement in politics. Since these items were also asked in two surveys of the West German Bundestag and the East German Volkskammer, I may also compare Berlin MPs with national-level MPs (Table 3). The first two procedures are nonbinding referenda, whereas procedures C-F would oblige legislators to act in accordance with the outcome of a referendum. East MPs on the national level and in Berlin are considerably more likely to support plebiscitarian procedures, especially when the procedures obtain legally binding status. In fact, East Berlin elites frequently accompanied their rejection of the first two options with comments indicating their disagreement with the non-binding nature of these procedures. The proportion of elites supporting all four procedures (32.9% in East Berlin, 9.0% in West Berlin) reflects the different democratic visions that exist across the East-West boundary: in the West, plebiscitarian procedures are frequently seen as undermining democratic systems, whereas in the East, they are often viewed as a potential corrective to a detached political elite. 11

The similarity of the Berlin and national results in terms of the plebiscitarian procedures also provides partial evidence that the Berlin results tend to be generalizable to the national political elite. For the East-West differences are, if anything, even larger on the national level, at least for these indicators. No doubt, one would need a national survey, paralleling the Berlin study, in order to establish with full confidence the generalizability of the Berlin-based results. But in the absence of such a study, the similarities presented are persuasive.

Taken together, the analysis of East and West MPs' conceptions of democratic values reveals a different mix of democratic components. While all MPs frequently mention civil rights and political competition, a substantial proportion of East MPs primarily value direct democracy and social equality elements, whereas West MPs predominately specify (representative) institutions and competition among political and social groups. The differences emerge particularly between postwar elites, suggesting that both East and West German institutions and the peoples' revolution of 1989 molded elites' values. Thus both East and West elites support liberal democratic rights, but this commitment, while similar on the surface, is embedded in different democratic ideals.

SOURCES OF VALUE PREDISPOSITIONS: A MULTIVARIATE ANALYSIS

How well does the influence of institutional learning perform as a predictor in a multivariate analysis when other individual-level East- West differences are considered simultaneously? Three dependent variables are analyzed: a democratic-rights index based on the indicators from Table 1, an egalitarian democracy indicator based on an additive index of responses to the open-ended democracy question, and a directdemocracy indicator based on the items from Table 3. (The Appendix contains details on the construction of the indicators.) I use these three different measures in order to show that the results converge regardless of the indicator used. Further, the democratic rights and plebiscitarian indicators have been employed in previous studies so that I am able to compare previous results with the ones presented here.

An East-West indicator measures the direct effect of institutional learning on elites' democratic values. If this variable is a significant predictor of MPs' conceptions of democracy, it would signify the importance of regime experience independent of other East-West differences on the individual-level. Communist party membership is also employed to measure the influence of institutional learning on elites' democratic values. Presumably, communist party members were exposed more intensely to the institutional norms of the socialist system than other MPs (Dalton 1994). 12

It is also well-documented that individuals' religiosity tends to generate center-conservative political orientations, such as conservative economic or cultural issue positions (Lipset and Rokkan 1967, intro). Since religion is considerably more important in West than in East Germany, one must include this characteristic in the model. Further, elites' material-postmaterial value priorities are included because postmaterialists are more likely than materialists to support an increase of citizens' involvement in politics, favor broad citizen participation in politics, and (though to a lesser extent) sympathize with an egalitarian distribution of economic goods. Since East Germans are less postmaterialist than West Germans, one must

control for these value orientations in examining the influence of institutional learning on democratic values.

In addition to MPs' cohort membership, MPs' education must be included because education, like generation, reflects elites' regime experience (Weil 1985). Further, individuals with lower incomes may endorse egalitarian democratic ideals in order to improve their personal situation. Thus East German elites may support an egalitarian democracy because of their economic difficulties, not necessarily because of their regime experience. Finally, I analyze the potential impact of respondents' gender on their ideological predispositions because the East German state facilitated womens' efforts to join the workforce by providing, for example, access to day-care centers (Rueschemeyer and Lemke 1989). Therefore, female MPs from East Germany may endorse egalitarian democratic principles because of the perceived difficulties in pursuing a career path under the West German system, not because they have learned to endorse principally a broad range of egalitarian ideals.

Table 4 displays the results from an ordinary least squares analysis. For each dependent variable, I analyzed the equations separately for the East and the West in order to uncover potential differences of the impact of predictor variables. Then I applied the equation to the combined data in order to assess the impact of the systemic variable (controlling for individual-level variables) on elites' conceptions of democracies.

Several results are noteworthy. First, the regime membership coefficient is highly significant for the democratic rights index (beta = .17). Independent of East-West variations in sociodemographics, postmaterialist values, or religiosity, institutional learning influences moderately, though not dramatically, elites' support for general democratic norms (hypothesis 1). Second, the Communist party coefficient (beta = -.12), while statistically insignificant, still suggests that Communist party MPs (PDS) are slightly less likely than other MPs to endorse general democratic rights, a result that parallels the findings from a public opinion study in East and West Germany (Dalton 1994). Finally, elites' postmaterialist values also generate support for democratic rights, both in the East and the West. Unexpectedly (contra hypothesis 2), the postwar generation among East German elites is substantially more, not less, likely to support democratic rights than the prewar generation (beta = .24). The strength of the coefficient is particularly impressive in light of the fact that sociodemographic variables have in the past failed to predict elites' core values. (An explanation will be provided shortly). Moreover, the generational variable is also insignificant for West elites. Apparently, prewar elites in the West have been sufficiently socialized by democratic institutions so that any cohort differences over democratic rights that initially might have existed disappeared.

Parliament members' egalitarian democratic conceptions are strongly influenced by the process of institutional learning. The strength of the regime

IADLE 4		
Predicting MPs'	Democratic	Values

	DEMOCRATIC RIGHTS			EGALITARIAN DEMOCRACY			DIRECT DEMOCRACY		
INDEPENDENT VARIABLES	EAST	WEST	COM- BINED	EAST	WEST	COM- BINED	EAST	WEST	COM- BINED
Regime experience ^a		-	.17** (1.8)	-		−.30*** (−.41)		_	28*** (-1.5)
Communist party	12 (-1.4)	min to the same of	01 (21)	.25* (.53)		.21*** (.47)	.21 (1.4)	_	.13 (1.2)
Postwar generation	.26**	.04	.13*	.08	13	.04	.05	.08	.06
	(2.6)	(.46)	(1.4)	(.15)	(07)	(.05)	(.25)	(.38)	(.34)
Postmaterialism	.41***	.30***	.35***	.01	.17	.07	.18	.06	.11
	(2.4)	(1.8)	(1.9)	(.01)	(.04)	(.05)	(.56)	(.14)	(.32)
Religiosity	−.18	03	05	12	−.01	06	−.01	20*	−.14
	(−.60)	(11)	(16)	(07)	(−.01)	(03)	(−.01)	(32)	(−.19)
Education	.13	.32***	.24***	12	.05	.06	08	08	−.10
	(.77)	(1.4)	(1.2)	(13)	(.03)	(.04)	(29)	(17)	(−.25)
Income	.08	12	02	−.01	.06	01	02	02	01
	(.45)	(62)	(10)	(−.01)	(.01)	(01)	(07)	(04)	(01)
Gender ^b Adj. R ² N	.10	04	.02	17	-:12	14	15	11	11
	(1.0)	(51)	(.26)	(34)	(07)	(21)	(87)	(56)	(68)
	.19	.18	.20	.03	.01	.17	.09	.04	.17
	76	85	161	76	85	161	76	85	161

Note: Entries are standardized regression coefficients (OLS); unstandardized coefficients appear in parenthesis. The democratic rights index is an additive index based on items presented in Table 1. (The Appendix contains details about the construction of the indicators.) The egalitarian indicator is an additive indicator of the number of egalitarian responses to the open-ended democracy question. The direct-democracy index is an additive index based on items presented in Table 3. "High = west.

membership indicator (beta = -.30) is remarkable, considering that several individual-level characteristics are included in this analysis. Further, as expected, communist party MPs are more likely than other MPs to support egalitarian democratic conceptions. And while the generational coefficient is insignificant within the East, the total effect of the postwar generation (r = .21) still suggests that postwar MPs are more likely than prewar MPs to endorse egalitarian democratic ideals.

The analyses of elites' evaluations of direct-democracy procedures confirm that East elites are more likely than West elites to endorse direct-democracy conceptions (beta = -.28). This unequivocal result is particularly noteworthy because the model includes a number of predictors typically considered to be important individual-level sources of favorable views on plebiscitarian views, especially postmaterialism. Furthermore, although the cohort coefficient is insignificant, substantial cohort effects appear within the East when I use a direct-democracy indicator based on the open-ended question (beta = .26) but still not within the West (beta = .03). Within the East, this suggests that prewar and, especially, postwar cohorts are supportive of plebiscitarian procedures. Within the West, this finding is consistent with the account

given by the MP quoted earlier: MPs' initial support for direct democracy procedures, which is typically concentrated among postwar cohorts, may weaken as a results of MPs' involvement in parliamentary work. Finally, there is no ready explanation for the weak effect of postmaterialism (beta = .11), especially in light of the large number of studies establishing the link between postmaterialism and direct-democracy procedures (Inglehart 1990).14

On the whole, these analyses attest to the importance of regime experience as an important source of elites' conceptualization of an ideal-typical democracy. Substantial East-West differences exist over the social egalitarian and plebiscitarian dimension, corroborating the institutional-learning argument. In addition, several (although not all) cohort differences within the East and the West are consistent with the notion of cohort-based culture change, particularly for the social egalitarian and plebiscitarian dimension.

Value Divergence Versus Value Convergence

However, the East-West gap is less pronounced in terms of elites' commitment to liberal democratic rights, especially in the context of the open-ended question. In addition, the fact that within the East,

b High = male.

 $p \le .10$. $p \le .05$.

^{***} $p \le .01$.

the postwar generation is substantially more supportive of general democratic rights than the prewar generation cannot be explained with the institutional learning argument. The substantial support for liberal democratic rights among East elites converges with findings from studies of recent regime transitions in Europe, namely, that support for liberal democratic rights is surprisingly strong in light of these nations' lack of democratic experience. In order to account for this finding, several analysts of democratic transitions have resorted to a value diffusion model in explaining the evolution of democratic norms in Southern and East-Central Europe (Di Palma 1990; Fuchs, Klingemann, and Schoebel 1991; McDonough, Barnes, and Lopez-Pina 1986; Pye 1990; Starr 1991). The central premise of this perspective is that a variety of domestic and international factors (e.g., the mass media, economic development, education) enables citizens to compare their authoritarian system with the alternative offered by a democratic political system (Weil 1993, chap. 9). Since liberal democracy prospered economically and politically in the postwar era in Western Europe, many citizens in Southern and East-Central Europe endorse the liberal democratic principles that are associated with the promise and prestige offered by a democracy. Since East German citizens could receive West German television, which undoubtedly helped to erode the legitimacy of the East German economic and political system, this process may have contributed to the evolution of support for liberal democratic rights among East elites.

I would like to stress that I limit the diffusion argument to the evolution of general democratic rights. I do not wish to argue that East Germans have become complete democrats who, in addition to supporting liberal democratic rights, possess the ability to act democratically. For example, opinion research has frequently documented that while general rights are easily endorsed, these rights are frequently not applied to specific minority groups (Duch and Gibson 1992; Sullivan, Piereson, and Marcus 1982). 15 Still, if East Germans express an abstract preference for general democratic rights, it would indicate that the initial conditions for reconstructing a democratic system are more favorable in contemporary Germany than they were in West Germany in the immediate postwar years (Merritt and Merritt 1970; Weil 1989).

In order to conduct an indirect test for the presence of diffusion effects, consider that the diffusion perspective, combined with the notion of generational change, suggests that the democratic values of East and West elites are "pushed" toward convergence. Since the onset of the diffusion process falls into the postwar era (Pye 1990), its force should be primarily felt among the postwar generation of East MPs. Therefore, one indication for the influence of value diffusion would be if East–West differences over democratic rights are smaller within the postwar than within the prewar cohort. In contrast, the institutional learning perspective, combined with the notion of generational change, predicts that East and West

institutions produced differences especially among the postwar cohort. These predictions cannot be tested directly by the analyses in Table 4 because the regime experience variable is absent (columns 1–2) or the analyses do not distinguish directly between prewar and postwar generation (the combined analyses). Instead, I reanalyzed the sources of elites' values within the prewar and postwar generation, paying special attention to the strength of the East–West indicator across cohorts.

The results indicate that postwar elites' support for liberal democratic rights is partly influenced by converging forces (Table 5). For example, the regime membership indicator is insignificant for postwar elites' (b = .49). Instead, elites' education and, especially, postmaterialist values are the best predictors of elites' democratic values. Hence, postwar elites in East and West Germany are primarily divided by postmaterial values and educational background but not by their regime experience—at least as far as their commitment to liberal democratic values is concerned. In contrast, the regime experience variable is highly significant within the prewar cohort (b = 3.2), suggesting that the effects of value diffusion are smaller among prewar than postwar elites.

Yet the results for the egalitarian ideals equation also document the limits of the diffusion argument. In particular, the regime variable is especially strong among postwar cohorts (b=-.56), producing substantial East–West differences over social egalitarian ideals among the postwar elites. In contrast, the regime experience variable is only weakly related to prewar elites' egalitarian values (b=-.22). On this dimension, then, East and West elites have drifted apart during the time of division.

Finally, the results for the direct-democracy indicator provide the strongest evidence yet of the support for plebiscitarian procedures among all East elites. The most important predictor is elites' regime membership, for both prewar (b = -1.4) and postwar (beta = -1.5) elites. Individual-level traits, such as postmaterial values, are relatively unimportant in explaining elites' support for citizens' direct involvement in politics—undoubtedly a result of the significant role that East German citizens played in the overthrow of the East German regime and the socialist emphasis on direct democracy.

On the whole, these results support the interpretation that support for liberal democratic rights has been diffused into East Germany. At the same time, the diffusion of ideal-typical conceptions of democracies has not occurred to the same extent. Unlike their West German counterparts, East German postwar elites especially value liberal democratic rights in combination with social egalitarian and plebiscitarian ideals.

CONCLUSION

The unique historical situation of Germany's division and unification enables analysts to examine the insti-

TABLE 5 Predicting MPs' Democratic Values, by Generation **DEMOCRATIC RIGHTS** EGALITARIAN DEMOCRACY DIRECT DEMOCRACY INDEPENDENT PRF-POST-PRE-COM-POST-COM-PRE-POST-COM-**VARIABLES** WAR WAR BINED WAR WAR BINED WAR WAR BINED .30*** --.19 -.29*** Regime experience .05 .17** -.35*** -.30*** -.25^{*} -.28*** (3.2)(.49)(1.8)(-.22)(-.56)(-.41)(-1.5)(-1.4)(-1.5)Communist party -.03-.10-.01.25* .21*** .10 .17 .14 .13 (-.80)(-1.4)(-.21)(.10)(.54)(.47)(2.0)(1.1)(1.2)Postwar generation .13* .04 .06 (1.4)(.05)(.34).49*** .26*** Postmaterialism .35*** .09 .08 .07 .17 .07 .11 (1.5)(3.2)(1.9)(.05)(.07)(.05)(.45)(.23)(.32)Religiosity .04 -.15-.05-.05-.17 -.06-.19*-.01 -.14(-.53)(.13)(-.16)(-.02)(-.09)(-.03)(-.31)(-.01)(-.19).32*** .18* .24*** Education -.01-.10.06 -.14*-.09-.10(.93)(1.4)(1.2)(.04)(-.01)(-.07)(-.32)(-.22)(-.25)Income .10 -.07-.02-.16.10 -.03-.01 -.01-.05

Note: Entries are standardized regression coefficients (OLS); unstandardized coefficients appear in parenthesis. The democratic rights index is an additive index based on items presented in Table 1. (The Appendix contains details about the construction of the indicators.) The egalitarian indicator is an additive indicator of the number of egalitarian responses to the open-ended democracy question. The direct-democracy index is an additive index based on items presented in Table 3.
^aHigh = west.

(-.08)

-.14

(-.19)

86

.08

(.08)

-.16

.21

75

(-.27)

(-.01)

-.14

.17

161

(-.21)

Gender^b

Adj. R²

tution-culture linkage under quasi-experimental conditions. Consistent with systemic political culture studies that stress institutional learning as a source of elites' values, East and West elites' democratic conceptions are substantially shaped by individuals' exposure to a socialist or liberal democratic political system. The strength of regime experience as a predictor of elites' ideological values is remarkable because the empirical models include a number of individual-level variables (esp. cohort membership) that partially absorb the influence of elites' regime experience on their ideological values. Thus both East and West German systems were partially successful in reshaping the political culture. But the convergence of postwar cohorts over general democratic rights also attests to the diffusion of these rights into

(.47)

.16

.08

87

(2.1)

(-.30)

-.03

.31

75

(-.07)

(-.10)

.02

(.26)

.20

161

The partial success of the East German state's efforts to imbue its citizens with social egalitarian and plebiscitarian ideals suggests that the collapse of a specific set of institutions does not necessarily indicate the absence of institutional learning. The East German state collapsed, but a substantial number of East MPs continues to value socialist-democratic ideals. More generally, whether or not the collapsed institutions in East-Central Europe have imbued citizens with regime-conforming values must be examined in empirical studies and cannot be inferred from the collapse of socialist institutions itself.

(-.11)

-.11

.20

86

(-.67)

(-.08)

-.16

(-.97)

75

.10

(-.01)

-.11

(-.68)

1.17

161

Furthermore, the existence of partially different conceptions of democracies in East and West Germany also underlines the need to be cautious in applying Western (liberal-representative) democratic principles to the newly emerging democracies in East-Central Europe (Collier and Mahon 1993). Before analysts are able to specify the notion of democracy properly in the context of democratic transitions, they must know elites' conceptions of democracy in East-Central Europe. These considerations then call for further analyses of political elites' ideal-typical democratic conceptions in East-Central Europe.

The results also indicate the difficulty of merging Germany into a truly united country. While the power distribution among the East and West undoubtedly ensures that East elites have less influence on major political decisions than West elites, the value differences nevertheless suggest that East elites' preferences will differ from West elites' preferences on a number of policy domains as well as on more fundamental issues (e.g., plebiscitarian proce-

bHigh = male.

 $p \le .10$.

dures). Hence, the legal unification and the value differences between East and West German elites raises once again the possibility of severe political and social tensions in Germany.

On a more optimistic note, the onset of a value convergence in terms of liberal democratic rights also suggests that the issues facing Germany today are partially different from those which West Germany faced in the immediate World War II years. At that time, the major task was to convince the non- or antidemocratic citizens of the virtues of a democratic political system. But the presence of a general commitment to democratic rights in East Germany suggests that at least in principle (if not in practice), East German elites do not need to be persuaded of the virtues of a democratic system—they already are. Rather, one new task is to convince a significant portion of East German elites of the virtues of a parliamentary-representative democracy. What is new, then, is that a debate may take place over which democratic system is the most appropriate for the united Germany. In this sense, the unification of Germany not only revives old, troubling, issues but may also add a new, perhaps less destructive, dimension to the vexing course of Germany's history.

APPENDIX: COLLECTING THE DATA AND MEASUREMENT ISSUES

Several steps were undertaken to maximize the cooperation of Berlin MPs. Before the actual interviewing started in October 1991, an introductory letter was sent to all MPs in the summer of 1991. The letter introduced the project and announced the intention to meet personally with all legislators. I also met all party leaders who agreed to support the study. For example, party leaders agreed to make an announcement in a parliamentary group meeting asking MPs of their respective party to participate in the project. Simultaneously with the announcements, a second letter was sent to MPs, this time requesting a specific interview time. Although relatively few MPs responded to this request promptly, all MPs at this time had become familiar with the project. Most of the interviews were actually scheduled during the biweekly parliamentary session, which enabled me to contact personally those MPs who had not responded to the previous requests.

A total of 168 MPs was interviewed. The cooperation of MPs was excellent. Smaller parties are somewhat overrepresented because the almost intimate atmosphere among a small parliamentary group of, say, 15 to 20 MPs substantially increased the visibility of the project. (A table comparing the partisanship of interviewed MPs with the entire parliament is available upon request.) Extensive comparisons of the data with the entire parliament suggest that the data closely mirrors the parliament on important characteristics, such as age, education, gender, occupation of parliamentary leadership positions, and seniority.

A first draft of the questionnaire underwent close

scrutiny before the field work actually began. In October 1991, two party leaders from local district parliaments from East and West Berlin participated in a formal pretest of the questionnaire. Both leaders also discussed the questionnaire at length.

Measurement and Coding Schemes. The responses to the open-ended democracy question were coded based on detailed notes taken during the interview. I used a slightly modified coding scheme developed by Putnam (1973, 270–73).

1–8. The term democracy is frequently used without further specifications these days. What seem to you personally the essentials of a democracy? {CODE UP TO FIVE MENTIONS}.

Government by the People

- 11. Government by the people; popular control; control by the people
- 12. Popular interest in and awareness of politics
- 13. Responsibility or answerability of the government to the people; government by consent; government based on electoral mandate
- 14. Popular participation; an active role of the people; popular involvement in decision making; direct democracy within the framework of parliamentary democracy
- 15. Direct democracy; referenda for important decisions; public should be able to recall ministers at any time;
- 19. Other

Equality and Social Democracy

- 21. Equality in general
- 22. Political equality; one man, one vote
- 23. Equality of opportunity; each person has the possibility of developing him/herself as far as possible; participation of citizens in all areas of society
- 24. Just standard of living; freedom from want; social and economic security for all
- 25. Classless society; less social distance; fewer rich and poor; less social privilege
- 26. Social ownership/control over the economy; industrial democracy
- 27. Provide minimum social security/right to a job
- 28. Gender equality
- 29. Other

Liberty

- 31. Liberty; freedom in general
- 32. Political or civic liberties in general
- 33. Freedom of expression (speech, free press)
- 34. Minority rights; consideration of the minority
- 35. Limited government; checks and balances; no arbitrary power
- 36. Laissez-faire, socially and economically. Freedom from government interference in socioeconomic affairs
- 37. Religious liberty
- 39. Other



Institutions and Procedures

- 41. Elections
- 42. Majority rule
- 43. Representative or parliamentary government in general
- Parliamentary or legislative control over the executive
- 45. Rule of law; legal due process (Rechtsstaat)
- 46. Defendable democracy
- 49. Other

Political Competition and Choice

- 51. Possibility of government changes; minority can become majority
- 52. Party competition; more than one party
- 53. Strong, critical opposition
- 54. Elite competition; ruling oligarchy
- 59. Other

Societal Conditions

- 61. Pluralism, variety of private associations and institutions
- 62. Consultation by the government with groups and organizations
- 63. Parties as centers of participation and agents of representation
- 64. Absence of party discipline
- 65. Decentralized institutions; federalism
- 66. Ecologically sound politics
- 69. Other

Characteristics of Citizens

- 71. Mature, educated, intelligent citizens
- Freedom to do what is right; individual selfcontrol
- 73. Assumption of responsibility and duties
- 74. Action in the interest of collective, not only of individual
- 75. Reciprocal respect and tolerance
- 79. Other

Other

81.

- 98. Don't know
- 99. Not answered

The recodes for the data presented in Table 2 are as follows: government by the people (11–13), active participation (14), direct democracy (15), social equality (24–27), Equality of opportunity (21–23), civil rights/limited government (31–36), institutions (41–45), political competition (52–54), societal competition (61–62), citizens' responsibility (71–75).

Democratic Rights. Members of parliament were shown a card containing the indicators from Table 1 and asked, "Would you tell me for each of the statements, whether you agree or disagree?" The card also displays a seven-point scale, defining the polar ends of agree completely (7) and disagree completely (1). Percentages in Table 1 are based upon MPs who

agree with a statement (7–5). The indicator used in the multivariate analyses is additive after all items have been recoded such that agreement with a statement reflects democratic attitudes. Cronbach's alpha for the East, West, and combined data is .61, .63, and .62, respectively. The variable's minimum equals 20, maximum 42, mean 32.6, and standard deviation 5.4.

Equalitarian Democracy. I created an additive index of all the egalitarian responses to the open-ended democracy question (24–27 in Table A-1). The variable's minimum equals 0, maximum 4, mean .32, and standard deviation .69. Given the skewed distribution of this variable, particularly in the West, I also analyzed this model using probit, but the results generally lead to the same substantive conclusions, with two exceptions. Within the East, the Communist party coefficient misses statistical significance by a small margin (p = .11) and the gender coefficient is significant: female MPs are somewhat more likely to mention egalitarian democratic components.

Direct Democracy. For the direct-democracy indicators (Table 3), I used a dichotomous response format in order to keep the results comparable to the study of national-level East and West German MPs (Herzog et al. 1990; Werner 1991). The question reads, "Would you please indicate your opinion on the plebiscitarian involvement of citizens by indicating whether you find each of these procedures involving citizens directly meaningful or not meaningful?" Responses in Table 3 are respondents who find the procedure meaningful. For the plebiscitarian index used in multivariate analyses, I first included the small number of missing values as a neutral middle category and then created an additive index. Cronbach's alpha in the East, West, and combined data is .71, .65, and .71, respectively. The variable's minimum equals 4, maximum 12, mean 7.9, and standard deviation 2.7.

Regime Membership. This dichotomous variable is coded 0 for East Berlin MPs and 1 for West Berlin MPs.

Communist Party. This dichotomous variable is coded 1 if the MP is a member of the reformed Communist party (PDS) and 0 if not.

Postwar Generation. In this dichotomous variable, 1 represents MPs born in 1945 or later, and 0 represents MPs born in 1944 and earlier. I chose this cutting point for generations because it is theoretically meaningful and because it leaves enough cases within each cohort to conduct the cohort analyses. The decision to divide MPs into two groups—those born in 1944 or before and those born later—means that MPs reaching the age of 15 before 1960 are coded as a prewar cohort. This cutting point parallels the degree to which the quasi-laboratory conditions had been established in Germany because the Berlin Wall was not built until 1961. Before that time, East and West Germany were less insulated from each other. I have conducted a sensitivity analysis using 11 different

cutting points for the cohort definition. For example, I first defined a prewar cohort by combining MPs born in 1938 and before into the prewar category. A second prewar variable includes those born in 1939 and before, a third contains MPs born in 1940 and before, and so on. I then conducted the analyses from Table 4 and 5, using these different cohort variables in separate analyses. The results parallel the historical reality in Germany: the largest cohort differences within the East emerge when the postwar cohort predominately contains MPs who grew up after the construction of the Berlin Wall. While a more refined breakdown of cohorts would undoubtedly be desirable, these analyses suggest that the cohort analyses presented here are robust and do not depend on an arbitrary birth date cutting point.

Postmaterialism. I used a slightly modified coding procedure of the original items Inglehart developed. While materialists and postmaterialists are coded as Inglehart suggests, I distinguish between mixed-materialists and mixed-postmaterialists, depending on whether respondents first mention the materialist or the postmaterialist item. This coding procedure uses more information than the three-point indicator. (The results are almost identical when the mixed categories are combined.) The variable's minimum equals 1, maximum 4, mean 3.2, and standard deviation .94.

Religiosity. "How often do you attend church? Would you say: At least every Sunday; almost every Sunday; sometimes; once a year; less than once a year; never." High values represent religious responses. The variable's minimum equals 1, maximum 6, mean 4.3, standard deviation 1.5.

Education. Higher values reflect higher educational attainment. (1) Volkschule/8. Klasse, (2) Mittlere Reife/10. Klasse/Mittel Schule, (3) Abitur/EOS, (4) Fachhochschule/Fachschule, (5) Universitaet. The variable's minimum equals 1, maximum 5, mean 4.3, standard deviation 1.1.

Income. (1) Below 4,000DM, (2) 4,000–5,999DM, (3) 6,000–7,999DM, (4) 8,000–9,999DM, (5) above 10,000DM. The variable's minimum equals 1, maximum 7, mean 2.7, standard deviation 1.1.

Gender. This is coded 1 for male MPs and 0 for female MPs.

Notes

I would like to thank Russell Dalton for the helpful suggestions he made at various stages of this research. He, Dana Chabot, Robert Huckfeldt, and Manfred Kuechler provided insightful comments on earlier versions of this paper. I am also grateful to the following individuals in Berlin for the suggestions and the stimulating intellectual environment they provided: Niels Diederich, Dieter Fuchs, Dietrich Herzog, Hans-Dieter Klingemann, and Bernhard Wessels. Finally, I would like to acknowledge the financial support provided by

a fellowship from the Berlin program for advanced German and European studies of the Social Science Research Council and the Free University of Berlin.

1. In order to avoid the repetitive use of one term, I use the terms conceptions of democracy, core values, and ideological

values interchangeably.

2. Other elite studies document the relatively weak influence of sociodemographics on elites' attitudes (Beyme 1971; Crewe 1974; Edinger and Searing 1967; Herzog 1975).

Additional systemic factors mentioned are the socioeconomic composition of elite sectors when modern political systems formed and the outcome of political competitions

between social groups.

- 4. Empirical studies of mass political culture try to circumvent the case problem by collecting institutional data and information about political cultures from public opinion surveys of a large number of countries (Bollen and Jackman 1989; Inglehart 1988; Weil 1989). Although valuable, these studies must rely on relatively few indicators of mass political culture. Other studies, in turn, examine the evolving democratic attitudes of mass publics in Spain (McDonough, Barnes, and Lopez-Pina 1986) or Germany (Baker, Dalton, and Hildebrandt 1981); or else they examine the link between regional institutions and regional cultures (Putnam 1993). But none of the cases examined offers the opportunity to compare directly the influence of two fundamentally opposed systems on elites' values.
- 5. Analyses of mass political cultures in other Central Europe nations also appear to corroborate this argument. For example, Almond (1983) notes that the Polish mass public began to endorse egalitarian values, like the Czech public (McGregor 1991). Similarly, a recent study of mass publics in the "republics" of the former Soviet Union notes that there has not been a "wholesale abandonment of socialist principles" (Finifter and Mickiewicz 1992, 861).

6. Indeed, previous studies of mass publics attest to the importance of institutional arrangements (Powell 1986) and individuals' group membership (Klingemann 1979) as sources

of political orientations and behavior.

- 7. In this discussion, I assume that the new political system is as alien to the preexisting political culture as the respective socialist and democratic systems were in Central Europe and West Germany after World War II and as democratic systems are now in contemporary Central Europe and in East Germany. Consequently, a different argument would have to be constructed for cases like the United States or Great Britain, where the gradual evolution of democratic political institutions over centuries makes it virtually impossible to disentangle discrete variables representing the effect of institutions on culture and vice versa.
- 8. Consistent with this perspective, cohort studies in West Germany find that elites' historical experiences during their formative socialization years markedly affects the predominant values of each generation (Baker, Dalton, and Hildebrandt 1981). Analogously, postwar elites in Central Europe, who only experienced the socialist systems, should be shaped more by socialist values and practices than prewar elites, whose cohort experience also includes nonsocialist systems.

9. These variations are largely controlled for because many, although not all, MPs grew up in Berlin.

10. A question about MPs' background revealed that two West Berlin MPs escaped from the GDR after they passed adolescence. I excluded them from the study.

- 11. This interpretation about the reasoning of MPs is based on a preliminary analysis of an open-ended question asking whether MPs would support solving the asylum issue through a referendum. A detailed discussion cannot be pursued here.
- 12. Throughout the field work, I tried to obtain additional information about East MPs' involvement in various GDR organizations. It turned out to be extraordinarily difficult to find this information in archives. In addition, the published biographies of East MPs frequently do not mention membership in GDR organizations. Although a more complete mea-

sure of the extent of regime involvement would undoubtedly be desirable, this information could not be found.

13. This alternative indicator is an additive indicator of the number of mentions of categories 14 and 15 in the Appendix.

- 14. It should be studied in future elite surveys whether this finding is unique to the Berlin study or whether institutional learning mitigates the predispositional linkages between postmaterialism and direct democracy on the level of elites. In the absence of comparable elite surveys, this issue must remain unresolved here.
- 15. I have presented evidence elsewhere showing East MPs are substantially less tolerant than West MPs, despite the strong support for liberal democratic rights among East MPs (Rohrschneider 1993).

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CONTROVERSY

QUESTION FORM AND CONTEXT EFFECTS IN THE MEASUREMENT OF PARTISANSHIP: EXPERIMENTAL TESTS OF THE ARTIFACT HYPOTHESIS

Previous articles in this Review, including a Controversy in 1992, debated the comparability of alternative forms of the question about partisanship asked in Gallup and Michigan SRC surveys. Bishop, Tuchfarber and Smith contribute to this debate by reporting and analyzing evidence from 15 experimental surveys in Ohio in 1991–1993. They conclude that the distribution of partisan loyalties will generally be the same whether one uses the Gallup or Michigan Survey Research Center question and that, contrary to findings of Abramson and Ostrom, the Gallup form is no more responsive to short-term political forces than its SRC counterpart. In response, Abramson and Ostrom agree that during many time periods there will be little difference between aggregate levels of macropartisanship regardless of which measure is used. But they argue that during periods of political volatility the Gallup approach will accentuate differences, while the SRC version will attenuate them.

COMMENT

Political party identification has become probably the most widely used theoretical construct in the literature on American political behavior. Though its measurement dates back to at least the early days of public opinion polling in the 1930s, and even a bit before that, it was not until the 1950s that it became established at the Michigan Survey Research Center (SRC) as the intervening variable in the study of the American voter.2 While it has become a well-established staple in the empirical study of American politics, various investigators have raised questions from time to time about the dimensionality, measurement, and meaning of party identification.3 More recently, political scientists and public opinion pollsters alike have also raised significant doubts about the comparability of the two most widely used measures of partisanship in the literature: the Gallup form, "In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?," and the Michigan SRC form, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?" (Abramson and Ostrom, 1991, 1994; Bishop, Tuchfarber, and Smith 1992, 1993; Kagay 1991; Kohut 1991; MacKuen, et al., 1992; see also the anticipation of this controversy by Converse and Pierce 1985).

The Gallup form of the question, it is typically argued, is more responsive to the effects of short-term economic and political forces because it focuses the respondent's attention on how he or she feels about the two political parties "as of today," whereas the Michigan form is said to elicit more of the respondent's long-term loyalty to one of the two parties with its emphasis on how he or she "generally" and "usually" thinks. It is claimed, therefore, that any significant difference (1) between the marginal distributions of party identification produced by these two question forms at any one point in time, (2) in the variability of responses to these two forms over time,

or (3) in their intercorrelations with various shortterm indicators (e.g., presidential approval, consumer sentiment, voting preferences) results from the difference in *time horizon* created by these alternative wordings of the question (Abramson and Ostrom 1991, 1994; MacKuen et al. 1992, resp.; see also Borrelli, Lockerbie, and Niemi 1987 and Fine 1992).

THE MACROPARTISANSHIP CONTROVERSY

This ongoing argument about the time frame in the wording of these two versions of the party identification question lies at the heart of the recent controversy in this Review about whether the revisionist findings on macropartisanship reported by Mac-Kuen, Erickson, and Stimson (1989) can be generalized only to the Gallup, or also to the Michigan, form of the question (Abramson and Ostrom 1991, 1994; MacKuen et al. 1992, resp.) In their most recent response, MacKuen et al. (1992) maintain that their findings are not an artifact of the wording of the question used by Gallup, whereas Abramson and Ostrom (1994; MacKuen et al. 1992, resp.) adhere to the view that the bulk of the evidence supports their position that the wording of the question does make a significant and substantial difference in the conclusions one reaches about the changes, causes, and consequences of macropartisanship in the American electorate.

Until fairly recently, all of the evidence in this debate had come from nonexperimental observations of how the Gallup and Michigan measures of partisanship have fluctuated over time in response to such political indicators as presidential approval and to such economic barometers as consumer confidence. It soon became obvious to everyone involved in this controversy, however, that there was a critical need to test the *question-wording-artifact hypothesis* advanced by Abramson and Ostrom (1991, 1994; MacKuen et al.

1992, resp.) by comparing the Gallup and Michigan forms of the party identification question in a controlled experiment, using the same mode of interviewing within a single organization to rule out mode-of-interviewing effects and "house effects" (see Hugick 1991; Smith 1978). And to guard against ad hoc interpretations of mere noise, the findings, significant or not, needed be replicated under different political conditions and preferably with different populations.

EXPERIMENTS AND REPLICATIONS

In April 1991, we began a series of such experiments and replications, carried out as part of the Ohio Poll and the Greater Cincinnati Survey, all of which were conducted by computer-aided, random-digit-dialed telephone interviews at the University of Cincinnati's Institute for Policy Research. In a previous paper, we presented the preliminary results from the first 7 experiments we did (Bishop, Tuchfarber, and Smith 1992). Here we will report the results of 8 additional replications (a total of 15 experiments altogether), 7 of which were done either during, or immediately after, the 1992 presidential election campaign.

In the interim we have become well aware of a similar set of experiments begun by Abramson and Ostrom (1994; MacKuen et al. 1992, resp.) in March 1992 in a series of surveys conducted in the state of Michigan. As a result, where direct comparisons of evidence exist, we have contrasted their findings with ours. Pitting one set of results against another in the spirit of scientific exchange should be of benefit not only to the participants and spectators in this particular controversy but also to the intellectual growth of an "experimental political science" that Kinder and Palfrey (1993) have called for elsewhere.

HYPOTHESES

If the artifact hypothesis—or time horizon hypothesis, as we prefer to call it-is correct, we should find not only a significant difference between the marginal distributions of responses to the Gallup and Michigan forms of the question at any one point in time but also a significant variation in the degree to which each measure of partisanship correlates with a respondent's current political and economic attitudes on theoretically related, short-term indicators. If, as is argued, the Gallup form of the question is more sensitive to current events because of its shortened time horizon ("as of today"), then we should expect to find that responses to it are more highly and significantly correlated with responses to questions about the respondent's approval of the president's performance, confidence in the economy (consumer confidence), and current preferences for candidates for various political offices (and other short-term political evaluations) than are responses to the Michigan form of the question with its extended time horizon of "generally" and "usually" (see Abramson and Ostrom 1991, 1994; Kagay 1991; Kohut 1991; MacKuen et al. 1992, resp.) As we shall show, that is not the case.

FINDINGS

Rarely, if ever, do we have the opportunity to examine such a profusion of experimental evidence on an issue of such fundamental importance to the discipline as the measurement of political party identification. Let us look now at what the empirical evidence tells us.

Effects on Marginal Distributions

The results in Table 1 tell us that the wording of the party identification question made little or no difference in the distribution of responses. In 14 of the 15 experiments we conducted the difference was not statistically significant. The one exception that occurred in the Greater Cincinnati Survey of November-December 1991 is probably best regarded as due to chance since the rather odd pattern it produced (more Independents with the Gallup form and both more Democrats and Republicans with the Michigan form) does not appear consistently in the other 14 experiments. Indeed, finding a statistically significant difference in 1 out of 15 experiments is just about what would be expected by chance (at p = .05). Furthermore, this repeated finding of insignificant differences at different times, under different political conditions, and in three different populations, each with a somewhat distinct partisan coloration (the city, tending to be Democratic, the metro area, more Republican, the state, more evenly balanced) underscores the value of replication in this controversy in particular and in social science research more generally (see Neuliep 1991). It is not hard to imagine how a single survey with a large enough sample might generate a statistically significant difference (as in the one instance we observed) only to evaporate like "cold fusion" in the next independent replication (see Schuman and Presser 1981, 317-23).

Abramson and Ostrom (1994) have also found that the wording of the party identification question generally produces no significant difference in the aggregate distribution of responses. In only one of their six experimental comparisons did they find a difference that was statistically significant at the .05 level. For another replication of this result with multiple measures of party identification within the same survey, see Green and Schickler 1993. So on this point at least there appears to be substantial evidence and agreement among us that the wording of this question, in either the Gallup form or the Michigan SRC form, will generally make little or no difference in the conclusion a researcher would draw about the aggregate distribution of political party identification. That much seems settled.

TABLE 1			
Political Party Identific Fifteen Experiments (%		uestion	Form in
-	QUEST FOR		
SURVEY	GALLUP	MICH- IGAN	$X^2(df = 2)$
Ohio Poll			;
1. 24–30 Apr. 91	,		
Democrat	28.4	34.0 ງ	
Independent	42.2	40.5 }	2. 6 5, n.s.
Republican	29.4	25.4 J	
Number of cases	313	338	•
2, 2–11 Oct. 91 Democrat	33.4	ר 30.9	
Independent	39.5	39.1	.78, n.s
Republican	27.0	30.0	., 0,0
Number of cases	296	317	
3. 20-30 Mar. 92		٠,	•
Democrat	30.1	29.2	
Independent	38.3	40.9	.54, n.s.
Republican	31.6	29.9 J 394	
Number of cases 4. 9–19 Sept. 92	386	394	
Democrat	33.3	31.0 າ	
Independent	38.6	39.1	.74, n.s.
Republican	28.2	29.9	
Number of cases	412	481	,
5. 16-25 Oct. 92		•	
Democrat	36.7	34.1	
Independent	38.1	33.8	4.9, n.s.
Republican Number of cases	25.3 431	32.1 J 414	
6. 28 Oct1 Nov. 92 ^a		**!**	•
Democrat	30.1	30.8 ז	
Independent	41.0	40.6	.07, n.s.
Republican	28.9	28.6 J	:
Number of cases	529	539	•
7. 3–10 Feb. 93			
Democrat	33.3	34.2	44
Independent	40.0	40.2	.14, n.s.
Republican Number of cases	26.7 408	25.6 J 398	
8. 3–14 May 93	700	030	
Democrat	27.1	30.0	
Independent	44.2	40.5	1.2, n.s.
Republican	28.7	29.5 J	•
Number of cases		407	
Greater Cincinnati Surve			
1. 29 May-13 June 91		20.0 -	
Democrat Independent	23.5 39.0	$\frac{22.2}{37.5}$	74, n.s.
Republican	37.5	40.4	.,, 7, 11.0.
Number of cases	421	451	
2. 14 Nov3 Dec. 91			
Democrat	22.4	27.6	7.51,
Independent	46.0	37.0 }	p < .05
Republican	31.6	35.4 J	F
Number of cases 3. 9–29 June 92	437	435	
Democrat	21.3	21.3 7	
Independent	43.6	44.9	.19, n.s.
Republican	35.1	33.8	,
Number of cases	436	423	٠.

TABLE 1 continued		,	
	QUEST FOR		
		MICH-	
SURVEY	GALLUP	IGAN	$X^2(df=2)$
4, 20 Nov14 Dec. 92			
Democrat	25.2	25.7 ๅ	
Independent	40.5	42.8	.97, n.s.
Republican	34.3	31.4 J	
Number of cases	504	474	
5. 14 May-2 June 93			
Democrat	29.7	26.1	
Independent	36.9	40.1 }	1.71, n.s.
Republican	33.4	33.8 J	
Number of cases	488	459	
Other Surveys			
1. City of Cincinnati,			
27 May-3 June 92			
Democrat	30.0	33.3 ר	
Independent	46.1	40.0 }	1.69, n.s.
Republican	23.9	27.0 J	
Number of cases	230	215	
2. Cincinnati Area Pro	ject ^b ,		
5 Oct16 Dec. 91	,		
Democrat	23.2	ר 27.8	
Independent	38.1	40.2 }	3.4, n.s
Republican	38.7	32.0 J	,
Number of cases	323	306	

University of Cincinnati, fall quarter 1991 (see Rademacher 1993).

Responsiveness to Short-Term Forces in the 1992 Campaign

Abramson and Ostrom (1994) have argued, however, that despite this absence of aggregate differences, the Gallup measure is more responsive than the Michigan SRC form to short-term forces, such as changes in presidential approval, particularly during the most recent presidential election campaign. To begin with, they hypothesized that because of George Bush's relatively low approval ratings during the campaign—a situation they believe made the political context much more volatile than normal—the percentage identifying with the Republican party would tend to be higher with the Michigan SRC form (which supposedly taps more of the long-term component of partisanship) than with the Gallup form (which should be more responsive to the short-term decline in Bush's popularity). Support for the Democratic party, they predicted, should be about the same with either form as they believed it should be mostly unaffected by Bush's low approval ratings. Finally, they speculated that because the Gallup form is more responsive to short-term political forces, it might be expected to produce more Independents, especially in a year in which there was a strong independent candidacy in the form of Ross Perot, not to mention other factors leading many voters to rethink their partisanship in a rather volatile political year.

In support of their hypothesis Abramson and Ostrom (1994) reported that more Republicans were found with the Michigan SRC form than with the Gallup form, in all six of their surveys, with the differences varying from 1.1% to 6.6%. The percentage difference in Democratic identifiers, as predicted, was quite small, with the largest difference between forms being 1.3%. Furthermore, in five of the six surveys, the Gallup form produced more Independents, with the differences varying from 2.5% to 6.8%. Though none of these differences were statistically significant (as they acknowledged), they were all in the predicted direction.

The relevant question here is, How well did these predictions replicate elsewhere during that same election campaign? Table 1 shows evidence from four State of Ohio polls (March-November 1992); two Greater Cincinnati Surveys, one conducted shortly after the election (1992); and one City of Cincinnati survey (June 1992). Looking first at the Ohio polls, we find that in March 1992, just a week or so after Abramson and Ostrom completed their survey in the state of Michigan, the percentage of Republican identifiers in our Ohio poll was higher with the Gallup form, not with the Michigan form (difference = 1.7%). In both the September and mid-October Ohio polls, the percentage of Republican identifiers was higher with the Michigan form (difference = 1.7% and 6.8%, respectively), as predicted; but in the final preelection Ohio poll (28 October-1 November) of likely voters, the percentage of Republican identifiers was slightly higher with the Gallup form (difference = .3%).

The pattern during the election campaign looks much the same in the Cincinnati surveys. In the June 1992 Greater Cincinnati Survey the percentage of Republican identifiers was higher with the Gallup form, not with the Michigan form (difference 1.3%). But in the City of Cincinnati survey done just a few weeks before that (27 May-23 June), the percentage of Republican identifiers was higher with the Michigan form (difference = 3.1%), as predicted. Finally, in the postelection Greater Cincinnati Survey (20 November-14 December), at a time when the Democratic triumph of Bill Clinton was still fresh in the minds of voters and consumer confidence, that critical short-term barometer of the public mood, was rising on the tailwinds of the Democratic victory, the percentage of Republican identifiers was higher with the Gallup form (difference = 2.7%), not (as would be predicted) with the Michigan form. And so the story goes: in four of the same seven surveys that show no significant aggregate differences between the two question forms (just as in the State of Michigan surveys) the percentage of Republican identifiers is higher with the Gallup form, but in the other three surveys it is higher with the Michigan form. The percentage of Democratic and Independent identifiers too, in these same seven surveys, fluctuates back and forth-higher on one form, then higher on the other (see Table 1). These are as hard to distinguish from what we would expect to find by chance as are the insignificant differences we see in 14 of the 15 experiments (Table 1) and in *five* of the six experiments reported by Abramson and Ostrom (1994). The data are clearly telling us that there is not much going on beyond sheer random fluctuation.

Variations in Macropartisanship

The results for the macropartisanship indicator developed by MacKuen, Erikson, and Stimson (1989) tell much the same story. Abramson and Ostrom's (1994) hypothesis predicts that the macropartisanship score (the percentage of major party identifiers who are Democrats) should be consistently more Democratic with the Gallup form than with the Michigan form, as the former question should be more responsive to the volatile political context associated with the low approval ratings for President Bush during the 1992 campaign. Indeed, the data from five of their six election campaign surveys shows the ratio of Democrats to Republicans to be higher, as predicted, with the Gallup form. But once again the data from our Ohio studies fails to find this consistent evidence for their hypothesis (see Table 2). In two of the four Ohio polls done during the 1992 election campaign the macropartisanship score was higher on the Michigan form, but in the other two polls it was higher on the Gallup form. In both the June and November-December (postelection) 1992 Greater Cincinnati Surveys, the macropartisanship score was higher on the Michigan form, but in the City of Cincinnati survey in late May and early June 1992, it was slightly higher on the Gallup form. So once more we find a familiar random rocking between the two forms of the questions—now somewhat higher with the Gallup form, then somewhat higher with the Michigan form.

Table 2 also shows some variation over time in the macropartisanship scores observed in our eight Ohio polls, alongside the trend in presidential approval from the same polls. As Abramson and Ostrom (1994) have carefully pointed out, our April 1991 Ohio poll, which was conducted at a time when presidential approval was near an all-time high shortly after the Persian Gulf War and when Republicanism was surging in the national Gallup polls, the macropartisanship score (percent Democrat) was, as would be predicted, noticeably higher on the Michigan form (thus showing an aggregate difference between forms in response to a major short-term force). In October 1991, presidential approval, as measured in both the Ohio and national polls, had dropped considerably (down 14.6% in Ohio) and, as Abramson and Ostrom would predict, the percentage of Democratic identifiers is now higher on the more responsive Gallup form and moves down correspondingly on the Michigan form. By March 1992, presidential approval had plummeted even further in the Ohio poll (down 17.4%), as well as in the national polls. In fact, by that time, it was well below 50% (43.7%), making the political context, as Abramson and Ostrom would argue, quite volatile. But the percentage of Democratic identifiers does not rise further on the more

÷			% APPROVING
SURVEY	GALLUP	MICHIGAN	PRESIDENT IN OHIO POLL
Ohio Poli			
1. 24–30 Apr. 91	49.3	57.2	75.7
2. 2-11 Oct. 91	55.2	50.7	61.1
3. 20–30 Mar. 92	48.8	49.4	43.7
4. 9-19 Sept. 92	54.1	50.9	39.2
5. 16-25 Oct. 92	59.2	51.5	Not Asked
6. 28 Oct1 Nov. 92ª	51.0	51.9	Not Asked
7. 3-10 Feb. 93	55.5	57.2	42.4
8. 3-14 May 93	48.6	50.4	38.1
Greater Cincinnati Survey			
1, 29 May-13 June 91	38.5	35.5	
2. 14 Nov3 Dec. 91	41.5	43.8	, ()
3. 9-29 June 92	37.8	38.7	
4. 20 Nov14 Dec. 92	42.4	45.0	
5. 14 May-2 June 93	47.1	43.6	
Other Surveys		•	
1. City of Cincinnati,		•	
27 May-3 June 92	55.7	55.0	
2. Cincinnati Area Project,			•
5 Oct16 Dec. 91	37.5	46.5	

responsive Gallup form, as would be predicted. It actually drops back to a level below that observed in the April 1991 Ohio poll, which Abramson and Ostrom (1994) have cited as evidence supporting their predictions on macropartisanship. Notice, too, that in the March 1992 Ohio poll, the percentage of Democratic identifiers on the Michigan form barely moves from the base line of October 1991 (50.7 to 49.4), whereas the plunging presidential approval rating would lead us to expect a much sharper drop and a corresponding rise in Republican identifiers on the Michigan form of the question, which supposedly better measures the long-term component of partisanship. In the September 1992 Ohio poll, the macropartisanship score shoots back up 5.3% on the Gallup form, but barely moves on the Michigan form, while presidential approval nudges down just a bit more (43.7 to 39.2). In the mid-October poll it shoots up again on the Gallup form (5.1%) and moves just a little on the Michigan form. And in the final preelection poll of likely voters there is little or no difference between forms in the macropartisanship scores, though it is slightly higher on the Michigan form.

Notice, finally, that the macropartisanship score, as Abramson and Ostrom would predict, rises on the Gallup form from 37.8% in the June 1992 Greater Cincinnati Survey to 42.4% in the postelection Greater Cincinnati Survey of November-December. But unexpectedly, it rises even more on the Michigan form in these same two surveys (38.7% to 45.0%). Once again, it all looks rather random and unpredictable and the hypothesis about the greater responsive-

ness of the Gallup form appears rather unsalvageable without a lot of ad hoc, unfalsifiable interpretations after each and every point in time. Before we are tempted to do that, let us take a look at the correlational evidence on which of the two forms is more responsive to short-term evaluations and electoral preferences.

Correlations with Short-Term Political Evaluations and Electoral Preferences

Abramson's and Ostrom's (1994) thesis clearly predicts that responses to the Gallup form should be more highly correlated with short-term economic, political, and electoral evaluations, such as presidential approval, consumer confidence, candidate evaluations, voting preferences, and the like than should responses to the Michigan form. The evidence from their analysis appears to support their prediction. In 30 out of 33 comparisons, they found that the responses to the Gallup form were more highly correlated with various short-term indicators than were those given to one of their two recoded versions of the Michigan form (SRC 1). With the other recoded version of the Michigan form (SRC 2), their results were even better: all 33 comparisons were in the predicted direction (but only three statistically significant), leading them to conclude that "the Gallup question is more likely to capture short-term preferences than the Michigan SRC question." It all looks

rather impressive; but once more, we must ask, How well does it replicate?

The answer given in Table 3 is, not very well. Consider first the correlations from the April 1991 Ohio poll.6 This same Ohio poll, as Abramson and Ostrom (1994) have pointed out, shows the surge in Republican identifiers on the Gallup form corresponding to the soaring presidential approval ratings for George Bush shortly after the Persian Gulf War; and the macropartisanship score (percent of major party identifiers who are Democrats) is higher on the Michigan form, as they would predict. But as this Republican presidential tide is rising, we find that the Michigan form of the party identification question, not the supposedly more responsive Gallup form, is somewhat more sensitive to such shifts in presidential fortunes. In each case, we find that the correlation between party identification and presidential approval ratings is higher with the Michigan form than with the Gallup form, not only for the overall approval rating but also for approval of the president's performance in handling the economy, foreign affairs, and the very recent, short-term event of the Persian Gulf War.

The same pattern shows up on the gubernatorial and senatorial approval and evaluation ratings, as well as in the senatorial trial heat votes: the correlations are generally higher with the Michigan form. But on the consumer confidence measures, with one exception, the correlations tended to be higher with the Gallup form. And for the most part, this general pattern repeats itself in the October 1991 Ohio poll (see Table 3).

Consider next the March 1992 Ohio poll conducted, again, just a week or so after Abramson's and Ostrom's Michigan poll. We find that the correlation between party identification and overall presidential approval is a bit higher on the Michigan form, not on the supposedly more responsive Gallup form. Similarly, the ratings for the president's handling of the economy and foreign affairs more highly correlated with the Michigan form of the party identification question, an additional piece of evidence not available in Abramson's and Ostrom's analysis. Consumer confidence ratings (another key short-term indicator in this debate) also tended to be somewhat more highly correlated with responses with the Michigan form of the question, not with the Gallup form. Gubernatorial and lieutenant-gubernatorial approval ratings were somewhat higher with the Gallup form. But then when we look at the results for short-term favorability ratings of different presidential candidates at that time and voting preferences in various presidential trial heats, the correlations are noticeably higher with the Michigan form.

We could go on multiplying examples and counterexamples before the campaign, during the campaign, and after the campaign. But it would be best here to summarize what is shown in Table 3. Of the 134 comparisons in Table 3, involving a great variety of short-term evaluations and electoral preferences at various times and under various economic and polit-

TABLE 3

Product-Moment Correlations between Partisanship and Short-Term Political and Economic Evaluations by Question Form

SURVEY	Economic Evaluations by Quest	ion Form	
1. 24—30 Apr. 91 Pres. approval General General Economy Foreign affairs Persian Gulf Gubernatorial approval U.S. senatorial evaluation U.S. Senate pref. DeWine/Glenn Petro/Glenn Consumer confidence Personal financial outlook Outlook U.S. business Outlook for major purchases Outlook for major purchases Overall index D. 2. 2—11 Oct. 91 Pres. approval Cu.S. senatorial approval Cu.S. senatorial evaluation D. 2. 2 (310) D. 2 (280) D. 338 (192) D. 34 (317) D. 35 (198) D. 36 (186) D. 38 (192) D. 36 (186) D. 38 (192) D. 37 (195) D. 38 (192) D. 38 (192) D. 39 (197) D. 30 (198) D. 31 (299) D. 32 (298) D. 34 (296) D. 34 (296) D. 35 (298) D. 36 (297) D. 34 (296) D. 36 (298) D. 37 (298) D. 38 (295) D. 39 (296) D. 30 (296) D. 30 (296) D. 30 (296) D. 31 (296) D. 32 (296) D. 33 (296) D. 34 (296) D. 35 (296) D. 36 (296) D. 36 (296) D. 37 (296) D. 38 (296) D. 39 (296) D. 30 (SURVEY		
1. 24—30 Apr. 91 Pres. approval General General Economy Foreign affairs Persian Gulf Gubernatorial approval U.S. senatorial evaluation U.S. Senate pref. DeWine/Glenn Petro/Glenn Consumer confidence Personal financial outlook Outlook U.S. business Outlook for major purchases Outlook for major purchases Overall index D. 2. 2—11 Oct. 91 Pres. approval Cu.S. senatorial approval Cu.S. senatorial evaluation D. 2. 2 (310) D. 2 (280) D. 338 (192) D. 34 (317) D. 35 (198) D. 36 (186) D. 38 (192) D. 36 (186) D. 38 (192) D. 37 (195) D. 38 (192) D. 38 (192) D. 39 (197) D. 30 (198) D. 31 (299) D. 32 (298) D. 34 (296) D. 34 (296) D. 35 (298) D. 36 (297) D. 34 (296) D. 36 (298) D. 37 (298) D. 38 (295) D. 39 (296) D. 30 (296) D. 30 (296) D. 30 (296) D. 31 (296) D. 32 (296) D. 33 (296) D. 34 (296) D. 35 (296) D. 36 (296) D. 36 (296) D. 37 (296) D. 38 (296) D. 39 (296) D. 30 (Ohio Poll	- Warning	
Pres. approval .30 (337) .25 (312) Economy .36 (337) .31 (313) Foreign affairs .31 (338) .21 (312) Persian Gulf .25 (338) .19 (313) Gubernatorial approval .27 (334) .20 (311) U.S. senatorial approval .19 (338) .13 (313) U.S. senatorial evaluation .12 (310) .02 (280) U.S. Senate pref. .54 (195) .53 (198) Douglas/Glenn .54 (195) .53 (198) Douglas/Glenn .54 (195) .53 (198) McEwen/Glenn .54 (194) .49 (197) Petro/Glenn .46 (187) .43 (185) Tatt/Glenn .51 (206) .54 (196) Consumer confidence .51 (206) .54 (196) Personal financial outlook .12 (336) .22 (308) Outlook U.S. business .19 (333) .15 (303) Outlook for major purchases .00 (329) .15 (299) Overall index .17 (325) .21 (287) 2. 2-11 Oct. 91 .52 (21) .23 (296) Prese, approval .34 (317) .28 (295)			
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Economy Foreign affairs Foreig		.30 (337)	.25 (312)
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U.S. Senate pref. DeWine/Glenn Douglas/Glenn Douglas/Glenn McEwen/Glenn Petro/Glenn Taft/Glenn Consumer confidence Personal financial outlook Outlook U.S. business Outlook for major purchases Overall index 17 (325) 21 (287) 2. 2–11 Oct. 91 Pres. approval General General Economy Foreign affairs Gubernatorial Approval U.S. senatorial evaluation U.S. Senate pref. DeWine/Glenn Douglas/Glenn McEwen/Glenn 56 (186) .54 (194) .49 (197) .49 (197) .40 (186) .51 (206) .54 (196) .54 (196) .53 (188) .90 (329) .54 (196) .53 (188) .20 (266) .20 (266) .20 (266) .21 (287) .23 (296) .24 (296) .25 (178) .26 (317) .27 (296) .28 (296) .29 (296) .20 (266)		.19 (338)	
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Personal financial outlook Outlook U.S. business Outlook for major purchases Overall index Overall i		.51 (206)	.54 (196)
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Overall index .17 (325) .21 (287) 2. 2–11 Oct. 91 Pres. approval General .39 (317) .28 (296) Economy .34 (317) .28 (295) Foreign affairs .21 (317) .29 (296) Gubernatorial Approval .26 (317) .24 (296) U.S. senatorial approval .24 (317) .23 (296) U.S. senatorial evaluation .23 (268) .20 (266) U.S. Senate pref. DeWine/Glenn .60 (186) .55 (178) Douglas/Glenn .57 (175) .50 (169) McEwen/Glenn .53 (184) .50 (181)			
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General .39 (317) .34 (296) Economy .34 (317) .28 (295) Foreign affairs .21 (317) .29 (296) Gubernatorial Approval .26 (317) .24 (296) U.S. senatorial approval .24 (317) .23 (296) U.S. senatorial evaluation .23 (268) .20 (266) U.S. Senate pref. .50 (186) .55 (178) Douglas/Glenn .57 (175) .50 (169) McEwen/Glenn .53 (184) .50 (181)			
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Gubernatorial Approval .26 (317) .24 (296) U.S. senatorial approval .24 (317) .23 (296) U.S. senatorial evaluation .23 (268) .20 (266) U.S. Senate pref. .50 (186) .55 (178) Douglas/Glenn .57 (175) .50 (169) McEwen/Glenn .53 (184) .50 (181)		.21 (317)	
U.S. senatorial approval U.S. senatorial evaluation U.S. Senate pref. DeWine/Glenn Douglas/Glenn McEwen/Glenn Douglas/Glenn			
U.S. Senate pref. DeWine/Glenn Douglas/Glenn McEwen/Glenn .50 (186) .55 (178) .50 (169) .53 (184) .50 (181)			
U.S. Senate pref. DeWine/Glenn Douglas/Glenn McEwen/Glenn .50 (186) .55 (178) .50 (169) .53 (184) .50 (181)			
Douglas/Glenn .57 (175) .50 (169) McEwen/Glenn .53 (184) .50 (181)	U.S. Senate pref.		
McEwen/Glenn .53 (184) .50 (181)			
McEwen/Glenn .53 (184) .50 (181)			
Detro/Class 40 (177) E0 (160)	• •	.53 (184)	.50 (181)
Petro/Gierin .49 (177) .50 (169)	Petro/Glenn	.49 (177)	.50 (169)
Taft/Glenn .51 (180) .48 (168)		.51 (180)	.48 (168)
Consumer confidence		05 (000)	4= (000)
Personal financial outlook .05 (309) .15 (292)			
Outlook U.S. business .14 (311) .20 (290)			
Outlook for major purchases .10 (304) .15 (282) Overall index .15 (293) .26 (275)			
Overall index .15 (293) .26 (275) 3. 20–30 Mar. 92		.15 (293)	.20 (2/5)
Pres. Approval			
General .49 (394) .47 (387)		49 (394)	47 (387)
Economy .45 (394) .37 (387)		.45 (394)	
Foreign affairs .32 (394) .30 (387)			
Gubernatorial approval .21 (393) .25 (387)			
Lt. gov. approval .18 (393) .20 (385)			
U.S. senatorial approval .21 (393) .20 (387)			
U.S. senatorial evaluation .21 (357) .21 (358)	U.S. senatorial evaluation		.21 (358)
Favorability rating	Favorability rating		
Jerry Brown .16 (119) .15 (119)	Jerry Brown		
George Bush .52 (352) .46 (337)			
Bill Clinton .46 (204) .33 (196)			
Michael DeWine .33 (62) .52 (68)			
John Glenn .19 (309) .23 (303)		.19 (309)	
Pres. vote pref., Bush/Clinton .67 (246) .57 (233)			
Pres. vote pref., Bush/Brown .56 (248) .44 (237)			
U.S. Senate vote pref38 (257) .41 (250)	U.S. Seriale vote pref.	.30 (257)	.41 (250)

TABLE 3 (continued) SURVEY Consumer Confidence Personal financial outlook Outlook U.S. business Overall index 4. 9–19 Sept. 92 Presidential approval Gubernatorial approval Lt. gov. approval U.S. senatorial approval Favorability rating George Bush Bill Clinton Michael DeWine MICHI- GALL UP 30 (382) .09 (377 .30 (379) .30 (379) .30 (379) .30 (379) .30 (379) .30 (379) .31 (379) .37 (374 .38 (366) .31 (356) .31 (356) .31 (356) .31 (356) .31 (356) .31 (356) .31 (356) .31 (356) .31 (356) .31 (356) .31 (356) .35 (374) .36 (303) .36 (309) .31 (89)
SURVEY GAN UP Consumer Confidence Personal financial outlook .20 (382) .09 (377) Outlook U.S. business .30 (379) .30 (377) Outlook for major purchases .19 (387) .17 (374) Overall index .33 (366) .31 (356) 4. 9–19 Sept. 92 Presidential approval, general .56 (478) .60 (411) Gubernatorial approval .23 (480) .20 (412) Lt. gov. approval .22 (479) .23 (412) U.S. senatorial approval .31 (479) .30 (412) Favorability rating George Bush .59 (453) .65 (374) Bill Clinton .59 (357) .64 (303)
SURVEY GAN UP Consumer Confidence Personal financial outlook .20 (382) .09 (377) Outlook U.S. business .30 (379) .30 (377) Outlook for major purchases .19 (387) .17 (374) Overall index .33 (366) .31 (356) 4. 9–19 Sept. 92 Presidential approval, general .56 (478) .60 (411) Gubernatorial approval .23 (480) .20 (412) Lt. gov. approval .22 (479) .23 (412) U.S. senatorial approval .31 (479) .30 (412) Favorability rating George Bush .59 (453) .65 (374) Bill Clinton .59 (357) .64 (303)
Personal financial outlook .20 (382) .09 (377 Outlook U.S. business .30 (379) .30 (377 Outlook for major purchases .19 (387) .17 (374 Overall index .33 (366) .31 (356 4. 9–19 Sept. 92 Presidential approval, general .56 (478) .60 (411 Gubernatorial approval .23 (480) .20 (412 Lt. gov. approval .22 (479) .23 (412 U.S. senatorial approval .31 (479) .30 (412 Favorability rating George Bush .59 (453) .65 (374 Bill Clinton .59 (357) .64 (303
Outlook U.S. business .30 (379) .30 (377) Outlook for major purchases .19 (387) .17 (374) Overall index .33 (366) .31 (356) 4. 9–19 Sept. 92 Presidential approval, general .56 (478) .60 (411) Gubernatorial approval .23 (480) .20 (412) Lt. gov. approval .22 (479) .23 (412) U.S. senatorial approval .31 (479) .30 (412) Favorability rating George Bush .59 (453) .65 (374) Bill Clinton .59 (357) .64 (303)
Outlook for major purchases .19 (387) .17 (374 Overall index .33 (366) .31 (356 4.9–19 Sept. 92 Presidential approval, general .56 (478) .60 (411 Gubernatorial approval .23 (480) .20 (412 Lt. gov. approval .22 (479) .23 (412 U.S. senatorial approval .31 (479) .30 (412 Favorability rating George Bush .59 (453) .65 (374 Bill Clinton .59 (357) .64 (303
Overall index .33 (366) .31 (356) 4. 9–19 Sept. 92 Presidential approval, general .56 (478) .60 (411) Gubernatorial approval .23 (480) .20 (412) Lt. gov. approval .22 (479) .23 (412) U.S. senatorial approval .31 (479) .30 (412) Favorability rating George Bush .59 (453) .65 (374) Bill Clinton .59 (357) .64 (303)
4. 9–19 Sept. 92 Presidential approval, general .56 (478) .60 (411 Gubernatorial approval .23 (480) .20 (412 Lt. gov. approval .22 (479) .23 (412 U.S. senatorial approval .31 (479) .30 (412 Favorability rating George Bush .59 (453) .65 (374 Bill Clinton .59 (357) .64 (303
Presidential approval, general .56 (478) .60 (411 Gubernatorial approval .23 (480) .20 (412 Lt. gov. approval .22 (479) .23 (412 U.S. senatorial approval .31 (479) .30 (412 Favorability rating George Bush .59 (453) .65 (374 Bill Clinton .59 (357) .64 (303
Gubernatorial approval .23 (480) .20 (412 Lt. gov. approval .22 (479) .23 (412 U.S. senatorial approval .31 (479) .30 (412 Favorability rating George Bush .59 (453) .65 (374 Bill Clinton .59 (357) .64 (303
Lt. gov. approval .22 (479) .23 (412 U.S. senatorial approval .31 (479) .30 (412 Favorability rating George Bush .59 (453) .65 (374 Bill Clinton .59 (357) .64 (303
U.S. senatorial approval .31 (479) .30 (412 Favorability rating George Bush .59 (453) .65 (374 Bill Clinton .59 (357) .64 (303
Favorability rating George Bush .59 (453) .65 (374 Bill Clinton .59 (357) .64 (303
Bill Clinton .59 (357) .64 (303
Bill Clinton .59 (357) .64 (303 Michael DeWine .36 (109) .31 (89)
(Michael DeWine .36 (109) .31 (89)
John Glenn .35.(352) .34 (312 Albert Gore .44 (237) .49 (212
Dan Quayle .50 (372) .49 (212
Pres. Vote Pref.
Bush/Clinton .78 (303) .79 (270
Bush/Clinton & Perot .76 (309) .76 (286
Bush & Pert/Clinton .77 (309) .77 (286
U.S. Senate Vote Pref55 (327) .54 (278
Consumer confidence
Personal financial outlook .14 (467) .09 (401 Outlook U.S. business .19 (455) .17 (394
Outlook U.S. business .19 (455) .17 (394 Outlook for major purchases .17 (456) .18 (394
Overall index .21 (427) .22 (373
5. 16–25 Oct. 92
Favorability Rating
George Bush .61 (311) .50 (320
Bill Clinton .67 (283) .61 (283
H. Ross Perot .17 (216) .04 (224 Michael DeWine .44 (150) .33 (134
Michael DeWine .44 (150) .33 (134 John Glenn .42 (259) .35 (283
Comfort Rating
George Bush .62 (330) .54 (343
Bill Clinton .66 (326) .53 (342
H. Ross Perot .07 (323) .03 (341
Pres. vote pref.
Bush/Clinton .77 (239) .69 (235
Bush/Clinton & Perot .65 (292) .59 (288 Bush & Perot/Clinton .70 (292) .61 (288
U.S. Senate vote pref
Best cand. in debates
Bush/Clinton .47 (184) .42 (221
Bush/Clinton & Perot .29 (309) .31 (328
Bush & jPerot/Clinton .41 (309) .33 (328
Next best in debates
Bush/Clinton .10 (187) .02 (200 Bush/Clinton & Perot .18 (296) .07 (317
Bush/Clinton & Perot .18 (296) .07 (317 Bush & Perot/Clinton .06 (296) .09 (317
Consumer confidence
Personal financial outlook .06 (407) .06 (417)
Outlook U.S. business .02 (381) .00 (393
Outlook for major purchases .06 (402) .10 (417)
Overall index .05 (368) .03(372)
6. 28 Oct.–1 Nov. 92 Favorability rating
George Bush .58 (528) .62 (524) Bill Clinton .61 (524) .65 (523)
H. Ross Perot .06 (520) .03 (508)
Michael DeWine .37 (473) .41 (480)
John Glenn .40 (518) .45 (512)

TABLE 0 (continued)		
TABLE 3 (continued)		
	місні-	GALL-
SURVEY	GAN	UP
Comfort rating		
George Bush	.57 (533)	.64 (524)
Bill Clinton	.62 (532)	.69 (521)
H. Ross Perot Pres. vote pref.	.10 (528)	.05 (519)
Bush/Clinton	.72 (386)	.77 (410)
Bush/Clinton & Perot	.57 (477)	.69 (480)
Bush & Perot/Clinton	.66 (477)	.69 (480)
U.S. Senate vote pref.	.50 (481)	.58 (469)
7. 3–10 Feb. 93		
Presidential approval, general	.37 (398)	.41 (407)
Approval	.57 (555)	.41 (407)
Hillary appointment	.22 (398)	.34 (408)
Gays-in-military plan	. 33 (397)	.27 (406)
Abortion policy decision	.17 (397)	.11 (404)
Gubernatorial approval, general	.19 (398)	.21 (407)
Gubernatorial approval, state		
budget	.16 (396)	.18 (407)
Consumer Confidence	OT (000)	44 (404)
Personal financial outlook Outlook U.S. business	.07 (388)	.14 (401)
Outlook o.s. business Outlook for major purchases	.11 (386) .01 (380)	.11 (391)
Overall index	.03 (364)	.01 (390) . 10 (373)
8. 3–14 May 93	.03 (304)	.10 (3/3)
Presidential approval, general	.62 (407)	.54 (369)
Approval, Hillary Clinton	.40 (406)	.33 (369)
Gubernatorial approval, general	.18 (406)	.19 (368)
Consumer confidence	, ,	` ,
Personal financial outlook	.17 (404)	.18 (366)
Outlook U.S. business	.12 (388)	
Outlook for major purchases	.07 (389)	.01 (348)
Overall index	.13 (370)	.27 (336)
Favorability rating Richard Celeste	00 (400)	00 (455)
Michael DeWine	.26 (166) .40 (82)	.33 (155) .52 (85)
Joel Hyatt	.46 (70)	.24 (53)
George Voinovich	.22 (211)	.25 (172)
U.S. Senate vote pref.,	(, ,	()
Celeste/DeWine	.62 (271)	.61 (244)
U.S. Senate vote pref.,	, ,	` ,
Hyatt/DeWine	.66 (261)	.67 (232)
Gubernatorial vote pref.,		
Celeste/Voinovich	.55 (292)	.54 (269)
Greater Cincinnati Survey 2–29 June 92		
Pres. vote pref.		i
Clinton/Bush	.70 (196)	.72 (196)
Clinton/Bush & Perot	.56 (284)	
Clinton & Perot/Bush	.51 (284)	
20 Nov14 Dec. 92		100 (201)
Pres. vote		
Bush/Clinton	.79 (304)	.80 (330)
Bush/Clinton & Perot	.69 (347)	.72 (370)
Bush & Perot/Clinton	.74 (347)	.75 (370)
3–14 May 93, Pres. approval,	C7 (450)	E7 (100)
general Cincinnati Area Project,	.57 (458)	.57 (488)
5 Oct.—16 Dec. 91		
Pres. approval, General	.49 (303)	.50 (329)
Note: The higher correlation in each comparemphasis. All coefficients are Pearson r's, w	ison has been	bolded for
that the predicted zero-order relationships ar	aa aae variabl e positive.	es coded so
	•	

ical conditions, we found that 70 of the correlations (52.2%) were higher when the Michigan form was used, and 57 (42.5%), when the Gallup form was used, with 7 tied cases (5.2%). And only 7 of these 134 comparisons produced a statistically significant difference, 4 or them being significantly higher on the Michigan form, 3 on the Gallup form—just about what would be expected by chance (7/134 = .052).

If we restrict our analysis to the surveys done during the campaign, the pattern is nearly identical. Of the 78 available comparisons, 41 (52.6%) show that the correlations were higher with the Michigan form, and 31 (39.7%), with the Gallup form, with 6 tied cases (7.7%).

If we confine the comparisons to those involving presidential approval ratings, we find there are 15 possible comparisons, 10 of which (66.7%) produce higher correlations with the Michigan form, and 4 (26.7%), with the Gallup form, with 1 tie (6.7%). Short-term "favorability" and "comfort" ratings of the presidential candidates during the campaign behave much the same way. Of 17 comparisons, 11 (64.7%) were higher with the Michigan form, 6 (35.3%), with the Gallup form. Presidential, senatorial, and gubernatorial trial heats tend in the same direction: the correlations are generally higher on the Michigan form (see Table 3). In contrast, the consumer confidence ratings were only weakly related to partisanship in most of the comparisons shown in Table 3 regardless of the question form that was used, though the correlations tended to be slightly higher with the Gallup form in a majority of the cases.

To summarize, the evidence from this correlational analysis of short-term evaluations and electoral preferences is mixed at best. If anything, the data suggest that the Michigan form of the question may be the better predictor of the two under a variety of political, economic, and electoral conditions—in this case, over a two-year period. And to the extent that such predictive validity implies higher reliability of measurement (see Green and Schickler, 1993), a rival hypothesis about greater *random* error with the Gallup form, as opposed to greater responsiveness in the sense of true variance, becomes more plausible as an explanation of at least some of the findings in this controversy.

Responsiveness to Question Order and Context Effects

We have one more piece of empirical evidence on the relative responsiveness of the two question forms. In the Greater Cincinnati Survey conducted shortly after the 1992 presidential election (20 November—December 14), we varied not only the wording of the party identification question but also the order and context in which it was asked. In one split-half of the sample it was asked just *before* a question about how respondents said they had voted in the presidential election (Bush, Clinton, or Perot); and in the other split-half,

it was asked just after the question on how they had voted.8

Now we already know that the wording of this question did not make a significant difference in the marginal distribution of responses to the two forms, as reconfirmed by the chi-squared value shown in Table 4 ($X^2 = .97$, df = 2, n.s.). But the order and context in which it was asked did make a significant difference in the aggregate results ($X^2 = 8.47$, df = 2, p < .02). When the party identification question was asked after the vote question, the percentage of Independent and Democratic identifiers tended to be significantly higher than when it was asked before the vote question, suggesting that it was indeed responsive to the recent election results. Now if the Gallup form is more responsive to short-term stimulation, then we would expect that it would be more sensitive than the Michigan form to this immediate (less than 30 seconds), contextual effect of making highly salient to the respondent, when answering the party identification question, how he or she had voted in the recent presidential election.

The analysis in Table 4, however, tells us that the order effect occurs with both the Gallup and the Michigan forms of the question, so that the three-way interaction is not statistically significant. Moreover, if there is any trend in these data, it is that the order effect was somewhat larger with the Michigan form: the difference in the percentage of Democratic and Independent identifiers was noticeably larger with the Michigan form than with the Gallup form. So here we have a clear experimental manipulation of the key theoretical variable of "short-term stimulation" in a postelection survey laboratory, and the evidence on the supposedly greater responsiveness of the Gallup form is not only negative, but it tends, much like the correlational evidence in Table 3, to run in the opposite direction, making the Michigan form look somewhat more responsive and, to that extent, perhaps more reliable and valid as well.

CONCLUSIONS

The weight of the evidence from the multiple experiments and replications we have carried out, plus that reported by Abramson and Ostrom (1994), as well as by Green and Schickler (1993), leads first of all, to one very clear conclusion: at any given point in time the distribution of political party identification responses will generally be the same, regardless of whether one uses the Gallup or Michigan form of the question. Furthermore, the data from our surveys conducted during the 1992 election campaign clearly do not support Abramson's and Ostrom's conclusions about the greater responsiveness of the Gallup form to the short-term forces operating during the campaign. For one thing, we did not find consistently more Republicans and Independents with the Gallup form or consistently more Democrats with the macropartisanship indicator. For another (and more importantly), we did not find that responses to the Gallup form

Political Party Identification by Question Form, by Question Order and Context, in the 1992 November-December Greater Cincinnati Survey (%)

		ORI	DER		
PARTY	REPORTED 92 VOTE/PARTY ID ^a		PARTY ID/REF	ORTED 92 VOTE ^b	
IDENTIFICATION	GALLUP	MICHIGAN	GALLUP	MICHIGAN	
Democrat	24.0	20.6	26.6	31.4	
Independent	43.3	48.0	37.3	37.2	
Republican	32.7	31.5	36.1	31.4	
Total	100.0	100.1	100.0	100.0	
N	263	248	241	226	
4	$^{a}X^{2} = 1.33$	2, df = 2, n.s.	${}^{b}X^{2} = 1.7$	1, $df = 2$, n.s.	

Note: Party ID by wording: $X^2 = .97$, df = 2, n.s.; party ID by order; $X^2 = 8.47$, df = 2, p < .02; three-way interaction (party ID by wording by order): $X^2 = 2.06$, df = 2, n.s.

were more consistently related to short-term evaluations and electoral preferences than the Michigan form during the campaign, before the campaign, or after the campaign. If anything, we found that responses to the Michigan form, not the Gallup form, were more responsive to such short-term forces, suggesting that the former may be the more reliable and predictively valid of the two. If the Gallup form does generate a slightly larger random error component (because respondents interpret the expression "in politics, as of today" in a random or quasirandom manner), that, and not systematic responsiveness would help explain some of the apparently inexplicable differences in variability between the two forms observed by Abramson and Ostrom. Random error, rather than true change, thus becomes a somewhat more plausible rival hypothesis.9

Finally, the convergent evidence from the analysis of all these experiments and replications makes it very difficult not to agree with MacKuen's, Erickson's, and Stimson's conclusion that "the underlying phenomenon of macropartisanship does not depend on any particular question wording for its substance" (1992, 479). (For a similar conclusion based on alternative measurements of party identification, see Franklin 1992). Given that the survey is conducted by the same organization, using the same mode of interviewing, the Gallup and Michigan forms of the party identification question can, for all practical purposes, be regarded as functionally equivalent, interchangeable indices. 10 Those who disagree with this conclusion bear a heavy burden of disproof; for, rumors to the contrary and analogous precedents (Bishop, Oldendick, and Tuchfarber 1978; Sullivan, Pierson, and Marcus 1978) notwithstanding, changes in question wording do not always make a significant difference in one's results and conclusions. Artifacts, as Schuman (1982) would put it, are often in the mind of the beholder.

APPENDIX: INTERVIEWING PROCEDURES, SAMPLING AND RESPONSE RATES

Interviewing Procedures

The data used in this paper come from eight statewide Ohio polls, five Greater Cincinnati Surveys (of Hamilton County, Ohio), a City of Cincinnati Survey, and a Greater Cincinnati Area Project. The Ohio polls, the Greater Cincinnati Surveys, and the City of Cincinnati Survey were conducted by the University of Cincinnati Institute for Policy Research (IPR) at the institute's centrally supervised Computer-assisted Telephone Interview (CATI) facility by interviewers trained at the institute. (The Cincinnati Area Project was a "paper-and-pencil" telephone survey; CATI was not used.) Most of the interviewers have been with the institute for several years (many for 10 or more), although a few students were used. Interviewers were not informed of the purpose of any of the experiments and have not seen any of the results of this research.

Sampling

Households were selected using random-digit-dialing procedures. First, with the aid of the computer, one of the three-digit telephone exchanges currently used in the area (e.g., 772) is randomly selected. The computer then randomly selects one of the "working blocks" (the next pair of digits, e.g., 64) and attaches it to the randomly selected exchange. Finally, the computer program then generates a two-digit random number between 00 and 99 (e.g., 57) and attaches it to the previously selected prefix (772) and working block (64), resulting in a complete telephone number (e.g., 772-6457). This procedure is then repeated numerous times by the computer to generate more random numbers, until we have a sufficient

quantity to conduct the survey. The end result is that each household in the area with a telephone, has an equally likely chance of being selected into the sample. The samples used in all 15 surveys were purchased from Survey Sampling, Incorporated, Fairfield, Connecticut.

In all 15 surveys, respondents were selected within households using the last-birthday method. If the number contacted is a residential number, the interviewer then randomly selects a member of the household by asking to speak with the person currently living in the household who is age 18 or older and who has had the most recent birthday. This selection process ensures that every adult (18 years of age or older) in the household has an equally likely chance of being included in the survey. No substitutions are allowed. If, for example, the randomly selected adult is not at home when the household is first contacted, the interviewer cannot substitute by selecting someone else who just happens to be there at the time. Instead, he or she must make an appointment to call back when the randomly selected person is at home. In this way, respondent selection bias is minimized. A selected respondent who refuses to participate in the study is recontacted by a senior interviewer, who attempts to complete the interview. This procedure, though costly, significantly reduces the percentage of respondents who refuse to complete a survey, thereby increasing completion rates.

Split-Half Assignment

In the eight Ohio polls, the five Greater Cincinnati Surveys, and the City of Cincinnati Survey, respondents were randomly assigned by the IPR CATI system to receive either the SRC form of the party identification ("Generally speaking") or the Gallup form ("In politics, as of today"). In the Cincinnati Area Project, respondents were randomly assigned to receive one of the two questionnaire forms.

Weighting

The data reported herein were all unweighted. An analysis of the same surveys using weighted data based on demographic characteristics of the respondents (age, education, race, and sex) produced results not differing significantly from those we report.

Completion Rates

Completion rates are shown in Table A-1.

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TABLE A-1						
Completion Rates						
William Walls William Walls				FAIL	URE TO INT	ERVIEW
SURVEY	HOUSEHOLDS CONTACTED	INTERVIEWS COMPLETED	% COM- PLETED	% REFUS- ALS	% INCOM- PETENT	% UNAVAIL ABLE ^b
Ohio Poll						
1. 24-30 Apr. 91	991	684	69.0	13.9	6.2	10.9
2. 2-11 Oct. 91	n.a	646		-		
3. 20-30 Mar. 92	1,273	816	64.1	10.7	5.6	19.6
4. 9-19 Sept. 92	1,468	976	66.5	16.1	5.9	11.5
5. 16-25 Oct. 92	1,450	940	64.8	11.2	5.3	18.6
6. 28 Oct1 Nov. 92	1,411	931°	66.0	7.3	3.2	23.5
7. 3-10 Feb. 93	1,276	854	66.9	13.4	4.9	14.7
8. 3-14 May 93	1,257	816	64.9 ⁻	17.7	4.5	13.0
Greater Cincinnati Survey ^d	•					
1. 29 May-14 June 91	1,538	1,028	66.8	13.4	10.7	9.0
2, 14 Nov3 Dec. 91	1,328	959	72.2	15.2	5.5	7.1
3. 9-29 June 92	1,361	954	70.1	13.1	5.2	11.6
4. 20 Nov14 Dec. 92	1,647	1,105	67.1	14.0	4.6	14.3
5. 20 May-2 June 93 City of Cincinnati,	1,556	1,055	67.8	15.2	4.6	12.5
27 May-3 June 93 Cincinnati Area Project,*	719	482	67.0	9.0	7.0	17.0
5 Oct16 Dec. 92	1,085	691	63.7	. —		

Due to language barrier, hearing problem, senility, illness, or illiteracy.

Due to business, vacation, or other cause.

The business, vacation, or other cause.

The business, vacation, or other cause.

The business, vacation, or other cause.

The business, vacation, or other cause.

The business, vacation, or other cause.

The business, vacation, or other cause.

The business, vacation, or other cause.

The business of the they voted in the 1988 general election and their stated level of interest in this particular election. Hamilton County, Ohio.

This was part of a Practicum in Survey Research conducted by graduate students in the political science department. None of the students involved was aware of the purpose of the experiment.

RESPONSE

For the past three decades political scientists have analyzed the changing distribution of partisan loyalties among the U.S. electorate. MacKuen, Erikson, and Stimson's (1989) study of aggregate levels of Gallup partisanship between 1945 and 1987 is especially important, because their time series analyses found substantial short-term variation in partisan loyalties, variation driven by short-term assessments of economic conditions. Their findings challenged the traditional view of party identification advanced by Campbell and his colleagues (1960) and led MacKuen, Erikson, and Stimson to argue that political scientists should shift their attention from long-term patterns of partisan change to short-term changes in the aggregate distribution of partisan loyalties.

MacKuen, Erikson, and Stimson's basic measure was the percentage of partisans who said they were Democrats, which they labeled *macropartisanship*. We were skeptical of these conclusions because they were based upon analyses of a question that seemed to tap short-term partisan sentiments, rather than long-term partisan loyalties. We argued that at least part of the variation in Gallup macropartisanship resulted from the short-term focus of the Gallup question itself (Abramson and Ostrom 1991).

Although aggregate-level analyses found that responses to the Gallup question were more responsive to short-term change than responses to the Michigan SRC question, these analyses could not definitely identify the cause of these differences. We recommended that political scientists conduct question-wording experiments that might shed light on why the Gallup and Michigan SRC results differed from one another (MacKuen et al. 1992, resp.).

Bishop, Tuchfarber, and Smith have conducted an extensive series of question-wording experiments. They present results from 15 telephone polls conducted in Ohio between April 1991 and June 1993 and compare results from the Gallup and Michigan SRC questions. They find few differences and conclude, "Given that the survey is conducted by the same organization, using the same mode of interviewing, the Gallup and Michigan forms of the party identification question can, for all practical purposes, be regarded as functionally equivalent, interchangeable indices" (953).

Except for a handful of experimental surveys, the Gallup and the Michigan SRC measures are not used by the same organizations, and since 1988, Gallup has relied mainly upon telephone polls. Therefore, even if the Gallup and SRC measures are functionally equivalent when used by the same survey organization, the actual Gallup data might have limited utility for making inferences about the Michigan SRC party identification measure. In fact, Bishop, Tuchfarber, and Smith's own discussion raises an important additional question about the comparability of the Gallup time series and results based upon the U.S. National Election Studies (NES). They point out that

in actual practice the Gallup organization has asked its partisanship question in a wide variety of contexts and that these differing contexts may contribute to greater variability in the Gallup time series.

Despite their arguments, the weight of the evidence suggests that the Gallup item itself is more responsive to short-term political forces than the SRC measure. Bishop and his colleagues challenge the question-wording experiments we conducted in six statewide surveys of Michigan between 8 March and 26 October 1992 (Abramson and Ostrom 1994). In fact, their findings differ from ours in only one minor respect. They find no systematic tendency for responses to the Gallup question to be more strongly related to short-term indicators (e.g., presidential approval, vote preferences, and consumer confidence) than responses to the Michigan SRC questions. We found that in 30 of 33 comparisons, the responses to the Gallup question were more strongly related to these indicators than responses to SRC₁ (the Michigan measure excluding independents), although, as we reported, only three of these differences were significant.

In other respects, our findings are either similar to those presented by Bishop, Tuchfarber, and Smith's findings or are based upon different types of experiments. For example, Bishop and his colleagues compare aggregate-level differences between alternative question formats in their surveys and ours, and we both find relatively small differences. We agree that during many periods there will be little difference between aggregate levels of macropartisanship, whether the Gallup or the SRC measure is used. But during periods of political volatility the Gallup preface will accentuate differences and the Michigan preface will attenuate them. The strongest evidence comes from the divergence between surveys using the Gallup and SRC measures conducted shortly after the Persian Gulf War, when surveys using the Gallup measure showed a surge in Republicanism but surveys using the SRC measure did not. (For a summary, see MacKuen et al. 1992, resp.). Bishop, Tuchfarber, and Smith's statewide Ohio survey conducted in April 1991 supports findings reached by national surveys and provides important new evidence, because differences between the two measures cannot result from house effects—that is to say, from differing procedures employed by different survey organizations. Fifty-one percent of the major party supporters were Republicans when the Gallup question was employed, but only 43% when the SRC measure was

used (see Table 2).

The period when our surveys were conducted was not as dramatic, but it was clearly a politically volatile time with negative sentiments toward George Bush among the electorate. We therefore predicted that Gallup macropartisanship would be more Democratic than SRC macropartisanship. This was true in five of our six surveys, and the mean difference across all six surveys shows the Michigan electorate to be 3.2 points more Democratic when the Gallup measure is used. Bishop and his colleagues present three polls of

the Ohio electorate conducted during this time period. In two of them Gallup macropartisanship is more Democratic than SRC macropartisanship, and in one the Ohio electorate is slightly less Democratic (see Table 2). But the mean difference for their three statewide surveys shows the Ohio electorate to be 3.4 points more Democratic when the Gallup measure is employed, a result remarkably similar to ours. Because small differences in macropartisanship scores can affect time-series estimates (Abramson and Ostrom 1991), the tendency for the Gallup measure to accentuate short-term change raises questions about using the Gallup time series to make inferences about the Michigan SRC measure.

Other findings we present are based upon different types of experiments from those Bishop and his colleagues conducted and are not discussed by them. For example, we present the results of two crosssectional surveys that compared responses among the same respondents who were asked SRC and Gallup questions about 12 to 15 minutes apart. About 1 respondent in 8 was classified differently. Granted, these differences could result from contrast effects, in which responding to the first partisanship question leads respondents to respond differently to the second. But our findings are remarkably consistent with those based on a national survey conducted in December 1973 by the National Opinion Research Center (NORC) in which differing partisanship questions were scattered over lengthy in-person interviews.¹¹

More importantly, our experiment included a four-wave panel study. 12 Our panel allowed us to compare respondents who were consistently asked the Michigan SRC questions with those consistently asked the Gallup question. Eleven over-time comparisons were possible and in all 11 comparisons, the responses to the SRC measure were more stable than the responses to the Gallup measure. In 6 of these 11 comparisons, these differences were statistically significant at the .05 level. These results strongly suggest that responses to the Gallup question are more subject to short-term forces than responses to the Michigan SRC question, although we recognize that an ideal design would have included additional control groups (e.g., a control group interviewed on only the first and fourth wave of our panel).

If most of our findings are either similar or unrelated to those presented by Bishop and his colleagues, why is there a controversy between us? The important issue is not why our results and theirs differ in the relative magnitude of correlation coefficients that are seldom significantly different from each other, regardless of whether the Gallup or SRC measure is employed. Rather, it is whether the Gallup time series can be used to reach conclusions about research that employs the Michigan SRC measure. A careful reading of their argument raises additional doubts about the utility of the Gallup times series. Bishop and his colleagues argue that over-time variation in the Gallup measure may result from the "more rapid turnover of topics, shifts in response to preceding questions about the most important problem(s) facing the country, presidential approval, candidate trial heats, and the like, as compared to the more slowly evolving, less variable contexts of the NES and the NORC GSS" (n. 9). The "variable context hypothesis" is important and should be tested. If contextual differences prove to be important, MacKuen, Erikson, and Stimson's (1989) findings would be called into question. Their analysis was not based upon all of the available Gallup surveys (in which the varying effects of contextual differences might even out) but upon the first Gallup survey of every odd-numbered month, aggregated into quarters. ¹³

Despite minor discrepancies between Bishop, Tuchfarber, and Smith's findings and our own, differences in question wording remain the most likely explanation for the greater aggregate-level variation in Gallup macropartisanship. Of course, the Gallup measure and the Michigan measure usually yield similar results, and cross-sectional surveys that employ the Gallup measure will usually yield similar results to those using the basic SRC party identification question. But political scientists should be cautious about using time-series analyses of the postwar Gallup data to reach conclusions about either the Michigan SRC party identification measure itself or the best way to study changing party identification among the U.S. electorate.

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Notes

The first part of this Controversy is the revised version of a paper presented at the 1993 meeting of the American Political Science Association, Washington; for their constructive comments, criticisms, and suggestions, Bishop, Tuchfarber, and Smith would like to thank Paul Abramson, Charles Franklin, Richard Niemi, Michael MacKuen, and James Stimson. Abramson and Ostrom are grateful to Ada W. Finifter, Jim Granato, and Brian D. Silver for their suggestions.

- 1. For a brief, recent overview of the meaningfulness and measurement of this central concept, see Niemi and Weisberg 1993a, 210–23. See also their review of the various controversies in the measurement of party identification (Niemi and Weisberg 1993b, 268–83). See, too, the recent special issue on party identification in *Political Behavior* (Franklin, Brody, and Sniderman 1992).
- 2. See Belknap and Campbell 1951/52, in which the Michigan SRC form of the question first developed, and Campbell et al. 1960, in which it evolved into the central, intervening variable of the "Michigan model" of American voting behavior. For a brief historical review of the measurement of party allegiance, party affiliation, and party identification as these concepts emerged in public opinion and electoral research in the United States, see Rademacher 1993.
 - 3. See, esp. Krosnick and Berent 1993; Weisberg 1980.
- 4. See the Appendix for documentation of the sampling procedures and response rates in these surveys and see Abramson and Ostrom 1994, app., for a comparison of the quality of these two data collections.
- 5. We want to thank Paul Abramson and Charles Ostrom for providing us with copies of various drafts of their manuscript, as well as copies of the questionnaires they have used

in their surveys. It illustrates well the spirit of scientific exchange between rival investigators with competing hypoth-

- 6. The correlations presented in Table 3 are based on a fully unfolded, seven-point party identification scale for both the Michigan SRC and Gallup forms of the question, which maximizes the measurement of variance in the responsiveness of these two forms to the various short-term indicators shown in this table. We have also done the same analysis of correlations, using Abramson's and Ostrom's (1993) dichotomous recoding of the Michigan SRC and Gallup forms, which either excludes "leaning independents" (SRCI and GAL1) or includes them (SRC2 and GAL2), with "true independents" deleted completely from the analysis. Our analysis shows that regardless of the recodings, the substantive conclusion remains the same—no significant difference (see Bishop, Tuchfarber, and Smith 1993, tbls. 3a-b).
- 7. A Z-test of the difference between correlations showed the following to be significantly higher with the Michigan form: (1) U.S. Senate vote preference in the April 1991 Ohio poll, (2) the favorability rating of Bush in the October Ohio poll, and (3) the comfort rating of Clinton in the same October Ohio poll, and (4) U.S. Senate vote preference in the same poll. The following were significantly higher with the Gallup form: (1) comfort rating of Clinton in the 28 October-1 November 1992 Ohio poll, (2) presidential vote intention (Bush vs. Clinton and Perot in the same poll), and (3) consumer confidence with respect to the outlook for U.S. business in the May 1993 Ohio poll. No pattern appears to exist in these significant differences beyond chance fluctuation.

8. Copies of these and other questions are available, upon request, from the authors.

9. The participants in this debate have also overlooked an equally plausible source of the variability among the different time series for the party identification question, namely, the influence of responses to previous questions in the interview—what we call "context effects." A casual inspection of the interview questionnaires on which these different survey time series are based will show considerable variability in the content of relevant questions preceding the party identification item, not only across organizations (Michigan, Gallup, NORC GSS, and CBS News/New York Times) but also within the same organization over time (see any of the cumulative codebooks for these surveys). The apparently greater variability of the relatively frequent Gallup (and CBS News/New York Times) polls may thus reflect their more rapid turnover of topics, shifts in responses to preceding questions about the most important problem(s) facing the country, presidential approval, candidate trial heats, and the like, as compared to the more slowing evolving, less variable contexts of the NES and the NORC GSS (see the cumulative codebooks). This explanation would fit with Abramson's and Ostrom's observations of the variability among the different time series, while allowing us to reject their rival question-wording hypothesis. Furthermore, having ruled out "house effects" as an alternative explanation with our in-house set of experiments and replications, there are hardly any other plausible, contending explanations left but context effects. (For a recent perspective of this literature, see Schwarz and Sudman 1992.) Indeed, the plausibility of the variable context hypothesis has grown with the recently published report by Johnston (1992) demonstrating, experimentally, that responses to party identification questions are affected by wording, depending upon the order and context in which they are asked. (On how the order and context of candidate preference questions may account for apparent variability in poll results, see also Frankovic 1992.)

10. We have also compared the variance of the responses to the two forms in each of our 15 experiments and found no significant differences in any of them nor any pattern of differences (data not shown here). If the phrase in the Gallup form "In politics, as of today" makes it somehow more responsive to short-term events at any given point in time, we would expect to see some evidence of greater variability. But none exists, making the Gallup responsiveness hypothesis even less plausible.

11. See Green and Schickler 1993 for an analysis of this survey. According to our recalculations of results they present (1993, tbl. 8), 15.5% of 1,489 respondents who were asked both the Gallup and Michigan SRC partisanship questions responded differently to them.

12. In an earlier version of their study, Bishop, Tuchfarber, and Smith (1993) discuss our panel analyses. In the present critique, however, they do not mention that we conducted a

four-wave panel.

13. For the second and fourth quarter of each year, Mac-Kuen and his colleagues rely upon a single Gallup survey. For most of the period they study, the Gallup organization conducted three to six polls per quarter. Bear in mind, however, that the variable context hypothesis has not been supported by any direct experimental evidence. If MacKuen, Erikson, and Stimson's results were replicated by using all available Gallup surveys (as aggregated quarterly), these findings would render the variable-context hypothesis implausible. Contextual variation may account for more "noise" in the Gallup time series, but it is difficult to see how it can account for the systematic tendency of Gallup macropartisanship to be more strongly related to short-term change in attitudes toward the economy than are short-term changes in Michigan SRC macropartisanship.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN MARCH, 1995

The following articles, research notes and controversies have been tentatively scheduled for publication in a forthcoming issue.

Charles O. Jones. "A Way of Life and Law." 1994 Presidential Address.

Larry Arnhart. "The New Darwinian Naturalism in Political Theory."

Kathleen Bawn. "Political Control versus Expertise: Congressional Choices about Administrative Procedures."

Robert D. Brown. "Party Cleavages and Welfare Effort in the American States."

Gregory A. Caldeira and James L. Gibson. "The Legitimacy of the Court of Justice in the European Community: Models of Institutional Support."

Dana Chabot. "Thomas Hobbes: Skeptical Moralist." Jorge I. Domínguez and James McCann. "Shaping Mexico's Electoral Arena: The Construction of Partisan Cleavages in the 1988 and 1991 National Elections."

Milton Lodge, Marco R. Steenbergen, and Shawn Brau. "The Responsive Voter: Campaign Information and the Dynamics of Candidate Evaluation."

Peter Nardulli. "Critical Realignments, Electoral Behavior, and Political Change."

Karen Orren. "The Primacy of Labor in American Constitutional Development.'

Stergios Skaperdas and Bernard Grofman. "Modeling Negative Campaigning."

Piotr Swistak and Gzregorz Lissowski. "Choosing the Best Social Order: New Principles of Justice and Normative Dimensions of Choice.

Lonna Rae Atkeson and Randall W. Partin. "Economic and Referendum Voting: A Comparison of Gubernatorial and Senate Elections." A research note.

James C. Clingermeyer and B. Dan Wood. "Disentangling Patterns of State Debt Financing." A research note.

Richard L. Hall and Robert Van Houweling. "Avarice and Ambition in Congress: Representatives' Decisions to Run or Retire from the U.S. House." A research note.

Bradford Jones, Benjamin Radcliff, Charles Taber, and Richard Timpone. "Condorcet Winners and the Paradox of Voting: Probability Calculations for Weak Preference Orders." A research note.

Randall D. Lloyd. "Separating Partisanship from Party in Judicial Research: Reapportionment in the U.S. District Courts." A research note.

Laura Stoker and M. Kent Jennings. "Life-Cycle Transitions and Political Participation: The Case of Marriage." A research note.

Christopher M. Duncan; Shelley Burtt. "Civic Virtue and Self-Interest Rightly Understood." A controversy.

Mary P. Nichols; Robert C. Bartlett. "Aristotle's Science of the Best Regime." A controversy.

Statement of Ownership, Management and Circulation

Required by Act of October 23, 1962 Section 4369, Title 39, United States Code

1. Date of Filing: October 14, 1994.

2. Title of Publication: American Political Science Review.

Frequency of Issue: Quarterly in March, June, September, December.

4. Location of Known Office of Publication:

1527 New Hampshire Avenue, N.W., Washington, D.C. 20036. 5. Location of the Headquarters or General Business Office of the Publishers:

1527 New Hampshire Avenue, N.W., Washington, D.C. 20036. 6. Names and Addresses of Publisher and Managing Editor:

Publisher: American Political Science Association

1527 New Hampshire Avenue, N.W., Washington, D.C. 20036. Manager Editor: G. Bingham Powell, Jr., American Political Science Review,
Department of Political Science, University of Rochester, Rochester, NY 14627.

7. Owner: American Political Science Association

1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.

8. Known Bondholders, Mortgagees, etc.: None.9. The purpose, function and nonprofit status of this organization and the exempt status for federal income tax purposes has not changed during the preceding 12 months.

10. Extent and Nature of Circulation:

	Average No. Copies Each Issue During Preceding 12 Months	Average No. Copies Single Issue Nearest to Filing Date
A. Total Number Copies Printed	16,560	560,560
B. Paid Circulation	•	ŕ
1. Sales	0	0
2. Mail Subscriptions	15,643	15,422
C. Total Paid Circulation	15,643	15,422
D. Free Distribution by Mail, Carrier or Other Means	127	152
E. Total Distribution (C + D)	15,770	15,574
F. Office Use, Left-Over, Unaccounted, Spoiled after Printing	79 0	986
G. Total	16,560	16,560

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CONTESTING CARE

JEAN BETHKE ELSHTAIN University of Chicago

The Problem of the Passions: Feminism, Psychoanalysis, and Social Theory. By Cynthia Burack. New York: New York University Press, 1993. 140p. \$35.00.

York: New York University Press, 1993. 140p. \$35.00. Disputed Subjects: Essays on Psychoanalysis, Politics, and Philosophy. By Jane Flax. New York: Routledge, 1993. 183p. \$49.95 cloth, \$15.95 paper. Feminist Morality: Transforming Culture, Society, and Politics. By Virginia Held. Chicago: University of Chicago Press, 1993. 285p. \$42.00 cloth, \$17.95 paper.

Moral Boundaries: A Political Argument for an Ethic of Care. By Joan C. Tronto. New York: Routledge,

1993. 226p. \$49.95 cloth, \$16.95 paper.

Gendered Community: Rousseau, Sex, and Politics. By Penny Weiss. New York: New York University Press, 1993. 189p. \$40.00.

meminist theory is a moving target, difficult to stabilize. Indeed, for many feminist theorists especially but not exclusively those associated with post-modernism—any attempt to fix the object of feminist discourse and to refine that discourse definitively is a dangerous aspiration tethered to discredited meta-narratives of logocentric mastery. ("The Enlightenment" usually comes into play here as the moment when such architectonic theoretical ambition crystallized for Western thinkers.) Other feminist theorists, however, believe that stabilizing the discourse is, or should be, a central ambition of any feminist theory worthy of the name. To do less, they insist, is to shrink from necessary philosophical tasks and exigent political actions. There are, of course, feminist thinkers who find themselves unable to go along with either post-modernism or its strongest discursive opponent and who prefer instead to deal competently and modestly with reinterpreting texts or pushing for an achievable set of political objectives.

The books under review offer examples of each genre and help to give a sense of the scope and diversity of current feminist scholarship. Whoever hopes to settle "once and for all" what feminism is about will scarcely be comforted by the sharpness of the issues joined here. Much of the most withering fire by feminist thinkers at this point is reserved for discursive and political opponents within feminism. To some this is a source of concern, even sadness. To others (and I include myself in this second camp), argument is what it is all about. After all, feminism historically has been defined in part by contests generated over its meaning. The diverse history of feminisms in the plural forms the basis of current feminism discourse and debate. Feminists can be found who push for the achievement of an egalitarian society, but there are others who, in the words of Jane Flax, doubt "that equality can mean anything" other than assimilation to a preexisting male norm

(p. 113). There are feminists who embrace a strong notion of women's difference and others who minimize distinctions between the sexes.

What is being contested in the five books under review? Just about everything, save the need for feminist theory and a feminist-inspired politics, it seems. But a number of key issues surface again and again. They are perhaps best summarized in the form of questions: What is the actual or desirable relationship between political and social theory and political action or public policy? Is there a uniquely feminist way of construing this relationship? Is there a distinctive feminist mode of moral theorizing? Is there a distinctively female mode of moral thinking? Are women, in fact, more caring than men? If so, what does this mean and how does the felt imperative to care arise? Should feminists evoke maternal imagery as a symbolic representation of a worthy moral and political ideal? What of the self itself? Is there a self that can be pointed to or achieved, and can this attained or achieved self serve as the ground for political action? Does this self, whether "unitary" or provisional," have anything to do with citizenship? I shall cluster these questions into three broad themes and take each up in the texts in question: (1) Feminist and female moral thinking and the question of care, (2) The self and politics, (3) The relationship between feminist theorizing and political action.

One way or another, each author either endorses or challenges what might be called the "turn toward care" associated with the work of Carol Gilligan and Sara Ruddick, among others. For Cynthia Burack, care has been reconceptualized by feminists as part of "the feminine capacity for nurturance, love, empathy" (p. 2). But she fears that an "analytic gap has emerged within feminist theory," insofar as feminists have failed to investigate fully gender identity. That identity has something to do with what Burack calls "the disagreeable passions," such as rage and hatred (pp. 3, 5). Feminist theory would benefit, she insists, if it took seriously these passions rather than persist in maintaining an untheorized ambivalence toward them. Burack would "reintegrate" the disagreeable passions, but it is never really very clear what this means or what such a reintegration would help us to attain—other than a presumptively more realistic assessment of human capacities. Burack, critical of Gilligan for an overemphasis on empathy, care, and love, puts her own money on object relations theory, as embodied in the work of Melanie Klein. Reiterating her basic theme, Burack claims that an integration of the "disagreeable passions" would help to complete a feminist "transformative vision of selfhood and social relations" (p. 23). We are told that these unsavory passions "are a feature of social and political life" (p. 46). But we are not offered a robust account of what features of social life the disagreeable passions might have a central role in constituting. Burack's critique of Gilligan for her one-sidedness is not as compelling as it might be because her call for an account of the "disagreeable passions" is not followed by such an account. We are asked to join Burack in the hope that a reconstituted self—in the form of a "self-critical feminist object relations theory"-would offer not only a more complete view of the self but a more compelling vision of feminist theory in relation to social transformation, as well. An agreeable book, clearly written, Burack lays on the "we need to's" pretty thick, but the book is thin on the ground when it comes to showing what integrated "disagreeable passions" would look like in political life, that is what form or forms these might take. Concrete examples of what can be laid on the doorstep of an unintegrated vision of self (one that denies the "disagreeable passions") by contrast to the preferred integrated self who has come to terms with these passions would have made for a more compelling volume. I think of war, for example, as a pervasive human institution. Does this arise from integrating or failing to integrate the "disagreeable passions?" Burack is surely correct that no political and social theory in this day and age is likely to persuade unless some mention is made of the "self" or the "inner life." But she has failed to specify how a reconstructed self molded on the object-relations model might think and act politically in ways significantly different from the old unintegrated self.

At first glance Penny Weiss's analysis of the theme of gender in Jean-Jacques Rousseau would seem to have little to do with the concerns that currently preoccupy feminist thinkers. Indeed, her book starts out rather traditionally (if the recent history of feminist political theory is one's benchmark.) She acknowledges that the ground has been well been covered in the matter of Rousseau and she makes the pitch—one not unfamiliar to readers of earlier treatments of Rousseau on women and politics—that one cannot hive off bits from Rousseau's political texts and sever these from such works as The Emile. Fair enough. Weiss's own take is that Rousseau is internally consistent in this sense: sexual politics is central to his work throughout. And she also makes a strong pitch (her second aim) for why it is important to continue to study "major" (she puts this in scare quotes) historical figures in political philosophy. Her treatment of Rousseau is admirably balanced, particularly her insistence that the ostensibly free Emile is in fact as "manipulated by his education" and his condition is as "marked by limitations" as that of Sophie (pp. 25, 29). Restraint, for Rousseau, is what it is all about. Thus Sophie is no more a "helpless slave" than Emile is a "tyrant" for each is required by Rousseau to see her or himself "as parts of larger wholes, to which they contribute and from which they receive necessary and desirable benefits" (p. 32). Well and good.

How, then, does Weiss join the issues I have located as organizing themes of the texts under

review? In the following way. She insists, first, that there is "good reason to be suspicion of the creation and enforcement of systematic differences." In other words, she comes down on the side of equality, fearing that "difference" is "often nothing more than a palatable name for inequality." And yet she does not want to efface difference entirely. Some differences, "such as cultural ones, can be enriching. Assimilation and uniformity are clearly not always to be preferred" (p. 35). How to promote good differences and forestall bad inequalities is not made clear, although Weiss makes the welcome point that feminists "should not be too quick to point to any role difference as proof of inequality" (p. 74).

At this point, the wheels begin to grind a bit and the joints to creak. Despite her riposte concerning role difference, Weiss concludes that "sex roles, in fact, are self-defeating and illegitimate" (p. 89). The strong term of opprobrium-illegitimate-seems to come out of nowhere at this point as does Weiss' conclusion that equality "requires abolishing the political categories of 'woman' and 'man' " (p. 91). I am unsure how much slippage in the argument is going on here because Weiss does not explain what the political categories of woman and man have to do with biological femaleness or masculinity; how we might "condition" children to respect difference but not to be "political" men or women; nor what these categories have to do with an ideal of citizenship available to both men and women in a democratic society.

She tips her hand in the direction of Carol Gilligan, but it is difficult to endorse Gilligan and to call for elimination of the categories of man and woman at one and the same time. Her final word on Rousseau is that he offers "no convincing account of the unhappiness of women who are fully immersed in traditional gender roles and family structures" (p. 115). She does not consider the possibility that unhappiness just may be the human condition. What about men "fully immersed in traditional gender roles?" The male tradition of going to work every day, day in and day out, year after year, in order to support a family probably did not yield giddiness. A final problem with Weiss' texts is that, in the second and weaker section of her book devoted to "Feminism and Communitarianism," she decocts feminism to one conception of the self and sees this feminist self as a clear-cut alternative to "abstract individualism." But surely this bumps up against her insistence that man and woman as categories must be effaced, particularly as she goes on to emphasize the ways in which relations between mothering persons and children—"characterized by more care and concern and openness and trust and human feeling"—should serve as models for all relationships (p. 137). Would she include citizenship here? Rulers and ruled? Or would the latter be eliminated in a future caring world?

I will take up Weiss' call for an "unbiased political theory" at the conclusion of this review, but let me note one unfortunate lapse. Given her expansive and admirable interpretive generosity toward Rousseau, it is unfortunate that Weiss blindsides Nietzsche with one curt interrogatory and one snappy injunction from *Thus Spake Zarathustra*, to wit: "Do you go to women? Do not forget the whip!" To which Weiss replies bitterly: "Thus spake misogyny." My question is: Why should Nietzsche be exempt from the interpretive complexity Weiss brings to bear in the case of Jean-Jacques? Do we have different hermeneutical rules for different thinkers?

Virginia Held, a distinguished feminist philosopher, belongs solidly in the camp of those who see post-modernism as a threat and who yearn for a world-view that aspires to completeness and serves as a clear and direct goad to transformative social action. This is an admonitory volume, dominated by "shoulds" and "oughts," relentlessly injunctive. Held claims, first, that feminism is "transforming the ways in which we think and live and express ourselves" and, second, that feminism must go even further, or feminist philosophy must, in offering up a general understanding "of reality, knowledge, methods of inquiry, and morality" (p. 16). The subject and object of her discourse is a we. "We often address... We often argue . . . We look to culture. . . . " She admits that feminists "are . . . not a unified group. We all belong to other groups, such as those that make our racial or class or ethnic backgrounds, and thus our experiences, very different" (p. 1). This is a bit odd on the face of it. Is being a member of something as amorphous as a "race" or a "class" a form of membership in any coherent sense? Held assumes so but does not argue the point.

I will reserve her strong tethering of feminist philosophy to direct action for my conclusion but it should be noted that Held is convinced that postmodernism sends feminism off the rails because it contributes "to a loss of political activism among those engaged in women's studies. "This, too, is odd in several senses, first, because Held sees those laboring in the vineyard of women's studies as being on the forefront of feminist political action and anything that deters them from this mission must be gainsaid and, second, because she attributes such enormous political power to philosophy. Without a philosophic prod action withers, apparently. Thus it comes as no surprise that Held's ubiquitous we requires "detailed guidance, and we need to assemble arguments for our various recommendations" (p. 9).

When her we isn't going about this forbidding task, she (individually and collectively) must force moral theories to confront lived life and, as well, embrace an "approach to morality" that "might give a felt relationship of trust priority over principle and seek a morality compatible with this priority" (p. 33). In other words, the "felt" relationship is the tail that wags the principled dog. Held has high hopes for what might result, nothing less than an alternative to moral theories "constructed from male points of view," hence not "gender neutral" (p. 43). Care enters as that which is to be embraced and that which feminist moral theory celebrates and locates as central. "Caring, empathy, feeling for others, being

sensitive to each other's feelings—all may be better guides to what morality requires . . . than may abstract rules of reason or rational calculation," Held writes (p. 52). She accepts, with no hint or skepticism, Gilligan's "different voice" ("we already have evidence") and she claims that men have had "fewer occasions for experiencing the moral problems of family life" than have women (p. 69). On the face of it, this is a rather extraordinary claim but it goes undefended.

Held places a tremendous moral and political load on the "activity of mothering." Although, in a somewhat off-handed way, she suggests that the activity of "mothering" (is there, then, to be no more "fathering"?) is as open to men as to women, men are quickly effaced as Held avers that it is women who are the persons who create other human beings. . . . Men produce sperm and women produce babies, and the difference is very great" (pp. 80-81). It is unclear what sort of claim this is other than a polemical one. Are there ontological implications? Ethical concerns? It would seem so for Held goes on to make, and then to repeat, a claim along these lines: Women suffer "severe pain for the sake of new life." This suffering leads to a "natural tendency to value what pain has been endured for' (pp. 82-83). Her repetition of this melodramatic claim is even worse, by which I mean more suspect from a moral point of view, as it makes the child's "value" contingent upon a woman's suffering. She insists: "The reason, if it is one, that the child should be valued because I have suffering to give the child life is different from the reason, if it is one, that the child should be valued because someone unlike me suffered to give the child life" (p. 85).

On this shaky foundation, Held hopes to build a feminist morality within which the flourishing of children would be central. She lashes out against the mass media as a perverting influence; offers up a short list of human rights that locates "freedom of expression" near the top of the list; repeats the injunction ("we need . . . we will need") to create theories of social change and to redesign institutions. But when it gets down to brass tacks, Held's own "redesign" is strikingly thin. For example, her three page discussion of "Violence, War, and Politics" nowhere comes close to coming to grips with the pervasiveness of organized, rule-governed violence. Nation-states are bad, we learn, because they are built on a willingness to use violence; politics itself is suspect because it is but an extension of the "male barracks community" of "ancient warrior-heroes," an ahistoric formulation that cannot even account for the profound shift from the Homeric age to that of settled polis life—let alone anything beyond. Held segues from this fable of one long unbroken tale of warrior-heroes to the curt suggestion that men think of their children as "replaceable." Men may be "naturally" deficient in this regard, being "more indifferent to particular children than has been considered." Held produces no evidence to sustain this harsh indictment. There is a vast literature expressing fatherly love and devotion that one might take up before saddling men with a noxious indifference to their particular children.

Having trotted out the word "naturally" several times, it comes as a bit of a surprise when Held excoriates any suggestion that women are "more immersed in biological processes, more determined by instinctive and other 'natural' forces than men" (p. 151). At this point Held becomes something of a social constructionist, one who wants to begin with the "point of view of women" and to reconceptualize an entirely new society with "the dynamic relation between mothering person and child" as the "primary social relation" (p. 213). In this way and this way alone will humanity finally reject domination and attain "harmonious relations" between human beings and the global environment (pp. 225-226). What such harmonious relations would look like we are not told but, then, since "every domain of society needs transformation, from the military, law, politics, and economic activity, to cultural production, education, the health and welfare systems, and the family," we need not hold our breaths (p. 214). We'll be lucky to get health care and welfare reform any time soon so utopia is rather far off.

Jane Flax's defense of a psychoanalytic, post-modern feminism serves as a direct rejoinder to Held's project. Flax calls the hankering for a "unitary representation" a wish "to deny or avoid the many conflicts among women" (p. 27). She is open to those "disagreeable passions" of which Burack writes. Her perspective is of one deeply pragmatic ("We adopt the knowledge that fits our uses") and suspicious of "confidence in the integrity and powers of reason" (pp. 31, 52). Psychoanalysis is central in challenging various Enlightenment "ontological premises" and "positing various forms of desire . . . rather than reason as the definitive and motivating core of our being." Flax's self is provisional, pleasure seeking, alive in a world of subjectivities yearning "for multiplicity" which "can impel them toward emancipatory action" (p. 93). Indeed, attention to the process of gendering itself "reveals the delusory character of self-determination, individualistic, and autonomous ideas of subjectivity" (p. 97).

There is a good bit of repetition in Flax's text, a series of essays thematically united by her brief in behalf of "multiplicity, randomness, and contingency" by contrast to "order and purpose" (pp. 99–100). Although her rejoinder to high Enlightenment hopes (that "reason" will make us "free") seem to me well taken, Disputed Subjects is a bit frustrating from time to time as so much of it is devoted to questions, sometimes entire pages. We learn that we are not to deny our own pleasures, including expressions of aggression. We learn, and it is an important lesson to learn, that no one no where at any time is simply an "innocent victim(s) outside circuits of power" (p. 68). But how does this play politically? Flax works on this question by challenging the "valorization of maternal images within contemporary feminist theorizing and the splitting of (good) maternity and (bad) sexuality" (p. xi).

For Flax, the maternal turn in feminism is suspect; it is "neither accidental nor innocent" (p. 59). To be sure, it is not and cannot be "innocent." No endorsement in this world is or should pretend to be. Flax makes this case well. But she is unconvincing in suggesting that there is something of a proto-conspiracy at work in the simultaneous emergence, on her reading, of maternalistic endorsements in feminism and the disruptive coming-into-view of "women of color," lesbians, and others who do not fit the "white women" feminist paradigm. If, as she insists, all knowledge is "fictive" and "non-representational," we need more instruction on how to simultaneously endorse (anything) and make claims in behalf of one's own claims. Learning that citizenship "is also another transitional practice that helps manage the strain between the subjective and objective worlds" isn't much of a help on this score, first, because it turns on the sort of binary poles Flax objects to quite strenuously and, second, because the public world or public life come down to those arenas within which I transform my "I want" into "I and others in my situation are entitled" (p. 125). But doesn't this mean politics, or anything that Flax would acknowledge as a political process, must always be built on wantsfrom "I want" to "we are entitled?" Isn't that, in fact, pretty much what we have and one reason so many despair of contemporary American politics?

Although Flax is on target in warning against the use of "parent-child relations as a direct model for citizenship or political action," her understanding of politics seems deficient in articulating a political "we" that is something more, or other than, an aggregate of "I want's." Enter Joan Tronto whose Moral Boundaries is the most solidly and emphatically political of all the texts under review. Tronto would probably not escape Flax's brief against the "valorization" of care, but Tronto is no sentimentalist. Tronto hopes to re-situate care, framing it as hard hands-on work undertaken variously by women and others, and being "distributed by gender, by caste and class, and often, by race and ethnicity as well" (p. x). Tronto is unabashedly in favor of a "liberal, democratic, pluralistic society," a refreshing and clear endorsement by contrast to, say, Held's more tepid and somewhat perplexing insistence that "feminist society would be likely to have democratic political processes" (p. 224).

Tronto accomplishes a great deal in her wonderfully well-written, cogent book. She calls attention to boundaries between morality and politics; between the moral point of view and alternatives to such; and between public and private life. She does not want to abolish the distinction between public and private but to consider how, thinking both strategically and in more visionary ways, these and other boundaries can be rethought and redrawn as part of a democratic, contested political process. Rejecting both deontological universalism and post-modern feminist theory, Tronto stays on the ground of practical reason. She parts company with feminist theorists who either

despair of liberal democratic societies or articulate romanticized alternatives to them. She argues in behalf of a political concept of care with care defined as "everything we do to maintain, continue, and repair our 'world' so that we can live in it as well as possible" (p. 103). Refreshingly, she sees strong antecedents to "women's morality" and a care perspective in Scottish Enlightenment thinking with its emphasis on sentiment and its locating of the household as an antidote to the corruption and vanity of the public world.

Rather than assuming that men and women have different senses of morality, Tronto asks why this question is so interesting to us. This is a very good move by contrast to a too hasty endorsement or dismissal of the idea. Her response is complex and distinctive. Because morality is "always contextual and historicized," it would be surprising it were not "deeply gendered" (p. 61). But that in and of itself does not tell us very much. In fact, the Kohlberg-Gilligan debate repeats rather than reshapes the boundaries within which we take up this matter. Tronto offers one of the strongest critiques of Gilligan I know, insisting that Gilligan's "different voice" isn't gendered in the way Gilligan claims and, moreover, that it winds up being an "account of partial privilege, not . . . an account of an alternative way to conceive of morality" (p. 63). Tronto accounts for why the Gilligan view has been so widely embraced by arguing that this version of gendered morality helps to preserve the distribution of power and privilege along a number of lines-gender, class, race, ethnicity, and education, among others.

A political concept of care, by contrast, one revolving around care as a complex practice, taps at one and the same time "universal" imperatives and "particular" commitments and helps the theorist to raise a variety of questions about the quality of care in any given society. Tronto doesn't want care relegated to the world of obligatory moral theorizing—a world of "ought's" and "owe's." Rather, she recasts the ethic of care along these lines: "How can I (we) best meet my (our) caring responsibilities?" This version of care is viable as a political ideal only in the context of democratic institutions. There are, to be sure, dangers in the care perspective, including maternalism and parochialism. I wish Tronto had said more about these dangers. In fact, it seems to me that one of her political endorsements—formal day-care as a policy provision of the United States government-might be called into question under her own care ethic. Many are uneasy about formal government-sponsored day care not because they don't care about care but because they worry about standardization, bureaucratization, and excessive "distancing" between care giver and receiver. Tronto's contrast between "private" day care, which she regrets, and her endorsement of public care on the grand scale is misleading. We know from reputable studies done on the subject that most mothers want in-community or in-neighborhood care; they want it to be small and personal; and they want it located in churches or other "spaces" with which they are familiar or to which they have some commitment. This is a complex mix of private and public imperatives rather than one or the other. It would, as well, be interesting to learn from Tronto what she takes to be the limits of the care ethic: when does care break off and punishment or severing of ties take over or become necessary?

Finally, a few words about theory and its connection to "experience" and to political action. Tronto has the most nuanced view, showing the ways in which our ideas and commitments enter into our political actions but in a way that is complex, not straight-forward. Burack has less to say on this topic but she, too, assumes the importance of theory for politics. Weiss wants nothing less than an "unbiased political theory," one that she defines as knowing "that the personal is political and the political is personal" (p. 160). This would exempt many thinkers, including Hannah Arendt, from the world of "unbiased theory," for Arendt not only knew no such thing, she criticized it explicitly. Held is most adamant in insisting that philosophic transformation must underscore political action, finding, as she does, a direct line from "understanding from a feminist point of view" to "making recommendations" for 'more suitable social arrangements" (p. 4). This is not a point Held argues; it is one she asserts. Flax, on the other hand, severs the connection pretty completely when she moves from an initial claim that there is no "straightforward" relationship between "theorizing and experience" to a later insistence that it is altogether unclear what contributions "knowledge or truth can make to the development of . . . feelings and communities" (p. 147).

It might be intriguing at this point to put a Trontolike question: Why do political theorists and philosophers continue to find it essential to justify their thinking with the notion that there is, or must be, some "pay-off" in the world? What happens when one makes an irenic move of Flax's sort and punts on the question? And, as badly battered as it is, might there not be something to the ideal, not so much of neutrality, as of impartiality as Arendt understood it, an impartiality that entered the world when Homer wrote that if the songs of the vanquished were silenced, "I myself will testify for Hector." For Arendt, any thinker or historian or theorist worth her salt must be one capable of such impartiality. This is an aspiration, or a role Arendt called that of "the guardian of historical truth," thus far conspicuous by its absence in feminist theory. Can there, in fact, be a feminist theory that embraces this Arendtian ideal? This might be an interesting turn for feminist discourse to take but I doubt very much it will happen, not so long as we remain pinioned in between calls to strenuous battle, on the one hand, or to the play of multiplicities of subjectivity, on the other.

POLITICAL THEORY

Recasting Conservatism: Oakeshott, Strauss, and the Response to Postmodernism. By Robert Devigne. New Haven: Yale University Press, 1994. 268p. \$28.50.

Michael Oakeshott and Leo Strauss have done as much as anyone in this century to revive political philosophy as a substantive field of inquiry. In very different ways their collected works mark a standing refutation of the obituary for political philosophy pronounced inter alia by Peter Laslett in the first series of Philosophy, Politics and Society (1956). Yet despite their preeminence in their chosen domains, there has not been, until now, a comparative study of their thought and influence. Robert Devigne's book represents a first step in that

Recasting Conservatism is, as its title suggests, an exploration of ways in which Oakeshott and Strauss have shaped recent developments in British and American conservative thought and, to a lesser degree, policy. It may come as something of a surprise that while both thinkers eschewed the role of public intellectuals and explicitly warned against confusing philosophy with political action, they are seen here as purveyors of powerful and influential brands of conservative ideology. There is the additional irony that neither Oakeshott nor Strauss thought of themselves as conservatives in any ordinary sense. Oakeshott's thought displays marked affinities, not with Burke, but with Hobbes, perhaps the least traditional of English political thinkers. Strauss often emphasized the degree to which philosophy as such is predicated upon a break with traditional sources of opinion and authority. To be sure, Devigne acknowledges that conservative opinions and beliefs have been shaped by many non-theoretical sources as well. Nevertheless, he finds evidence that their views have come to be widely shared by those occupying positions of power in both Whitehall and the U.S. State

The most interesting parts of the book are the contrasts it draws between the British and American styles of conservatism. Recent British conservatism has championed a revival of the state as the sole law making body and locus of sovereignty. Devigne interprets Oakeshott's magisterial On Human Conduct (1975) as the opening salvo in the conservative defense of the state as the authoritative propounder of law. Unlike the older conservatism with its defense of the "small platoons" (Burke) of society, the new conservatism, drawing on the authority of Hobbes, Bodin, and Hegel, has called for an undivided national sovereign that would put limits on alternate sources of authority often stemming from labor-dominated local councils.

In the U.S., by contrast, the new conservatism has pursued a twofold strategy. On the domestic front, conservatives have generally defended the mediating institutions of civil society as the proper locus for the cultivation of citizen virtue, for only at the local level do there exist opportunities to foster such virtue effectively. Yet at the federal level, they have favored a powerful, even imperial, role for the presidency in asserting a unified national interest. Devigne sees Strauss's Thoughts on Machiavelli (1959) as advocating a "poetic idea of greatness" as a model for executive action, especially in the struggle against Soviet communism. Straussian conservatives, then, have tried to revivify a concern for religion, public morality, and citizen virtue at the local level and a poetic desire for fame and glory at the national and international levels.

The difference between these two conservatisms can to some degree be traced back to Oakeshott's and Strauss' different readings of Hobbes. For Oakeshott, Leviathan represents the argument for the strong state combined with the free economy and private liberty. Oakeshott's Hobbes is not the defender of monarchical absolutism, but the theorist of "civil association" understood as a kind of pure procedural formalism. The purpose of sovereign authority is not to advance a national agenda or serve some common purpose but to lay down laws which prescribe certain "adverbial conditions" to be subscribed to in conduct. Problems arise when one confuses the state with some kind of "enterprise association" aimed at the attainment of some overriding end or purpose.

For Strauss, Hobbes' psychology of fear and pride almost perfectly expresses the dual model of governance that Devigne attributes to Straussian conservatives. These two dispositions not only represent two types of political ambition but two opposing social and psychological types. At the same time that Hobbes made the fear of violent death the universal passion that would underwrite his new egalitarian social order, he recognized that the aristocratic passion of pride ("vainglory" could not be altogether eliminated from human nature. The task for the Hobbesian statesman was to domesticate as far as possible the honor-loving aristocrat so that he might be made useful for enlightened or peaceful ends. Devigne cites Joseph Cropsey's seminal "The United States as Regime" to the effect that from its founding American politics has sought to find institutional means for balancing the demotic need for safety, security, and survival with the heroic desire for great-

ness and resplendent glory.

There is much in this book that is useful and enlightening. To understand political theory by examining its influence on broader intellectual and policy debates is a legitimate and worthwhile enterprise. However, to reduce thinkers of the stature of Oakeshott and Strauss to their influence (real or alleged) on the formation of public policy is to risk misinterpretation. One can virtually comb the collected works of both authors and find scarcely a word on current events and policies. Their writings are in fact rife with warnings against using political philosophy as an instrument for changing the world. Oakeshott explicitly distinguished political philosophy of the kind found in Plato's Republic, Hobbes' Leviathan, and Hegel's Philosophy of Right, which is necessarily thinking sub specie aeternitatis, from political ideology, which has the task of justifying and defending actions in terms of general principles or "isms." Likewise, Strauss distinguished between certain writers' esoteric and exoteric communication as a way of distinguishing true philosophy from the merely salutary opinions that a writer might from time to time espouse. Like

Oakeshott, Strauss warned that the politicization of philosophy necessarily leads to its corruption.

In treating Oakeshott and Strauss as conservative philosophers one necessarily puts philosophy into the service of a political cause. This is to miss the profoundly skeptical disposition of their philosophical politics. In one of his most beautiful expressions, Oakeshott called himself a skeptic who would do better if only he knew how. Similarly, Strauss described his philosophy as fundamentally "zetetic" or skeptical in the Socratic sense. The task of philosophy is not to offer solutions to current or timely problems, but to keep alive an awareness of "the fundamental and comprehensive problems." In his exchange with Alexandre Kojeve, Strauss remarked that "the sectarian is born" at the precise moment when a belief in our own certainty comes to outweigh an awareness of the problematic status of our beliefs. Philosophical skepticism may go hand in hand with a certain type of conservative inclination to maintain existing social and moral practices. In fairness, Devigne makes some effort to distinguish between their intentions and the views of those who have been influenced by them. But unless one grasps the fundamentally skeptical temperament of Oakeshott's and Strauss' philosophies, one will understand nothing of their politics.

In addition, there is the problem of paying Oakeshott and Strauss the honor of attributing too much political influence to their writings. Oakeshott's influence on British politics came largely through John Gray, Shirley Letwin, and the contributors to the Salisbury Review who had some sympathetic readers within the successive Thatcher governments. A number of Strauss' students (or students of his students) went into government service during the Reagan and Bush years, although at least one is currently a well-placed official in the Clinton administration. But it is unclear to what extent Oakeshottians and Straussians actively shaped the conservative agendas of their respective governments or simply took advantage of them. Harvey C. Mansfield's acerbic barb that Straussians went to Washington only after they could not find university jobs seems to indicate that the desire to change policy was not the career of choice. One could also add Dan Quayle's remark that he tries to read Plato's Republic every year both as evidence of the influence of Straussians in the Vice-President's Office and as vivid testimony to the limits of theory.

Recasting Conservatism is a book well worth reading for anyone interested in the relation between high theory and certain lower order political debates in the U. S. and Britain. The author's treatment of Oakeshott and Strauss is fair and even-handed throughout, neither defensively partisan nor angrily recriminatory. If I have been less than whole-heartedly enthusiastic in my endorsement, it is because I doubt that any work, however well-crafted, could render complete justice to the weight and complexity of this subject.

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Francis Bacon and the Project of Progress. By Robert K. Faulkner. Lanham: Rowman & Littlefield, 1993. 308p. \$55.00 cloth, \$21.95 paper.

The Francis Bacon in Robert Faulkner's book is a far more radical thinker and politic writer than one is likely to expect. Faulkner's Bacon is a man whose ambition

went beyond teaching humanity the know-how needed for operating masterfully on nature: he had a plan for operating craftily on human nature to produce modes and orders as new as could be. Faulkner makes the argument clearly and with a deep appreciation of fundamental alternatives in political philosophy. His comprehensive treatment of the Baconian alternative gives particular attention to the Essays or Counsels, Civil and Moral. While bringing out Bacon's artful indirection as an author, Faulkner provides many straightforward summaries of his own argument, so that the argument itself will be easier to follow even though the evidence cannot avoid being crabbed and even seeming strained. At times Faulkner may go too far—but perhaps not; and it can be said with confidence, even prior to the assiduous work of checking out Faulkner's many interesting interpretations, that he has done a great service by drawing upon close reading in a vast and multifarious body of texts and presenting clearly the argument that modernity was, to a surprisingly large extent, prepared as a project in the writings of Francis Bacon. To establish Bacon's originality and influence is not Faulkner's concern; rather, it is to grasp the scope of Bacon's plan and to judge the adequacy of its premises. Postmodern disillusionment with the progress of modernity can lead to nihilism, but it should lead to rethinking of the disillusionment at the root of modernity. Faulkner intends his book to contribute to such rethinking, and it is an excellent contribution.

Part I, on "the politics on enlightenment," opens with the thesis that Bacon had an extraordinarily comprehensive plan. His famous project of a new science for instituting human power over nature, Faulkner says, is defined by his thinking on human affairs. Not only the applications of his natural science but also its end (human power) and the sort of knowledge that it seeks (experimental laws of operation or production) are governed by his doctrine of the "self." Man, for Bacon, is above all a self in need of power, comprising first the worldly skills that can advance the self-made man and then the state of progressive civilization that can supply security to peoples and to humanity at large. The plan's purpose and principles conclude this part of the book: Bacon's project of progress is a fundamentally Machiavellian edifice, modified by improvements (for private security, limited government, economic growth, and conquest of nature) that make it a more humane, more civil, and more visionary version of its nasty original. The stages of Bacon's plan are surveyed in this part's central chapter, which examines his rhetoric by keeping in mind his various remarks about secretive, indirect, and manipulative discourse. Although various writings by Bacon are treated at length, Faulkner's book emphasizes the Essays. Each essay, Faulkner says, takes traditional or familiar opinions and turns them into new, enlightened opinions while disguising the comprehensive civilizational transformation that Bacon is bringing on. His writing appears disorderly because he must operate indirectly in order to be effective. Using cunning toward his followers as well as toward his foes, Bacon manipulates fear and vengeful anger, acquisitiveness and ambition, in order to subvert the opinions and institutions that support clerics and nobles and even, in the end, the kings under whom the building of the modern state is to begin. Bacon's collection as a whole moves, like each essay in it, from critique to construction—going from subversion of the most authoritative

received pieties at its beginning to production of a newmodel, progressive nation-state in its middle to intimations of a new-model progressive civilization at its end.

The rest of Faulkner's book sets out Bacon's plan for the conquest of human nature through the making of the self-made man able to contend in an alien world (Part 2) and then for a man-made state of the world

appropriate to such a self (Part 3).

Faulkner says, early in the book, that this last part, on "the state of progress," contains "the obviously important and political innovations" and that "the reader who wishes to reach quickly much of the meat should turn there first." In this part, Faulkner contends that Bacon's doctrine of the state is far from the Elizabethan monarchism with which it is often identified: rather, Bacon plans a nation-state of the type that we call modernone bent on economic and industrial growth and presided over by a republican but effectual government. Such growth-oriented nations will constitute a world of great powers but will harbor Bacon's science with a human face. Each of Bacon's major political writings does its own work to produce the new orders, by showing how enlightened despots can establish a state, how rising men can transform an established state into a civil state (a civil society and a civil government), and how progressive intellectuals and their allies among the mercantile class can introduce into the civil state a visionary scientific project for ameliorating human suffering. Bacon follows Machiavelli in criticizing classical political philosophy as useless and in promising effectuality through reliance on the most powerful of passions; but while Bacon continues the building of hopes on fear, his own specialty is the nourishing of hopes.

Part 2 treats Bacon's transformation of ethics into a radically individualistic doctrine of the self-made man. Individualism is the central doctrine of modern politics, Faulkner says, and the life of the self is central for Bacon; his humane science, radically rethinking human ends and needs, discovers a needy self that must make its own provision to the point of making its own world. Bacon produces first a critique of divinity and morality as well as philosophy and then a construction of arts that enable the self to manage itself and others into the power that will preserve itself and them. Nature, even human nature, is stingy: the good that men get-even their purposes—they make for themselves. Morality is realistic humanitarianism, mixing a show of provision for humanity's needs with primary provision for satisfaction of one's own passion to survive. To serve oneself, there is need for public policy and also for a public persona as an image to win followers. Indeed, public persona is the delivery of self: while fear makes us wish to save ourselves, the art of doing so originates in vengeful anger at our natural state of misery, and real revenge is a far-sighted domination that gives the self a life after death through the devising of a public image; this culminates in the image of a new-model Caesar who will blend civil supports with promises of humane benefactions to achieve a general and durable conquest of the minds of future generations—with Bacon himself revered, in the end, as humanity's true savior.

The very last chapter of the entire book evaluates the premises of Bacon's project, subjecting his teachings, both on nature and on human nature, to the same twofold critique: his reductionist reasoning begins by excluding various naturally given notions that human living cannot do without; and his construction work

makes use of an impoverished version of what his demolition work excluded at the outset. Faulkner criticizes the doctrine of productive experimentalism that is the scientific foundation of Bacon's work and the doctrine of the self-made man that is the moral core of it. Faulkner does not, however, criticize the Baconian modern civil order; Faulkner is a politic man, and that order is the regime under which we live. It is also the regime in which we prosper, and Faulkner is a humane man; the truly progressive achievements of the regime should be defended, Faulkner says. But, he argues, the Baconian foundation and its core will not provide the means to defend or even properly to estimate the achievements of the regime.

Where to look for help? In the middle chapter of part 2, Faulkner centers attention on Bacon's rejection of "the philosophical alternative"—the classical teachings on wisdom and friendship that Bacon sought to correct. Those who would rush into postmodernity, seeking an alternative to the doctrine that informs the Baconian project, can find, in Faulkner's book, reason to pause for a good long look back.

St. John's College, Annapolis HARVEY FLAUMENHAFT

Hegemony and Power: On the Relation between Gramsci and Machiavelli. By Benedetto Fontana. Minneapolis: University of Minnesota Press, 1993. 226p. \$44.95 cloth, \$16.95 paper.

Gramsci is almost the only communist leader to survive with honor in the "post" fields of so many recent currents of leftist thought. He has, in fact, not only survived but flourished, through the widespread dissemination of key concepts like hegemony and organic intellectual. Fontana notes early in his introductory chapter the multiple appearances Gramsci continues to make, depending on the interpreter: "the Crocean or Hegelian Gramsci, the Leninist Gramsci, the Jacobin Gramsci, the democratic-spontaneist Gramsci, the voluntarist Gramsci, and so on ad nauseam" (p. 2). The ad nauseam is rather too much like an expression of Crocean intellectual distacco for a book that otherwise takes very seriously Gramsci's insistence that philosophy must become a popular political force. After all, multiple Gramscis around is not automatically reason for contempt. It can mean that his ideas are caught up in, and inform, significant political conflicts of direction; and after this early and distancing gesture Fontana himself gets quickly to the business of interpretively pursuing the complications in Gramsci's analysis of Machiavelli.

Fontana's angle of entry is a crucial one, for while Gramsci has indeed survived with honor even in self-designated "post-Marxist" thinking, the sticking point remains Gramsci's modernist (no post) insistence on the revolutionary necessity of an organized, mass-based political party. And nowhere is that insistence more evident than in the notebooks concerned with Machia-velli. Laclau and Mouffe's summary comment in Hegemony and Socialist Strategy (1985) can serve as a typical "post" dissociation from at least this aspect of Gramsci's politics: "We will thus retain from the Gramscian view the logic of articulation and the political centrality of the frontier effects, but we will eliminate the assumption of a single political space as the necessary framework for those phenomena to arise" (p. 137, emphasis original).

I think it a fruitless debate whether one can thus pick and choose parts of a political philosophy or whether one has to swallow a system whole. Gramsci is not a system in any case; he is not medicine (good or bad) that one has to swallow; people will use what they find useful and discard the rest. The serious issue Fontana's analysis raises (if indirectly) concerns the "strategic" sense in such a summary dismissal of the relevance of party politics to a "post" world. Fontana's approach is indirect because focused, first of all, on a matter of interpretation: How exactly did Gramsci understand "political party" in relation to Machiavelli's thought?

For Fontana, Gramsci was interested in Machiavelli "as the thinker who first understood and first theorized the need for a politics that would recognize the presence of the 'mass' as a new factor in the power equation" (p. 8). Croce's response to Machiavelli's recognition was that if they cannot be kept out of the "power equation," they can at least be kept out of knowledge; hence one had better be sure that knowledge and power are clearly understood as two very different things. Machiavelli was about power, not knowledge. Fontana's claim, however, is that Gramsci's counterreading of Machiavelli is not only a challenge to Croce's attempt to keep philosophical knowledge and ethical direction distinct from politics as a purely "technical" field of power relations. More fundamentally, Gramsci's reading insists that Croce's very focus on that distinction is already a political displacement. What seems from Croce's liberal perspective both a threat and a mystery (What happens when the "mass" becomes "a new factor in the power equation"? is transformed into an intellectual dilemma that might then admit to philosophical resolution: What is the relation between knowledge and power? Croce's feat of prestidigitation is the assurance that if you can get the right answer to the second question, the first will solve itself and the power threat posed by "the mass" disappear. Gramsci's "anti-Croce" begins with Machiavelli, as a reminder that Croce was asking the wrong question.

Fontana's second and connected claim is more complicated and takes up much of the book in its elaboration. Briefly, the thesis is that if, unlike Croce, one begins with "the presence of the 'mass'" as a political factor, then one must find a way to understand politics otherwise than as domination. Thus, for Fontana, the figure of "the prince" in Machiavelli (as, correlatively, party" in Gramsci) does not mark the emergence of new types of political leaders and new forms of instrumental political power interests. It is, instead, a figure embedded in a process of social transformation that constructs the political order as a realm of freedom. The party as "modern prince" is then out "to teach the people a new political knowledge" (p. 151), Fontana allows. But he adds immediately that "this knowledge . . . must now be understood as one always internal to the people, because the democratic philosopher (the teacher) is himself a product of the people he intends to educate" (p. 151). The party, in other words, is neither a cadre of revolutionary leaders nor the intellectual representative of an already existent (if mute) "class subject." The party is the democratic philosopher produced by the people as they themselves becoming democratic philosophers in action. It is finally a figure of politics-as-freedom built out of the destruction of politics-as-domination.

Thus, like Laclau and Mouffe and other "post" theorists, Fontana stresses that popular identities are not a

given of the social field; they must be constructed. His reading of Gramsci, however, specifies that process of construction as an educative logic of social relations implicated in the politics of party. The issue of whether the political order is a "single" space or, as in Laclau and Mouffe, a matter of intersecting, multiple contingencies is less important than whether the reciprocity of educational relationships can inform the activity of every citizen-participant. Fontana finds the basis for Gramsci's conception of educational reciprocity in the formula Machiavelli introduces in both the Discourses and The Art of War: "The new knowledge that Machiavelli intends to introduce is here established by the relation 'io . . . da voi . . . come voi da me' ('I . . . through you . . . as you through me'), a relation that establishes the active dynamism of a knowledge that itself teaches the necessity for action" (p. 104). For Fontana, education in this Gramscian sense is a precondition for general political freedom, rather than a political strategy articulated by a representative party leadership.

The largest claim of the book is that no matter how "post," the reduction of politics to strategies, organization, and interests in fact continues a liberal, Crocean tradition of avoiding "the presence of the 'mass' as a new factor in the power equation." It remains within the field of politics as domination. To some great extent, this overarching claim is asserted rather than argued, for Fontana gives scarcely enough attention to recent political discourses to ground it. Nevertheless, the insight and the awareness of both historical circumstances and philosophical positioning that Fontana brings to bear on Gramsci's reading of Machiavelli seems to me an indispensable corrective to an all-too-familiar "post" assumption that a party politics engaged in fundamental, global conflicts is merely a thing of the past.

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EVAN WATKINS

The Politics of Community: A Feminist Critique of the Liberal-Communitarian Debate. By Elizabeth Frazer and Nicola Lacey. Toronto: University of Toronto Press, 1993. 268p. \$50.00 cloth, \$19.95 paper.

Like many other feminists, Elizabeth Frazer and Nicola Lacey, both of Oxford University, are dismayed at the nearly complete domination of the "contested terrain" of political theory by theorists who pay no serious attention to what feminists see as thoroughly persuasive arguments. Clearly, the structures of gender fundamentally affect political and personal life and the societies in which such life occurs. When this is acknowledged, it profoundly alters the estimates that can be made of the adequacy of liberal political theory and communitarian social theory. Yet both the liberalism dominant in North America and its communitarian critics have done almost nothing beyond changing a few pronouns to take account of "the gendered reality behind the concepts of political theory" (p. 37).

Can anyone doubt that gender is a component of our identities, and that we are at least partly shaped by the structures of gender expectations and roles that underlie or frame or are woven into the social worlds we inhabit, whatever the metaphors we use to express this? But then the abstract, isolated individuals of liberal theory, and the view that political activity consists of the indi-

vidual actions of separate entities, are conceptually deficient. And the traditional liberal distinction between public and private can be seen to be in need of radical revision.

On the other hand, can anyone doubt that women can resist the practices that structure gender in the communities they inhabit, or that women can construct alternative discourses in which to understand sexual harassment, or pornography, or the way the personal is political? And that women can sometimes succeed in making these issues political in the liberal sense? But then the view of the person and of agency offered by communitarian alternatives to liberalism are conceptually unsatisfactory. A view of social transformation employing political action that goes beyond the practices of any given community is needed.

In this discerning and important book, Frazer and Lacey are as interested in furthering the discussion taking place among feminists as in providing additional reasons to take feminism seriously for those who prefer not to. The authors suggest how feminist social and political theory can use the best of liberalism and the best of communitarianism in the development of our own theories. They understand that political theory without a theory of society and of persons cannot be satisfactory. They persuasively focus on the need to accept feminist and communitarian critiques of liberalism while resisting the communitarian tendency to see

persons as determined by their social circumstances.

Against the liberalism of Rawls, Dworkin, Kymlicka, and Raz, they point out that "we experience ourselves as embodied and socially-situated being with multiple ties and commitments. Human life outside the context of human society, interaction and interdependence is evidently impossible. Therefore, the notion of the disembodied or pre-social individual is seen as an eminently unsuitable starting place for political theory, as is transcendence of our social being as a first political or ethical ideal" (p. 57). And they interpret liberal conceptions as historically and culturally specific rather than universal.

On the other hand, they counsel avoiding any alternative that would see the embodied and socially situated persons that political theory should start with as mere victims of circumstance, confined by their places in history and the traditions of their communities. Assessing the work of MacIntyre, Sandel, Taylor, and Unger, they worry that "the lack of critical foothold in communitarian analysis will tend to the rationalization of the status quo, whatever it happens to be" (p. 141).

Frazer and Lacey also argue that any satisfactory political theory must have as a central component a theory of power, and that this must enable us to understand the power of social structures, of discourses such as those illuminated by Foucault, and of groups, as well as of traditional liberal exercises of power by one individual over another. They fault both liberalism and communitarianism for lacking adequate theories of power. Our conceptions of power, they argue, must also include the power to empower, as in feminist practices of mothering and teaching, and, the authors argue, feminists should not shun power but use it appropriately to overcome domination of all kinds.

This book provides very useful overviews of liberalism, communitarianism, structuralism, deconstruction, discourse theory, and such concepts as practice, social structure, power, and self. Frazer and Lacey discuss knowledgeably various metaphysical and epistemologi-

cal issues concerning, for instance, the reality of social structures, the status of groups, and the shortcomings of empiricism.

In a final chapter outlining their own recommendations, they put forward a "post-liberal, post-structuralist conception of political theory" (p. 190). It focuses on oppression and is committed to a political practice which gives "a central place to dialogue and the ongoing democratic involvement of members of the political community" (pp. 192-193). This approach has been called "dialogic communitarianism." Appreciative especially of the work of Seyla Benhabib, Drucilla Cornell, Nancy Fraser, Michael Freeden, Michael Walzer, and Iris Marion Young, Frazer and Lacey see power as pervasive in structures and practices, and the political as extending far beyond the state. Though they reject Richard Rorty's "radical post-modern pragmatism," they advocate embracing a diversity of interpretive, meaning-generating communities within broader societies; rather than fearing the fragmentations of postmodern life, they see in them increased possibilities for recreating communities and overcoming oppression. Arguing for a view of the self as relational, they stress the potential for such selves to engage in critical reflection and to transform practices. An important part of this involves culture: "in a real sense, to change discur-sive practice is to change the world" (p. 210). For instance, when unwanted sexual attentions are named sexual harassment and seen as part of a structure that disempowers women rather than as individual foibles, "they are no longer the same . . . The world is now one in which a practice which was formerly taken as a matter of uncontroversial entitlement has been constructed as problematic . . . What ensues is, in effect, a battle for dominant cultural meaning" (pp. 210-211). Contrary to liberal approaches, legal restrictions and political reform are not necessarily the most effective ways to achieve change. But contrary to communitarian tendencies, practices are not simply given features of culture; "culture is not monolithic.

One of the weaker sections of this impressive book is the authors' discussion of feminist revaluations of traditional female roles. For instance, Frazer and Lacey are suspicious of the ways in which arguments for the importance of caring for children, play into the hands of traditionalists wanting women to be satisfied with such roles, and they discuss childcare as an appropriate concern of government. We can all agree that feminists must constantly guard against the misuse of our arguments, yet at the same time insist that we not have our thinking about restructuring society paralyzed by such fears. Many feminists want the traditional roles radically restructured, as they would be if men participated fully in them. But many also argue that only a revaluation of such practices will make possible the kinds of social transformation we should seek. The activity of childcare, for instance, should be valued as highly as, say, corporate management; the profession of teaching children should be seen to be of as much importance as professions with many times more influence at present. In a feminist society, various traditionally female practices would not only be transformed in themselves, but the relations between such practices would be radically restructured. Instead of being, as now, continually overshadowed by economic and legal and political institutions, concern for the best possible development of children would be at the center of feminist society and

would provide the point of view from which much of the rest of social life would be evaluated.

Frazer and Lacey tend to call any recommendations for improving social arrangements "utopian;" this seems a questionable characterization and a departure from their own perceptive understanding of culture and social change. Many proposals made by feminist theorists to reduce oppression call for what is urgently needed and practically possible.

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Hannah Arendt: Politics, History, and Citizenship. By Phillip Hansen. Stanford: Stanford University Press, 1993. 266p. \$39.50 cloth, \$14.95 paper.

Following Claude Lefort, Phillip Hansen reads Hannah Arendt as a theorist of the *political*. As Hansen conceives it, the political signifies the constituting power and spirit of collective life, rather than politics in a narrow institutional sense. The central concepts of Arendt's political ontology—freedom, action, and the public realm—provide critical standards to grasp the decline of "genuine" politics and suggest the outlines of political association built upon plurality and "sheer human togetherness." Hansen finds Arendt an ideal conversational companion in his effort to judge the contemporary prospects of emancipatory politics.

"False" politics ("forms of social life which deny solidarity and citizenship" [p. 89]) arise when genuine forms of political thinking and action are blocked. Human (ontological) needs, Hansen avers, grant potency to inauthentic politics because when "the possibilities for a genuine politics are checkmated, human action will find other outlets" (p. 10). Hansen provides an excellent illumination of false politics, and this is a noteworthy achievement given the relative neglect of these phenomena in much of the most novel, recent writing about Arendt. His treatment of the crisis in culture, organized political lying, violence, and totalitarianism are consistently instructive and thought-provoking. His exposition of Arendt's affirmative political vision is less satisfying. This may be attributed to Hansen's interpretive strategy. Because he is reluctant to "[push] forward with the development of Arendt's ideas" (p. 12), he leaves us with problems which her thinking crystallized. From another perspective, however, Hansen's exegetical skill makes his a sophisticated introductory text for those less familiar with Arendt's writings.

The book's title captures Hansen's insight that for Arendt "history and politics inhere in one another" (p. 48), which is to say, our conception and experience of history condition the character of citizenship. False politics thrive when we experience history as a "logical" or "lawful" process "which both compels and validates individual and collective activity" (p. 7). Loss of "conscious responsibility, moral sensibility and, in extreme cases, the ability to think at all" (p. 7), is the price of such "process-thinking." Genuine politics not only consciously direct social processes (Hansen explores this by reading Arendt in dialogue with recent theories of human need); it also interrupts processes to begin something new. Totalitarianism is a logical consequence and extreme example of false politics; the promising mo-

ments of modern revolutions exemplify genuine political forms.

Tracing Arendt's persistent if vexed relationship to Marx distinguishes Hansen's account and reveals one possible reason for the explosion of interest in Arendt: her writings are a treasure trove for radical critiques of late modern societies that abjure many of Marx's untenable historical and philosophical assumptions. As Arendt teaches, "Marxism emphatically sustains rather than challenges those forces in bourgeois society that tighten the stranglehold of necessity and undermine freedom" (p. 182). The antidote is a recovery of the political.

Hansen takes Arendt's task as his own: to reconceive what it means to think and act politically. He traces the connections in Arendt's work between public life and political thinking. Most notably, he interprets "political thinking as politics" rather than simply "thinking about politics" (p. 197). Thinking, he claims, entails coming home to oneself while political action concerns trying to be at home with others. According to Hansen, Arendtian politics has a distinctly personal character. At issue is the quality of our relationship with ourselves and others. Our relations are genuinely political when we are simply with others, neither for nor against them. Though political thinking and action are definitive of "what it is good to be" (p. 193), they are also daunting and difficult. They are demanding because our lives are "neither inevitably fated nor radically undetermined" (p. 4). Openness in the face of the contingencies of this human condition, resisting the desire for sovereign mastery over the world and others, is the political challenge. We share this world with others who are both distinct and like ourselves. This is why political thinking and action are estimable: They foster and reveal our unique individuality while creating the possibility of solidarity. Indeed, who we are cannot be disclosed without human togetherness. Hansen rightly regards preserving plurality as principle inspiring Arendt's thought-deeds. Whether endeavoring to feel at home with ourselves and in the world is the best (or Arendt's most suggestive) way of caring for plurality is another matter.

Can "genuine communal solidarity" exist without expunging individuality or violating others? Hansen answers affirmatively and invokes Arendt's public realm constituted by plurality. The commitment to plurality—as "identity in difference" (p. 49)—limits what you can reasonably do to others while at the same time securing a common world "where trust in what one's senses reveal is guaranteed" (p. 182). The skeptic might ask what is to prevent this feeling of being at home in the world from becoming a rote conformity to conventional morality which might serve evil as well as condemn it? Arendt's answer, Hansen maintains, is political thinking, especially political judgment which "involves both thinking in the stead of another and the capacity to determine 'right' and 'wrong'" (p. 209). Thinking fosters judgment, as Arendt interprets Socrates, by bringing us home to ourselves. Those who come home to the voice of conscience are more reticent to do wrong because they must live with themselves and, thus, wish to remain on friendly terms with their internal interlocutor. But if we conceive plurality as difference that precludes identity (and other aspects of Arendt's thinking suggest that we should, eg., the violations inherent in the activities of homo faber) then thinking politically may entail something more and different. Perhaps

Socrates resisted evil because he felt the otherness of his self. He never ceased to consider how his individual and Athenian home were predicated upon actual and potential harm to others even as (that much more so as) he showed himself to be Athens' most free and responsible citizen. Hansen's concluding reflections on Arendt's judgment of Heidegger and Jaspers suggest that he personally lives the dilemmas of plurality with the "dignity, decency and courage" (p. 198) Arendt esteems. But theoretically articulating the political conditions of doing so may require not only thinking with and against, but also beyond Arendt.

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The Economic Foundations of Government. By Randall G. Holcombe. New York: New York University Press, 1993. 273p. \$50.00.

This is an interesting, if flawed, book, another of the genre of books that look at government through the lens of microeconomic theory. (The author is an economics professor.) Partly, it has the feel of a brief text, presenting yet again a public choice perspective on big questions such as the origins and nature of government. It has, however, a number of original and intriguing ideas as well.

Although Holcombe's book covers a range of topics, the basic ground it covers is relatively straightforward. Government arises as a trade—"protection for tribute." People pay governments money in exchange for physical and economic security. Once governments come into existence, however, they are able to act like the monopolies they are—selling protective services at monopoly prices and, more importantly, producing goods other than protection and selling them at monopoly prices, "in a manner similar to the way in which monopolists might try tie-in sales, all-or-nothing sales, price discrimination, and so forth to enhance their profits" (p. 9).

Holcombe's account of government as a trade of protection for tribute represents a significant addition to the literature analyzing the Hobbesian model. One guestion that has been raised recently in this literature is why the strong would have any interest in participating in a social contract: Wouldn't they be better off stealing from the weak? The point of this criticism is to suggest that one cannot justify the development of government strictly from a contractual perspective of agreement among self-interested parties without referring to substantive ethical concepts (e.g., the immorality of theft) to justify government as arising to enforce a moral vision going beyond an agreement among the self-interested. Hobbes had an answer for the criticism that the strong would be better off without a social contract, suggesting that even the weak might kill the strong. Holcombe has a more convincing answer. He argues that without some agreement setting clear bounds on the "tribute" that the weak must pay the strong, the weak will have no incentive to produce anything (because all of it is subject to theft), and the strong will be worse off than if they agree to limit the tribute they take, since they will get a larger proportion of a much smaller pie. (As an aside, Holcombe notes that a weakness of the libertarian suggestion that "taxation is theft" is that taxation is predictable, while theft is not, so that the productivity-depressing consequences of taxation are fewer.) In Holcombe's social contract, disparity in initial bargaining power affects the results of the contract: the strong come out better than the weak. But both sides are better off, and the trade occurs. Thus, no appeal to morality is necessary to produce government.

One quick (and I hope not unfair) reaction to Holcombe's clever argument is that if it took a bunch of smart social scientists until 1994 to develop this argument about why the strong will realize that it is in their interest to tax instead of steal, it is questionable whether the prehistoric strong would have all been so smart. There are certainly elements of Holcombe's argument that have a ring of plausibility. Indeed, they bear some resemblance to the emergence of feudalism in medieval Europe. However, the feudal lords who bestowed protection in exchange for tribute were, so to speak, protection specialists who were protecting the weak against potential threats from many who apparently did not realize that the weak would be more productive if only no efforts were made to steal from them. That does not get us to a social contract agreed to by the strong as well as the weak. If one is to justify this arrangement, there is still no way of escaping a moral judgment about the behavior of potential thieves.

Holcombe's argument about government as monopolist is also intriguing but likewise flawed. Government's monopoly over protective services creates the opportunity for government to extract monopoly profits. Although Holcombe never states this explicitly, his view (consistent with the ideology of most public choice scholars) seems to be that essentially everything government does beside providing protection (everything from farm subsidies to antipoverty programs) involves government extraction of monopoly profits from citizens through a tie-in deal whereby they are forced to support such programs as a condition for receiving protection from theft. If government can exploit its monopoly position, those who hold power may use it either to adopt special-interest legislation or to vote their ideological convictions about good public policy without worrying about electoral defeat (actions Holcombe appears to regard as essentially equivalent from his analytic perspective).

Government's ability to act as a monopolist, Holcombe argues, is restrained in the same ways that monopoly power is restrained in the private economy—through competition among units vying to be our government. This may take the form of Tieboutian competition among local governments, based on individual mobility, for attracting and holding residents. Or it may take the form of democratic electoral competition in any one government, keeping in mind Harold Demsetz's insight that in the course of competition for the right to be a monopolist, monopoly profits will be competed away.

As a result of his view that competitive forces are crucial to reduce government monopoly profits, Holcombe emphasizes "barriers to entry" that make it more difficult for these competitive forces to operate. For example, Holcombe argues that barriers making it easier for incumbents to be reelected and increase government monopoly profits. He also presents the view that the oft-noted expansion of the role of government in connection with wars results from the decline in internation population mobility in connection with wars, which increases government monopoly power by short-circuiting the Tieboutian mechanism.

Holcombe's whole analogy of non-protection-related government programs to monopoly profits is, however, incomplete. Partly, there are some technical problems with the analogy, since the putative monopolist (elected officials) would seem to be interested in gaining a monopoly (i.e., assured reelection) for its own sake or for reasons not relating to extracting monopoly profits from citizens (e.g., simply enjoying the power to make decisions), rather than, as with a normal monopolist, for the sake of its instrumentality in extracting monopoly profits from customers. More importantly, in Holcombe's view, government expansion arises only from the activities of special interest groups or through the personal ideological agendas of elected officials. This ignores the fact that electoral competition is itself a major reason government has taken on new roles. The majority, Holcombe suggests (though he never specifically states), would never go along with a government that provided more than protection. Hence, any such government must reflect monopoly profits. Holcombe's bizarre suggestion that government expansion in the wake of world wars reflected reduced international mobility ignores not only such obvious alternative explanations as the effects of war on national debt, postwar military spending, and benefits for soldiers but also the impact of wars on the climate of ideas in the postwar eras.

Now, from a libertarian moral perspective (including James Buchanan's view that government policies should be required to elicit unanimous support), the fact that government has taken on these roles may be morally unacceptable. Holcombe, however, seems to want to use a majoritarian framework to make what is probably in fact a libertarian, minority-rights point about the expansion of government. That will not work.

There is a final observation about this kind of economic analysis applied to government. On the one hand, the use of economic models can provide valuable insights and new ways of thinking (e.g., seeing incumbency advantages as analogous to economic barriers to entry that increase the ability to exact monopoly profits). On the other hand, the danger of reductionism is that when one phenomenon (politics) is reduced to another (economics), important features of the phenomenon being reduced can get lost. The basic metaphor of government as a marketlike arena for self-interested exchanges, where normative considerations play no role, is an example of the dangers of reductionism. Political scientists must resist efforts to reduce politics to economics.

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Paradigms and Barriers: How Habits of Mind Govern Scientific Beliefs. By Howard Margolis. Chicago: University of Chicago Press, 1993. 267p. \$40.00 cloth, \$15.95 paper.

Margolis presents Paradigms and Barriers as the second of three books on "persuasion and belief in the context of social choice" (p. xi). Scholars of politics are likely to find most intriguing the first and last quarters of the book, where Margolis explains how "habits of mind" help, hinder, and undergird reasoning across several sciences. (A middle chapter on "The Emergence of

Probability" addresses a case of interest to political scientists; but the other chapters in the middle relate stories from early chemistry, astronomy, and physics.) Like the prequel, this book speaks to political scientists on four main levels.

Students of political cognition might find useful several features in the Margolis account of all cognition as recognition of patterns. An obvious limitation of this as an elemental treatment is the circularity in parsing cognition as re-cognition-as Margolis more or less admits. Nonetheless the Margolis approach can spur us to inventory recurrent patterns of perception as figures of conception that typify styles of thinking-whether in politics or elsewhere. Then we can examine their ties to one another and their environments. This is what Margolis does when specifying the "habits of mind" that sometimes aid and other times impede scientific inquiries. When persuasion is at issue, as it often is in politics, these habits of mind are the figures (or tropes) of experience and reasoning that inform elite references, public opinions, political myths, enemy interpretations, and media reports. The Margolis "habits of mind" are thus akin to the social conventions noted by Ludwig Wittgenstein, Stanley Cavell, and social-constructionist students of science. And cognitive scientists who trace strongly valenced paths of spreading activation in associative networks can plow the same ground, though in very different ways and with very different results. Margolis takes himself to sidestep specifically political patterns for most of the book at hand, yet that premise would hold only on a narrowly liberal notion of politics as activity related directly to the state.

Students of political choice, accordingly, can look to the book's case studies for reconstructions of rational choice in some of its more institutionalized and rigorous settings. The situational detail central to the Margolis analysis would be a wonderful antidote to the increasing abstraction of rational-choice work on politics. Where the retreat to formal theorems threatens to leave positive political theory as bereft of political substances as mathematical economics have become empty of matters economical, the historical ambitions of the Margolis approach can prove highly valuable. At times Margolis settles for the analytical philosopher's style of spinning a brief but plausible tale, without bothering to think through plausible counterarguments or stories from other perspectives. But these are lapses of execution, not method.

Students of political epistemology and methodology can gain from Margolis a sense of how social-constructionism can help us to understand political inquiries and practice them better. Margolis takes pains to rebut the arguments for epistemic relativism often advanced by social-constructionists. Indeed, he, like his targets, gives so much space to this old agenda of philosophical puzzles that the book degenerates on occasion into the idle kinds of dispute disparaged by cognitive science. Even so, the Margolis stories of science imply innumerable corrections to the objectivist canons of some singular Rationality or Method for studying politics. Showing habits of mind to differentiate paradigms and disciplines, Margolis quietly dispels idols of unified science. How much the author intends these lessons, I am not sure, but the book offers them readily to anyone who reads its case studies carefully.

Students of political persuasion can turn to the Margolis book for case studies and more. Margolis provides an anti-inflammatory sense of how politics, culture, and science entangle both constructively and not. He emphasizes how "to a striking extent what pass for good arguments are the same everywhere" (p. 199), and he downplays that importance of moves in rhetoric by saying that any decent rhetorician in science or politics will display "a mixed rhetorical style" that uses many devices (p. 197). Nevertheless his case studies often specify and always reveal intricate strategies and dynamics of persuasion of great interest in political terms. Margolis resists regarding persuasion as the politics of talk or reasoning, but his book helps us appreciate the politics amply involved in habits of mind important for episodes from the early modern sciences.

Gracefully written, the arguments of *Paradigms and Barriers* depend on a style of rational reconstruction and storytelling long common among philosophers of science. Margolis seems to acknowledge that his own rhetorics differ markedly from those now prominent in cognitive science or in accounts of rational choice. They have shortcomings, to be sure; still they comprise a voice that should be heard more in various studies of politics.

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JOHN S. NELSON

Explaining Political Disagreement. By Andrew Mason. New York: Cambridge University Press, 1993. 170p. \$44.95.

One of the central purposes generally ascribed to the liberal state, in both its minimal and robust incarnations, is dispute management. The metaphor of the forum expresses both the ambitions and limitations of this management. Persons introduce rival claims into a fixed space where they are to be adjudicated by public reason. The background conditions that produce the disputes are not addressed by the forum. The successes and failures of liberal dispute management famously occur within the parameters of deliberation among reasonable persons.

Andrew Mason provides a set of arguments, closely tied to relevant literatures, that attempt to explain political disagreements. Unfortunately the criteria for good or even adequate explanations are never specified in the work. One consequence is that it is never clear whether Mason is explaining the occurrence or persistence of disagreements. But this flaw aside, the work does advance our understandings of disagreements that might lead to political disputes. Mason even appropriates the signature mark of the liberal forum in distinguishing the reasoned approach of liberal governance from nonrational considerations. It is a mark of Mason's acumen that he attempts to combine these two species of explanation in a single model. The ultimate inadequacies of this synthesis do not subtract from the merit of the work.

Mason recognizes a distinction between two conceptions of disagreement: imperfection, in which at least one of the parties to a dispute is wrong, and contestability, where the proper use of political terms allows for a variety of reasonable interpretations of them. The first conception explains disagreements, and presumably resolves them, with a theory of error that accounts for mistakes. The second relies on the open-ended qualities of language for explanations.

Mason develops a hybrid argument that combines these two conceptions. He accepts the idea of moral truth and error, though he denies that moral or political disputes are resolvable through the correction of error. His version of moral cognitivism relies on norms of inquiry (found in both science and morality) rather than traditional moral realism. This Quinean approach allows him to accept essentially contested concepts and the persistence of reasonable disagreements, while allowing that some interpretations may be superior to others. Mason's view is a variant on the familiar coherence theory that is currently regarded with so much good will in political and legal philosophy.

At the end Mason provides a materialist explanation for a variety of disagreements that integrates the gender differences introduced by Gilligan with the psychoanalytic theory of gender construction developed by Chodorow. This model relies on the sexual division of labour to explain some political disagreements. Mason also uses the model to account for the attraction of Nozick's story of how persons acquire self-esteem.

No outline of the arguments can capture the intelligence of so many discussions in the book. Mason offers insights on the failures of explaining disagreements in terms of incompatible value premises, the difficulties in using distinctions between a concept and a conception, the desirability of regarding moral justification in terms of diverse forms rather than a single model (thus rejecting Hare's approach), the powers of analogical reasoning in moral discourse, the distinctions between ambiguous and essentially contested terms, the unintelligibility of the assertion that paradigms can be both incommensurate and contradictory (an assertion usually attributed to Kuhn), and much more. The reader will relish these sections even when dissenting from key observations and conclusions and wishing for sharper distinctions between disagreements and disputes.

Any reservations about this book are inspired by the intelligence of the contributions. My own thoughts are that the author fails in his stated goal to construct an empirical explanation that integrates rational (reasons for judgments) and non-rational (e.g., psychological propensities and social structures) considerations. It is true, as Mason points out, that "explanations which are capable of operating at each of these levels are potentially very powerful" (p. 15), but his ending theory does not achieve this power. The rational account of disagreement is followed in the text by the materialist theory. No model integrates the two.

The absence of an integrated theory may itself be explained by difficulties in separating rational and nonrational considerations rather than by Mason's failure to seize an opportunity. We might distinguish between the sources of disputes and their prolongation and even intractability. Persons can have conflicting preferences, interests, ideologies, and understandings of reality, any of which can lead to disputes that reach the political domain. The causes of social conflicts are multiple. The two material considerations cited by Mason-social structures and psychological factors—have received primary attention in social theory. But another material consideration must be discourse itself. Ideas, and their expressions, have an inertia that affects perceptions and understandings of experience over historical time, and perhaps structures realities. Discourses can conflict with one another, leading again to political disputes. Mason refuses to consider the causal relations between reasons

and beliefs. It may be that rational considerations are also nonrational explanations, an integration unfortunately dependent on reductionism.

Why disagreements persist is not the same as why they occur. Material explanations for disputes are causal, and may be generalizable to some degree across political societies. But the prolongation of disputes, their tractable or intractable natures, may be tied to regimes and seem eminently contextual. An authoritarian state may admit no intractable disputes, resolving all political differences with effective force. A theistic state, governed by truth, may regard all disputes as occasioned by error on the part of some or all of the disputants. Corrective mechanisms follow the imposition of a theory of truth. In liberal regimes, however, simple force and dominating truth are not available options for the state in managing disputes. A language of public reason must prevail. The contestable view is that this language admits multiple reasonable interpretations, which leads to the intractability of political disputes. Contestability may not be a product simply of language, however, but of a political setting that resists closure. The contestability of language may be a function of its political contexts, at least in the sense that regimes can activate or suppress the elastic possibilities of terms. Is the relationship between language (on this view) and political disagreement a rational or a materialist explanation? The fact is that we do not have an adequate account of material and non-material considerations that will allow us to ponder how to integrate them. The liberal reluctance to explore background variables in adjudicating disputes may be based on a separation between the rational and the non-rational that needs to be abandoned rather than closed with an integrated model.

But these are the kinds of speculations that Mason's book inspires. The arguments developed by Mason are important contributions to an ongoing conversation about political disagreement. Adding to the agenda of distinctions and criteria will be a normal reaction from readers forced to think about these problems at a different level. No higher tribute can be paid to a book.

Syracuse University

FRED M. FROHOCK

Tyranny and Political Culture in Ancient Greece. By James F. McGlew. Ithaca: Cornell University Press, 1993. 234p. \$31.50.

Athens on Trial: The Antidemocratic Tradition in Western Thought. By Jennifer Tolbert Roberts. Princeton: Princeton University Press, 1994. 391p. \$29.95.

Both books under review here explore the transformation of a particular historical political regime into an ideological myth. James F. McGlew, in the far more focused work, studies the place of the tyrannies that flourished during the Archaic Greece of the sixth century BC in the language and emerging self-conceptions of the democratic city-states (or poleis) of the fifth and fourth centuries. Jennifer Tolbert Roberts, in a much more wide-ranging (and much longer) work studies the place of Athens as a democratic regime in the thought of a wide assortment of authors across the last two and a half millennia, up to and including the latest conferences and forthcoming books in twentieth century America.

Both authors confront a difficult historical challenge: They take what is in the first place a not fully understood

political system and explore how others have used their own readings of that system to express views about their own political worlds and to influence the actors in those worlds. McGlew has the somewhat harder task in that he tries to study how the "city constructs and remembers its own political identity" (p. 10) from the stories and images of tyrants and founders; or, as he phrases it later in the volume: "The polis defined itself as a subject as it remade the world into its objective field" (p. 150). McGlew does not address the issue of agency, how it is that poleis or cities can act and, particularly, how they can re-write their own narratives. While his claims along these lines are somewhat discomforting, McGlew does force us to consider the interesting theoretical problem of the relationship between tyranny and democracy. We may not exactly learn how "images of sovereignty were the great political legacy of tyranny" (p. 9), but we are made aware that the tyrant—too often dismissed as a ruthless aberration in a political world that talks rather of authoritarianism and totalitarianism-should hold an important place in our conceptual framework as we try to understand the democratic regime.

Roberts has a somewhat easier time for she does not make causal claims about cities as actors and self-narrators; she instead writes a detailed intellectual history which recounts how Athens was perceived and interpreted in different ages and by different cultures. The accuracy or inaccuracy of the interpretations and the institutional effects of such readings is of less interest than the multitude of reactions to Athenian democracy that she finds in the seemingly myriad texts about which she writes. The texts she discusses reveal far more about the times in which they were written and what it was about democracy that found or did not find favor than they do about the practice of democracy, ancient or modern.

In Book Eight of the Republic Socrates offers a schematized portrait of the transition from democracy to tyranny, claiming that "it seems likely that from no other political regime than democracy is tyranny established" (564a). It is the ultimate freedom of democracy that leads to the slavery of the tyrannical regime in the a-historical analysis presented in Cephalus's house. McGlew, in contrast to Socrates, tries to analyze the transition between regimes historically, i.e., how tyranny gave birth to democracy, and show how some of the fundamental aspects of tyranny were translated into the life of the democratic city. In almost Hegelian fashion, we see how the democracies of the fifth century incorporate elements of the sixth century tyrannies that preceded them, especially in terms of freedom (eleutheria) and the autonomy of law. A chapter devoted to Solon explores Solon's efforts to remove the individual from the enforcement of justice, leaving the laws with their own power to provide order and thereby bring justice into the political community (p. 110). That Solon's attempt to establish the autonomous law failed, opening the way for the tyranny of Peisistratus, is for McGlew the result of Solon's reliance on "history" rather than "memory," or because "Solon's story was a product of his self-representation, not of popular legend" (p. 121). Nevertheless, Solon's story in McGlew's account suggests that one must consider the inherent relationship between tyranny and the rule of law, not as opposites, but as expressions of autonomy.

Autonomy emerges as well in McGlew's analysis of citizenship, perhaps more effectively than in the relationship between law and tyranny, for he draws together the freedom of the tyrant and the citizen: "The tyrant's power passed to his city not as a political abstraction or formula but as an individual possession" (p. 187). In a rather idiosyncratic reading of Pericles' funeral oration, McGlew tries to show the Athenian citizen as individual tyrant such that *erastes* from that speech, the lovers of Athens whom Pericles urges his listeners to become, expresses the "passion of the aspiring tyrant, not the loyalty of the honest citizen" (p. 188). This reading may raise questions, but it does alert us to the possibility of thinking about the tyrannical drives of the democratic citizen.

While McGlew tries to identify how the somewhat amorphous "memories" of tyranny and foundations become part of the political structures of succeeding centuries, Roberts traces more directly the "spectrum of responses to classical Athens" (p. 96). Vividly and deftly, she follows those responses through the millennia, blaming Plutarch most strongly for establishing the anti-democratic tradition that marred Athens's reputation. Though the hostile attitudes to Athens paralleled the anti-democratic attitudes of the numerous authors discussed before the eighteenth century, Roberts points out that it was not always political views that determined the response to Athens. In particular, the German author Wilhelm Winckelmann longs for the aesthetics of the Athenians; the resurrection of Athenian democracy in the eyes of this writer and other eighteenth century German authors occurs not from interest in the political regime that gives autonomy and freedom to the individual, but from the culture that Athenian democracy could produce. Similarly, in England in the nineteenth century, enchantment with the civilization that the Athenian regime nurtured justified its praise. And then-as Roberts nicely points out-the unity of democracy and empire could serve, by analogy, the contemporary political interests of a colonizing Britain. In a similarly intriguing way, Roberts shows how democratic Athens became part of the debate between abolitionists and the defenders of slavery prior to the American Civil War.

Roberts offers an enormous amount of information and discusses a hefty number of authors, often more than one per page, especially as we get to the last decades of the twentieth century. Through it all, Roberts reminds her readers that just as her subjects react subjectively to the Athens they are portraying, she too reads their works subjectively leaving us free to reread and reinterpret the works she discusses. As the book nears its end, it takes on more and more the feel of an extensive review essay where all articles touching on Athens in the last decade must be included; this appears to be part of her attempt to assert her own subjective place within the tradition of scholars writing about Athens, rather than present herself as an objective scholar assessing the quality and significance of what she discusses.

Neither book is particularly satisfying in the readings offered of ancient texts. Roberts, eager to show the antidemocratic tradition emerging from Athens, itself offers somewhat glib restatements of Thucydides (e.g., she summarizes Pericles' funeral oration in nine points), Plato, and Aristotle. That the Republic makes clear that only in a democracy could a Socrates emerge, that the Meno points to the reasoning abilities of a slave, that Socrates's supposed disdain for the many does not mean

his refusal to try to educate all who will talk with him, all these and many more points are ignored in making Plato a classic (and classist) antidemocrat. That Aristotle's definition of a citizen is one possible only for a democracy is likewise ignored, as are his arguments concerning the superiority of collective wisdom in things political; the ambiguity surrounding Aristotle's discussion of the hierarchy of the master-slave relationship is occasionally recounted, but its significance is not explored. McGlew concludes his book with brief readings of the Oresteia, Oedipus, and the Republic to suggest how "Athens made tyranny safe for democracy" (p. 190), but the brevity of the analyses allows only for a pointing to conjunctions of terms associated with tyranny rather than a full exploration of the theoretical significance of those conjunctions.

To ask for more careful and sustained readings is perhaps unfair, since the two authors do not necessarily build their books around these interpretations; rather, they move us to reflect on how historical political institutions take on conceptual lives of their own across the centuries and how we as scholars may most appropriately study and assess the significance of these conceptual lives.

University of Michigan

ARLENE W. SAXONHOUSE

Against the Market: Political Economy, Market Socialism and the Marxist Critique. By David McNally. New York: Verso, 1993. 262p. \$17.95 paper.

Against the Market is an ambitious defense of an unpopular position. The crippling defects of Stalinist command economies have led many socialists to advocate versions of (democratic) market socialism, combining collective ownership with commodity production. McNally argues that this is an unwarranted concession to the false view that "society cannot function except through alienated institutions which evade popular control" (p. 3). Worse, it is a contradiction in terms. The choice is socialism or the market (p. 2). A challenge to the current hegemony of promarket ideas can only be healthy; but while Against the Market deserves a wide readership among those interested in the history of political economy, it does not make the case McNally professes.

He proceeds by way of dazzling survey of the history of markets and theories of their operation, both conservative and radical, mainly in England in the century from Smith through Marx. His erudition is astonishing, and he has an enviable gift for concise exposition of complex ideas and an illuminating way of locating them in their political context. His chapter on Smith as a moral philosopher as well as a political economist helps recover that thinker from his reputation as a prophet of greed. Likewise fine are his discussions of lesser-known radical thinkers, many of them working-class activists, who attached the mean Malthusian dogmas of laissez faire and its cruel operation, in part with Smithian tools that they turned to increasingly socialist purposes. His treatments of Owen, Proudhon, and their market socialist disciples should be read by anyone interested in the intellectual background of Marx's economics. Similar consideration of French socialism (Saint-Simon, Fourier) would have been nice.

In discussing Marx's critique of market socialism,

however, McNally tends to paraphrase, rather than analyze. He criticizes Diane Elson's account of commodity fetishism (p. 214), but his own discussion of this difficult idea merely quotes and restates Marx's Delphic deliverances (p. 162). He endorses Marx's labor theory of value as if it were unproblematic; but it is generally dismissed by economists, including many Marxists. McNally faults the earlier historical figures for not attaining Marx's insights without showing that these are defensible. Owen and others may have erred in regarding exploitation as due to monopoly advantages—something arising in exchange—rather than, as Marx thought, in production. But even if Marx was right (as I agree he was), this cannot be assumed.

The book's main virtues as socially informed intellectual history are a clue to its weakness with regard to McNally's main thesis. He expounds the market debate up to Marx's time very well and explains clearly why Marx rejected the market socialisms of his day. But even if Marx's objections are good, it is not clear why we should reject those of our day. First, as McNally shows, Marx's main direct criticisms of Proudhon and the Owenists turn on a special feature of their views—that remuneration should be in proportion to labor contribution and exchange should be in terms of equivalent labor values. Marx argued, plausibly, that this could not hold in a competitive market. But contemporary market socialists (e.g., John Roemer, David Schweickart) do not favor this feature. McNally does not engage with the details any of these new market socialisms to argue that their specific features would be, as he claims, tantamount to capitalism.

Instead he argues, boldly but implausibly, that the real evils of capitalism (exploitation, alienated labor, and crises) are due to markets alone and not at all to private property or wage labor as usually understood. He rightly places the labor market at the center of the socialist project, arguing that the commodification of labor power is the main problem with capitalism (p. 5). But he maintains that "wage labor" can exist even if productive assets are collectively owed and there are no capitalists. As long as "the fund for wages [is] determined by market prices for . . . commodities," self-managed worker cooperatives will act as "their own capitalist[s]" (p. 181), be compelled by blind market forces to accumulate endlessly, and in general reproduce the defects (for workers) of capitalist firms.

This is quite misleading. The market under consideration does not have exploitation in Marx's sense (i.e., wrongful expropriation of surplus by nonproducers), since all are workers. Nor need this market be subject to crises if, as in some models, investment is planned by the state, rather than dependent on the animal spirits of private investors and full employment is guaranteed by law. Finally, far from involving endless accumulation, many economists think a main problem for self-managed market socialism is its tendency towards very low growth because firms resist expansion when that means dividing up profits among more workers. McNally is right that even in market socialism, market forces rule workers' lives in a way not subject to democratic control. That is a serious objection. But it is not tantamount to capitalism or to wage labor.

If "the anarchy of the market" and its lack of democratic accountability is the problem, what is the alternative? Any challenge to markets must specify in plausible detail how a nonmarket economy can avoid the catastrophic defects of Stalinist planning and go "beyond alienated labor, exploitation, competition, and crisis" (p. 223). The lack of such an articulated model is the second main problem with McNally's approach.

The central question is whether planning can offer the kind of control over our lives that markets deny. This, rather than efficiency, is the real bite to the Mises-Hayek "calculation problem," at least for socialists. In a modern economy, the amount of information about consumer preferences, producer capabilities, and alternative ways of matching these up is too vast for anyone to grasp or manipulate. If, per impossible, the information could be aggregated, imposing coordination on production units would mean that the plan would rule worker's lives. A democratic vote-or even democratic participation in planning-would translate into very little effective control. If the information is disaggregated and most decisions are left to production units, as McNally advocates, is the planning any more extensive than in market socialism or even modern capitalism? (He acknowledges that his approach is close to that of market socialists like Elson [p. 213].)

McNally claims that Mises and Hayek are wrong that markets are much better at processing information (p. 198). This is not to the point if plans do not enhance democratic control over our lives more than markets can. The case against markets requires that plans be better. In 30 quick pages, McNally sketches a "political economy of the working class" (pp. 184–212). But to vindicate Marx's communism, McNally would need a systematic treatment of the economics of planning, not an intellectual and social history of market socialism in the eighteenth and nineteenth century. Perhaps it is unfair to criticize an author for not writing a different book. To have made his case, that is what McNally would have to have done; but his history is exemplary and will be a lasting contribution.

University of Michigan Law School JUSTIN SCHWARTZ

Kant's Theory of Justice. By Allen D. Rosen. Ithaca: Cornell University Press, 1993, 237p. \$28.50.

This is a clear minded and careful study of Kantian principles of justice. Beginning with the relation between justice and freedom, the author spells out, in Chapters One and Two, both the peculiar character of external freedom for Kant and the essential arguments by which (in the author's view) such freedom for Kant is justified. Chapter Three maps a variety of distinctions in Kant's ethical thought (e.g., between perfect and imperfect duties, and between those of wide and narrow scope); the result is a helpfully precise delimitation of the boundaries that both relate and separate justice and ethics for Kant (an area that has proved notoriously troublesome to scholars.) Chapters Four and Five turn to two fields of application especially contentions both in the scholarship and beyond: revolution and social welfare. The book concludes with a brief but judicious weighing of the strengths and weaknesses of Kant's contribution to liberal thought, and makes a forceful case for the superiority of Kant's categorical grounding of rights, over against the relativism and skepticism of other recent champions of liberal freedom. Throughout, Rosen's argument is enriched by examples from legal theory and case law (the dust jacket announces that Rosen is a practicing attorney), examples that fruitfully extend the author's otherwise pertinent but narrow range of scholarly reference.

Readers, especially those who are not professional Kant scholars, will find particularly interesting Rosen's reconstruction of Kant's moral grounding of justice, and Rosen's effort to save Kant from some of the less attractive doctrines with which he has historically been associated: to wit, his denial of the right to revolution and his alleged endorsement of the neutral or "night watchman" state which has figured prominently in recent discussion within the discipline of political science and elsewhere. His treatment of the categorical imperative, which lays special weight on the relation between the form and content of the law, presents Kant's case with a precision lacking in many more elaborate discussions. By the same token, however, Rosen chooses to ignore the many difficulties and objections that figure prominently in the literature-difficulties beyond the well-known charge of empty formalism that Rosen ably dispatches. Moreover, Rosen's overriding emphasis on the technicalities of Kant's argument leaves little room for a broader consideration of the significance, and, arguably, the limits, of a Kantian orientation to questions of morality and justice over and against competing orientations. Why Kant's appeal to categorical demands is preferable, say, to the approach of Aristotle is never indicated. Nevertheless, readers who wish to have Kant's categorical grounding of justice laid out for them would be well advised to read Rosen's first two chapters with care. Rosen also spells out very clearly the nature of republicanism for Kant, with an especially helpful discussion of the relation between political and civic freedom.

Rosen's treatment of Kant on revolution is crucial to the author's general project of sympathetic reconstruction. For surely no doctrine is less palatable to modern ears than Kant's insistence that revolution, though in some extreme circumstances excusable, is never justified. Rosen agrees with Kant that active revolution (as distinguished from the passive disobedience that Kant under certain conditions allows) can never be *legally* justified. To rescue the right to rebel against unjust governments Rosen expands Kant's theory of justice beyond the boundaries of an "ideal theory of law" to

which Kant himself, according to the author, limits it. The resulting sacrifice of textual consistency yields a moral, though not strictly legal, right of rebellion—a move with evident attractions, however they may ultimately conflict with other aspects of Kant's enterprise.

The author's discussion of Kant on social welfare has more immediate practical weight, especially given the recent influence of Rawls. Rosen takes as his point of departure certain textual ambiguities that bear on the state's authority to aid the poor. Rosen convincingly shows that, first impressions aside, Kant is not the early advocate of pure laissez-faire that he is sometimes taken to be, in as much as the state has for Kant cultural and moral, as well narrowly juridical, missions. This reader was less convinced by the author's attempt to deduce a political duty of benevolence towards the poor, especially given the difficulty, on his account, of arriving at a principled demarcation of the limits to such benevolence. Rosen here follows a long line of thinkers, beginning with Fichte, who have attempted to blunt the rigor of Kant's distinction between formal and material equality. While it is clear that the Kantian state is authorized (Kant doesn't say "required") to provide with the bare necessities of life those who would otherwise perish, it is less obvious how Rosen's more expansive reading can be reconciled with the basic principle of justice, which calls for the formal maximization of individual freedom. Whatever conclusions they ultimately draw, readers should find Rosen's discussions of these matters exceptionally stimulating.

One wonders, finally, if the author does not take an unduly narrow view of the significance of Kant's views of justice for his thought as a whole. Telling in this regard is Rosen's deliberate disregard of international law as unnecessary to his purpose; for Kant's cosmopolitan idea is arguably (as Rosen partially acknowledges in his treatment of the historical mission of the state) the driving aim of Kant's philosophic project. Still, this is a consistently illuminating and, in terms of its own aims, successful study of a topic long neglected, especially in the English-speaking literature. For the care and intelligence with which he addresses it, the author is owed a special debt of gratitude.

Boston College

SUSAN MELD SHELL

AMERICAN POLITICS

Advertising and a Democratic Press. By C. Edwin Baker. Princeton: Princeton University Press, 1994. 203p.

Air Wars: Television Advertising in Election Campaigns, 1952-1992. By Darrell M. West. Washington: CQ Press, 1993. 223p. \$18.95 paper.

Since the advent of televised campaign advertising, the "marketing" of political candidates has been compared to the marketing of consumer products. As Darrell West reminds us, however, "In reality . . . political commercials have little in common with product ads" (p. 17). The former are more novel events, are more likely to be recalled by viewers, have a more limited time span with a fixed endpoint within which to operate, and are designed to capture a plurality (in primaries) or a majority (in most general elections) of the "market share." And, as these two books ably demonstrate, the political consequences of product and candidate advertising are different as well.

In Advertising and a Democratic Press, C. Edwin Baker argues that "advertising seriously distorts and diminishes the mass media's contribution to a free and democratic society" (p. ix). Baker's central concern is the impact of the media's financial dependence on advertising on the substance and distribution of their nonadvertising content. Beginning with a hypothetical (and innovative) economic analysis, he demonstrates that a shift from a "world without advertising to a world with advertising" (p. 13) could theoretically eliminate competitive newspapers, reduce diversity, inhibit the creation of new newspapers, lead to a decline in total newspaper readership, and reduce the total resources spent on editorial content. He then goes on to show (through an examination of aggregate historical trends and individual case studies) that as newspapers become dependent on advertising, the actual patterns of readership and competition closely match his hypothetical scenarios.

At the same time that advertising reduces competition and readership, it also, according to Baker, shapes the specific content of the media by encouraging news reports and editorials that are supportive of advertisers' interests, by creating a consumer mentality that makes readers more responsive to advertising, by reducing partisan or controversial reporting that might offend advertisers' potential customers, and by favoring the interests of middle-to-higher income audiences. Baker supports these conclusions with specific, if sometimes circumstantial, examples of both the reactions of advertisers to news content that violates these four tendencies, and of self-monitoring by news organizations themselves.

Having made his case for the deleterious effects of advertising, Baker turns to a proposal for remedying this state of affairs. Acknowledging both that advertising provides a number of benefits to consumers and news organizations, and that any attempt to ban advertising is unrealistic, he recommends a tax on advertising, the revenues from which would be used to subsidize circulation, thus making newspapers (and other media) somewhat less dependent on advertisers for revenue and somewhat more dependent on readers. This, he argues, would change the incentive structure of the media in ways that would increase competition, the diversity of content, and the number of readers and viewers. Baker highlights the benefits of his proposal and argues for its feasibility by comparing it to a number of alternative policies that employ either advertising taxes or audience subsidies. He concludes his book with an exploration of legal issues, arguing that little in current First Amendment doctrine would necessarily challenge the constitutionality of his proposal.

Readers familiar with the work of Ben Bagdikian will note a great similarity with Baker's argument, a debt he readily acknowledges. This similarity extends to occasionally drawing sweeping conclusions about the insidious effects of advertising from specific, sometimes only partially convincing, examples and cases. The real contribution of Baker's work is fourfold. First is the careful, systematic presentation of the underlying logic of Bagdikian's argument. Second is the use of economic analyses to show how an apparent benefit of advertising-the subsidizing of the cost of information to the public—can lead, ironically, to a less free and democratic press. Third is the variety of logical and empirical evidence he brings to bare in demonstrating these effects. And fourth is the specificity of his proposed solution to this problem. In the end, readers may not be convinced by his argument, evidence, or policy recommendation, but

they certainly will be stimulated by them.

While Baker explores the long-term, systemic, and indirect effects of product advertising on democratic politics, Darrell West focuses on the immediate, individual, and direct effects of campaign advertising. Drawing on a random sample of 150 "typical" presidential campaign spots aired between 1972 and 1992, and a purposive sample of 324 "prominent" spots aired between 1952 and 1992, he begins with a detailed analysis of the content of televised campaign advertisements. His findings both confirm and challenge conventional wisdom regarding this often criticized form of political communication. Among his more important conclusions are that campaign spots provide fewer mentions about public policy than prior research suggests; that only about half of these policy mentions provide "clear statements of past positions or expectations about future actions" (p. 36); that Democrats are more likely to emphasize domestic policy while Republicans are more likely to emphasize foreign policy in their television spots; and that attention to the personal qualities of candidates is somewhat more common in nominating than in general elections. West also uncovers several illuminating trends in the content of campaign spots over time. Most notably, he finds that since 1980 attention to specific policies has increased in prominent ads, while attention to candidates' personal qualities has decreased; that after 1956 specific appeals to party loyalty have been almost non-existent; and that negative advertising has generally increased over time (though it did decline in 1992). Within these general patterns, West also notes that the substance of political advertising varies in more election-specific and intuitively understandable ways; for example, mention of social welfare issues was most prominent in 1964, taxes in 1984, issues of war and peace in 1968, and so on.

Scholars such as Kathleen Hall Jamieson suggest that the news media have recently begun to cover political advertisements as campaign stories in their own right. West provides compelling evidence that this is indeed the case, with coverage of campaign ads by the New York Times, the Washington Post, and the CBS Evening News increasing substantially in the 1980s and 1990s. Especially revealing is his finding that two-thirds of the content of new stories about campaign advertisements are devoted to "horse race" issues (for example, how a particular spot fits into the candidate's larger electoral strategy). This emphasis on the strategic, as opposed to the substantive, import of campaign ads has been especially prominent since the early 1970s, though as West notes, in 1992 both the print and electronic media devoted considerable space to "Ad Watches"—stories which help citizens evaluate the accuracy and intended impact of specific campaign spots.

Having provided a thorough overview of the content of campaign commercials, West devotes the second half of his book to an examination of the impact of campaign spots on what voters learn about candidates, on what they see as the most important issues of the campaign, and on the standards they use for assessing the candidates. Using a variety of national and state public opinion surveys, he finds that citizens who were more exposed to political advertising (based on a four-point scale of reported attentiveness to political advertisements) were more likely to recognize the candidates, to rate them favorably, and to see them as potentially electable. Exposure to campaign advertising also affected voters' assessments of the personal traits and issue stands of candidates in ways consistent with the strategic appeals of the campaign ads themselves. Throughout his analyses, West controls for party identification, education, age, sex, race, ideology, political interest, and general media exposure, increasing one's confidence that the reported effects are due specifically to exposure to campaign advertising.

West also finds that campaign advertisements can be influential in setting the political agenda of voters and in establishing the particular issues used in assessing candidates, again controlling for possible confounding effects. Those who were most heavily exposed to ads often differed from those who were less heavily exposed in their views of the most important problem facing the nation and the most important event of the campaign. Significantly, these differences were consistent with the messages contained within the political advertisements themselves. For example, exposure to George Bush's 1988 "Revolving Door" spot (which criticized the Massachusetts prison furlough program that was in place during Michael Dukakis's tenure as Governor) increased the likelihood that voters believed crime was the most important problem facing the nation. Campaign ads were also effective in increasing voters' perceptions that a candidate was "doing well" in the campaign. Similarly, West finds that exposure to campaign ads can lead voters to emphasize certain issues in their evaluation of candidates (priming) while deemphasizing others (defusing). For example, in 1988 George Bush was able to defuse the potentially harmful issues of his record on the environment and his purported lack of compassion for people: Those most heavily exposed to campaign ads were least likely to report that these issues were important to their vote. And in 1992 the Clinton campaign appears to have successfully primed voters to focus on the nation's poor economy, while also increasing their perception that Clinton was best able to address this issue.

While making the general argument that campaign advertisements have a significant impact on political learning, agenda-setting, priming, and defusing, West is also careful to note (and show) that the effectiveness of a candidate's media campaign ultimately depends on the effectiveness of the opponents' ads, the timing of particular advertisements, the way in which the news media interprets the campaign, and the larger social and political context in which a particular election takes place. Effects can vary among groups of voters as wellfor example Bush's "Revolving Door" spot, which specifically mentions rape, was particularly effective at increasing concern about crime among women. In addition, the impact of campaign advertisements is generally stronger in campaigns or for candidates with lower initial visibility. In the end, West concludes that while "television could become an enlightening force in American elections" (p. 157), the current content of campaign advertisements, coupled with the tendency of both the news media and candidate organizations to think about ads in strategic rather than educational terms, has led to an information environment that is much less useful to voters than it might be. He also raises the concern that, absent appropriate watchdog institutions, campaign advertising can manipulate, rather than educate voters. His main solution to this problem is for journalists to take their oversight function more seriously, increasing and expanding the kinds of "Ad Watch" stories they turned to in 1992.

Air Wars is an important contribution to the literature on media and politics for several reasons. The content analysis of campaign commercials is the most complete and thorough I have seen, and should serve as a valuable resource for students of campaigns and elections (indeed, I wish West had devoted more of his book to this part of his research, and tied it more directly to his subsequent analyses of the impact of campaign ads). And while learning, agenda-setting, priming, and defusing have been well-documented by experimental studies that focus on the news media, West shows that these effects can also be found using the somewhat more natural design of voter surveys conducted during actual campaigns. In addition, he demonstrates that these effects can result specifically from exposure to campaign advertising. Finally, by examining a number of different elections, and by focusing on both general exposure to advertisements and exposure to specific ads, West is able to identify both the systematic and case specific impact of political advertising, and to point to the factors that are likely to make ads effective (though more attention to how campaign advertising affects different subgroups within the population would have been valuable). Of course, the fact that the impact of campaign advertising often varies from election to election could be seen as evidence of the relatively weak, idiosyncratic effects of such ads, rather than as evidence of the complex interrelations West often posits. Nonetheless, in the end the empirical evidence and reasonable assumptions presented in Air Wars makes a compelling case for the power (and limitations) of campaign advertising in American elections.

Barnard College, Columbia University MICHAEL X. DELLI CARPINI

The Constitution of Judicial Power. By Sotirios A. Barber. Baltimore: Johns Hopkins University Press, 1993. 279p. \$25.95.

In this work, Sotirios Barber sets forth a theory of judicial activism, in the tradition of the Warren Court, which he defends against "skillful enemies" on the right and "incompetent friends" on the left. Justices should make up their own minds as to what the Constitution means, giving no weight to the opinions of the framers, prior courts, legislators, current or past majorities. This practice, Barber maintains, is not the innovation of the Warren Court or the deviation of the Old Court, but in fact the "classical" theory of judicial review. In supporting this point, he closely analyzes the Federalist Papers, not exactly to reconstruct the arguments as Madison and Hamilton understood them, but to construct an interpretation that best combines plausibility with morality. In this way, Barber places the role of the courts in the context of The Federalist's aspiration to reconcile democracy with justice, not "justice" but real justice. To simplify, and thus to do a real injustice to a complex, imaginative, and well structured argument, the Constitution assumes a gap between justice and what the people believe about justice, between the people's aspirations for simple justice and their immediate and often self-centered interests, and it "institutionalizes a concern" for narrowing these gaps by creating structures of government that can be responsible to the peoples' aspirations exactly because they are not responsive to their immediate interests (pp. 23 and 31). Active use of judicial review in the name of simple justice is the peak institutionalization of this concern.

In dwelling on the arguments of the framers, of course, Barber wishes not to bolster a jurisprudence of original meaning, but to undercut it. The framers, by his analysis, wanted the Court to pursue real justice, not just somebody's opinion about it, including their own. Barred by his own methodology then from resting this case for judicial activism on the intent of the framers, Barber furthers it by drawing on the revitalization of moral realism in philosophy, especially in the work of Michael Moore, to defend the presuppositions underlying it: that the Constitution refers to a moral reality distinct from anyone's conception of it, that judges are capable of being guided by this reality rather than partisanship and self-interest, that the American people are united as one body from the founding in their aspiration to know and realize justice as the central element of moral reality (from which aspiration in fact derives their authority), that it is respectable to think that such moral reality exists to which the people aspire (even if we cannot know it with certainty), and that the Constitution actually embodies such commitment to simple justice.

With this classical case and its presuppositions, Barber defends judicial activism from skillful enemies and incompetent friends. He is most effective in exposing the incompetence of its friends, who in simple-minded or highly sophisticated ways end up defending judicial review as an instrument of popular willfulness. This is true even of those who share Barber's vocabulary of constitutional "aspirations," such as Michael Perry, or the important distinction between concepts and conceptions, such as Ronald Dworkin. In one way or another, because they refuse to take seriously the possibility of

real justice, these theorists end up rationalizing the conventional.

Barber is also concerned to identify the skill of judicial activism's enemies. That skill, he argues, lies not in the merits of their arguments. Indeed, the typical enemy, such as Robert Bork, cannot truly defend himself because his skepticism reduces all principles, which must include his own, to mere "preferences." Instead, their skill lies in appropriating as their own *The Federalist* Papers, John Marshall, Joseph Story, and James Bradley Thayer. As Barber attempts to show, not always persuasively in my mind, these thinkers support his classical theory of activism. The atypical enemy of judicial activism, whom Barber cannot so easily dismiss, is Walter Berns, who, like Barber, reaches to nature rather than convention for his argument. Yet in his neo-Hobbesian exposition of the Constitution, Berns cannot escape the paradox that plagued Hobbes as well-how to defend philosophically a regime that seeks to silence philosophic disputation as it liberates human acquisitiveness.

Barber is a platonist, his approach socratic. In masterly readings of commentator after commentator, fundamental premises are laid bare and shown to be at best paradoxical. Positivists are shown to require the normative philosophy they despise. Pragmatists are shown to presuppose the philosophic realism they would rather set aside. Especially in making the case for philosophy and, more particularly, moral realism, in constitutional interpretation, Barber is powerful. His book is a landmark achievement, one from which future arguments will have to take their bearings. That is not to say his argument is thoroughly sound—I, at least, find the step from moral realism to judicial activism a large non sequitur—but that it is plausible and thoroughly provocative. Whether one agrees or disagrees with Barber, one will find it hard to resist writing in the margins, or elsewhere, a companion volume of queries and comments. Here I will mention only a couple of concerns.

In emulating Socrates, Barber has set for himself an impossibly high standard, but since it is of his choosing, to measure him accordingly seems fitting, if not quite fair. In tone, his argument falls short of that steady detachment characteristic of the philosophic perspective. Towards the left, he in fact is calm and unstinting in his scholarly courtesy. Even as he points out the nihilistic implications of their thought, he acknowledges the possibility that they may be right. Towards the right, on the other hand, he is, well, excited. He repeatedly accuses those he calls "New Right" jurists (that is, the advocates of original meaning) of conducting a "McCarthyite" campaign against constitutional theory—also termed a "jihad" and a "crusade" (pp. xii, 119, 146, and 198; 128 and 149-50). Although he would not, of course, wish to call into question their underlying motives or raise concerns about the company they keep, he hints of something dark stirred into their attack on judicial activism: "Partly because academics tend to avoid open statements of political preference-few New Right scholars admit to racism, religious zealotry, greed, a scapegoating resentment of selected forms of welfare, and a desire for vengeance against selected classes of criminals—they make institutional arguments instead" (p. 1). One can almost hear the book announcing, "I have here, between my covers, the names of several originalists now teaching within the academy." The tone is especially unfortunate, for originalists, who have much to learn from his arguments about moral realism, will be able to listen only with heroic forbearance.

He also seems not to get the joke of Plato's Republic. Instead of seeing Socrates's constitution as an ironic impossibility, a perspective from which we might view the human comedy and endure its inevitable shortcomings with equanimity, Barber looks to it as an earnest model of government—our government, in fact, with the Court ruling, as Learned Hand once commented, as a bevy of Platonic guardians (with the qualification that we can at least impeach these guys [pp. 41-42, 115]). Do I exaggerate? Consider these points. First, the "heart" of the "real" constitution, is not the text; its essential dimension, i.e., what makes it a constitution, is its commitment to reason over passion and justice over democratic choice. Thus, paradoxically, "the sovereign people cannot create the Constitution [so understood]; the Constitution is rather a first step toward the people creating themselves as a sovereign" (pp. 65, 116, and 217). Second, in interpreting the defeasible text of the Constitution, in light of its essential commitment, justices should give that document the most attractive construction its language will bear (pp. 109-110). But third, when the language simply cannot be squared with the demands of justice under present circumstances, when in other words there is a conflict between its ends and its means, the Constitution becomes an incoherent measure of action. "Because an incoherent measure cannot measure, there is no possibility of constitutional action by any actor under conditions of constitutional incoherence. . . . [T]he Constitution has no prescriptive force" (pp. 207-08). Unbound by the Constitution except in its commitment to justice, the judge may then do whatever he or she thinks likely to restore a constitutionally governable state of affairs. As possible historical models of such action, Barber mentions school desegregation remedies, including the mandating of taxes for such operations.

Especially with the second and third points, it is hard to see how the Constitution could function as law rather than an authorization for the courts to lead us toward simple justice. Justices should look to constitutional provisions when they do not constrain their quest for justice, Barber seems to say, and ignore them when they do. That indeed is a Constitution of Judicial Power, but I had thought that ours is a constitution of law and of self-government.

Colgate University

STANLEY C. BRUBAKER

To Make a Nation: The Rediscovery of American Federalism. By Samuel H. Beer. Cambridge: Harvard University Press, 1993. 474p. \$29.95.

Samuel Beer has made many contributions to the study of politics in our time, but in this writer's opinion his most significant contributions have been his studies of federalism, to which, he says, he turned in the 1960s as a liberal concerned with the American federal union and fully supportive of its nationhood yet at the same time concerned that its states remain healthy, functioning parts of the American nation. In the years following, Beer addressed contemporary questions of federalism from his perspective, raising those issues for serious consideration on the American political science agenda.

Now Beer turns to a comprehensive study of the

emergence of American federalism and the federal system to which it gave birth as viewed from his liberal perspective. Nor should we have a doubt as to how liberal that perspective is. He himself mentions at the very outset of his book the influence of Herbert Croley's *Promise of American Life* (1909) on his argument for a new nationalism. Croley, however, gives the impression of acknowledging federalism only because he had to, as something he would just as soon have done away with from the beginning—definitely not Beer's position.

As Beer himself suggests, this is a work of filial piety in the sense that he sees it as repaying a debt to his grandfathers who served in the American Civil War, in the struggle to save the Union and, as others have suggested, to serve Lincoln's purpose of transforming it from a union of states into a nation. In a sense, it is as much a product of the Civil War as we can expect from one who experienced it two generations removed.

To Make a Nation is an important contribution to the current literature on federalism. First of all, it is felicitously written, as one would expect from its author. Beyond that, it states the best liberal case for federalism. The central premise of Beer's book is twofold: (1) American federalism is national federalism, that is to say, is based on the establishment, development, and integration of a single American people (rather than a compact among the states, the theory advanced by John C. Calhoun in the nineteenth century and periodically since then, e.g., by Ronald Reagan) and (2) American nationhood is based upon federal democracy-that is, the sovereign American people organized their government on the basis of two coordinate sets of institutions, federal and state, both equally essential to American nationhood and its operationalization as democratic nationhood.

Herein lies Beer's greatest contribution for our times. His argument against the compact theory of federalism, it seems me, has by and large been long since won in the hearts and minds of the American people and most especially among American academics and intellectuals. It may resurface from time to time and its proponents may even be able to present a good case for it, but their words fall on deaf ears.

Far less appreciated is the connection between American nationalism, federalism, and democracy. That, indeed, leads to one of my problems with the book. It insufficiently recognizes the problem of centralization in American society—that American nationhood is, for all too many, no longer the federalist nationalism of Lincoln that Beer follows implicitly and explicitly, a nationalism that sees democracy as integral to it and federalism as integral to democracy. Instead, it has become a Rousseauean or Jacobin nationalism that looks only to a putative center in Washington for all significant decision making. This is as much a distortion of the nature of the American union as Calhoun's compact theory of federalism.

Beer leads us into a rediscovery of American federalism in *To Make a Nation*. In his introduction, he examines American nationhood and the three great historical challenges to it—sectionalism, industrialism and racism—concluding with a statement of the relationship of federalism and political theory. The key to this relationship is that "federalism is a not a humdrum matter of public administration but a serious question of political philosophy," which operationally means that "decentralization is constitutional, not merely statutory," re-

quiring "the process of consent wider than and different from the process required for the ordinary statutory law of the central government" (pp. 21, 23). To build this case, Beer takes us back to the Middle Ages, beginning with Thomas Aquinas and moving to John Milton and James Harrington. In Part 1, "From Hierarchy to Republicanism," Beer rejects the premodern assumptions regarding the organic and hierarchical order of society. Part 2 is "The National Republican Revolution," Part 3, "The National and Republican Constitution." In those two parts Beer explores the federal options and the discovery of the nation and the constitutional movement from a confederate republic to a compound republic—all designed to establish a "social union" such as was first propounded by James Wilson, one of the great national federalists of the constitutional convention. Thus, Beer builds a case for American federal democracy based upon the modern republican revolution, in what heand this reviewer—considers most important the federal and democratic dimension that gave birth to modern constitutionalism and modern ideas of liberty, both of which have served the United States well over the past two centuries.

Beer has a very good case, and he makes it very well despite some quibbles and suggested expansions that I believe would be helpful to him. Like him, I believe that American federal democracy must be taken whole—that is to say, that the American union established a federal democratic nation; that the federal government and the states are both instruments of that nation and are coordinate in that respect, to be regarded and treated as such; and that federal democracy is thus made operational through government by discussion and deliberation. To the extent that this has been true even beyond the structures of federalism, the union has been a success. To the extent that it has been true in opposition to the structures of federalism, in this my opinion, it has deviated from standards of American federal democracy that Beer sets before us and has to that extent failed, even when it has done so to make the United States a better social union, the last of Beer's points of emphasis.

Precisely because Beer's devotion of federal democracy is a liberal devotion, he demonstrates the close connection between federalism, democracy, and liberty that should lie at the basis of every liberal's creed and should shape American liberalism in the future as it has in the past. By rescuing federalism from conservatism, Beer demonstrates that for Americans, both are branches out of the same federalist trunk. The debate between them is at its most productive when it is a debate over the substance of federal democracy, not a fight between federal and Jacobin democracy as it is has become in the twentieth century. We are all the poorer for that, and we are the richer and, hopefully, the wiser for having Samuel Beer's book to remind us of that aspect of American heritage.

Center for the Study of Federalism DANIEL J. ELAZAR

State Government and Economic Performance. By Paul Brace. Baltimore: John Hopkins University Press, 1993. 152p. \$28.95.

Surely one of the enduring legacies of the Reagan era in American politics has been the resurgence of the states in the federal arrangement. Although such was part of the intent that lay behind the contraction of intergovernmental aid from Washington in the early 1980s, the president could hardly have anticipated the way in which many of the states would fill the vacuum in the realm of economic development policy. As the Reagan administration preached the virtues of the free market, deregulation, and rugged self-reliance, many of the states chose the quite opposite course of active economic intervention. To encourage business investment within their borders, employment growth in industries of the future, and a steady revenue stream, state governments undertook a wide variety of strategies whose outlines suggested nothing less than the sort of industrial policies familiar to the Europeans and Japanese

Naturally, it occurred to many observers to wonder just what it was that state governments thought they were doing. States, after all, are not nations: they cannot control their borders, their currency, or their trade. Embedded as they are in the national—and indeed, the international—economy, is it possible for states to affect their economic performance? This is the important question that Paul Brace addresses.

Brace is not the first scholar to struggle with this issue, which he calls the "mystery of state political economy." Though he begins by claiming that we have virtually no evidence that state government efforts have positive effects on their economies, there is in fact a substantial recent econometric literature, summarized in Tim Bartik's Who Benefits from State and Local Economic Development Policies? (1991), that demonstrates just that. Thus, what sets Brace's work apart is not that he manages to demonstrate positive state-level effects but rather that he offers us a theoretically rich state-of-the-art research design.

Brace argues that to discern state influence on economic growth, we must first control for national economic effects. It is here that the book makes a unique and important contribution. If state patterns of economic growth and decline mirror those of the nation, then we must conclude that the state economy is entirely dependent on national forces. If, however, states track distinctive patterns, then we can argue that state economies are shaped more or less independently of national economic trends. Once the degree of state dependence or independence is established, we may explore the relative influence of various factors subject to state control, such as governmental capacity, the programmatic commitment to economic development, tax policy, and political factors.

The analysis proceeds first by a series of case studies to provide a state-level view from the perspective of two low-tax, weak government, laissez-faire states (Arizona and Texas) and two relatively active intervenors (New York and Michigan). The lesson we learn here is that when the national economy is booming, as it did in the 1960s and 1970s, states with small and inexpensive public sectors did well. States with high capacity and the high taxes necessary to support it are at a comparative disadvantage in such a climate: big government acts as a brake on growth as mobile capital and labor migrate to low-cost states. But in a period in which national growth cannot be taken for granted, such as 1980s, states willing and able to invest in education, worker training, infrastructure, and economic development are better positioned to weather the economic storms.

Brace makes much of the rapid deterioration of the

Arizona and Texas economies in the last decade, as the real estate and defense economies went sour and low-wage industry migrated off shore. Unfortunately, their "endogenous political process and policies . . . appear ill-suited to compete in the more complex global economy. . . . That which served to recruit industry 20 years ago produced rates of illiteracy that are disadvantageous in a service-oriented, technologically driven economy" (p. 49). The experience of Texas and Arizona, then, suggests that economies of states with poorly developed capacity tend to ride the waves of national economic growth and decline.

The obverse of this argument is that the states whose economic performance is largely independent of national trends and that have elaborate public sectors and a history of active economic intervention did well in the Reagan years. But the fact is that many of these highcapacity states also experienced severe decline in the 1980s. Remember the demise (ill-timed for candidate Dukakis) of the "Massachusetts miracle": despite its array of innovative economic development programs put in place in the 1970s, Massachusetts could not stave off the effects of national industrial disinvestment or the defense build-down. For that matter, other aggressive economic players like Connecticut or Michigan could not do much better. Thus, the case for planned state economic intervention (which theoretically makes more rapid recovery possible) is not supported empirically by these case studies.

Brace then turns to an analysis of aggregate growth patterns from 1968-89 for the 48 continental states, first looking at trends over the entire period and then breaking the trends down in five-year segments. The latter exercise is revealing. Beginning in the early-to-mid-1980s, there appears what Brace calls a "fundamental change in the nature of political economy among American states." Although, over the longer period under consideration, national economic effects generally account for state fluctuations in value added in manufacturing and, to a lesser extent, in personal income growth, after Reagan's New Federalism reforms, the independence of the states greatly increases. Moreover, when various national economic forces such as defense spending are controlled, state capacity (e.g., economic development initiatives and tax policy) is significantly positively related to personal income growth and somewhat less strongly to increased employment. Income growth, in turn, is related to manufacturing growth, measured in value-added terms.

The bottom line, then, is that the role of the states as economic actors did change in the 1980s, just as a number of less technically rigorous studies have claimed: in the aggregate, at least, the states appear to have developed an effective array of tools and strategies for husbanding their economies.

At the end of the book, Brace is concerned about the durability of this new American political economy. He already had seen signs of eroding state commitment to the new activism. He was right to wonder; for even in the time since the book was finished, new indications suggest that the ardor that the states brought to their emerging entrepreneurial roles has begun to wane. Nevertheless, the experience of the states with economic development over the last couple of decades is an extremely important story for understanding both the extraordinary dynamism of the federal arrangement (illustrated particularly by the constantly changing roles

of the states vis-à-vis Washington) and the evolving relations between American governments and the market economy. With this book, Brace takes his place as one of the most rigorous and important chroniclers of these shifts.

University of Wisconsin, Madison

PETER EISINGER

Crafting Equality: America's Anglo-African Word. By Celeste Michelle Condit and John Louis Lucaites. Chicago: University of Chicago Press, 1993. 355p. \$56.00 cloth, \$18.95 paper.

America began, we are told, with Thomas Jefferson's enunciation of the principle that "all men are created equal," and its political history consists in the ever more perfect implementation of that putatively self-evident truth. The abolition of chattel slavery, the extension of the franchise, the recognition of women's right to full social participation, multiculturalism—all this represents the successful adjustment of reality to the demands of a precept that was fully comprehensive the moment it was articulated. In opposition to this cheering legend, Celeste Michelle Condit and John Louis Lucaites's exhaustively researched Crafting Equality marshals impressive historical evidence in support of the argument that the American conception of "full" equality—equality as the fundamental foundation and purpose of a political association dedicated to the common exploration of difference—is the late-blooming outcome of a complex encounter between Anglo-American and African-American publics. The distinctively American commitment to equality did not spring fully formed from the minds of the founders, whose conception of equality was only slightly more expansive than the very restrictive beliefs held on the subject by their British forebears. Our current usage reflects the creative energies of African-Americans as they reinterpreted the meaning of equality in light of their own experiences, interests, and aspirations, gradually convincing Anglo-Americans to make these innovations in linguistic meaning their own. As a result of these discursive achievements, the United States in the late twentieth century is as much African-American as Anglo-American. More broadly, the understanding that emerges from the history of how the goal of equality was "crafted" in America suggests a view of the polity as a creative work, reflecting the multiple ends of the various groups who find themselves on the North American continent and their capacity, through public argument and contestation, to produce themselves as a differentiated but coherent body politic.

Turning away from official policy pronouncements and canonical political theories, Condit and Lucaites survey the use of the word equality in public arguments over race relations in America—speeches, debates, pamphlets, journalism, and other informal public spheres. On the basis of these sources, they track the meanings attributed to equality in British political ideology, its modification by the American Revolution, and its further inflection by the African–American community beginning in the nineteenth century. Prior to interventions by the latter, we learn, even the relatively expansive conception of equality put in play by those dissatisfied with British rule relied on a conception of the American nation as a racially homogeneous people with a distinc-

tive "genius for liberty," from which enslaved peoples and their descendants were necessarily excluded. Especially problematic for this logic of exclusion (the dream of expatriation was its political expression) was the presence of free blacks, who regarded America as their home and resisted efforts to resettle them elsewhere. Their response was to universalize the meaning of equality, which they claimed applied to "all men" and included social equality (equality of material condition as well as political rights); a conception of citizenship based on the historical fact of being in America rather than on racial determinants; and an activist role for government in promoting and defending equality in these senses.

Such universalizing was only encouraged by the disastrous conditions legitimated by the separate-but-equal compromise between white supremacists and white and black egalitarians, leading ultimately to Booker T. Washington's idea of America as a "composite nation" grounded on full social equality for blacks and whites. Here, again, the authors show that with respect to linguistic innovation, white egalitarians followed leads invented in the black public sphere, with eventual federal support from the Roosevelt and Truman administrations leading ultimately to Brown v. Board of Education. By the 1960s, the rhetorical strategies pioneered by African–American publics were characteristic of much of mainstream American political discourse.

The backlash of recent decades stems from the fact that white egalitarian sentiment did not reflect the sensibilities of most whites, leading to a failure actually to achieve equality. This, in turn, led to divisiveness in the African-American public sphere, elements of which rejected the idea of America as a composite nation founded on equality, described America as inherently racist, identified being black solely with being a victim of racism, and put forward separatist ideologies. Such an approach, Condit and Lucaites suggest, turns its back on what is most valuable about America, namely, its commitment to the pursuit of multiple ends and to balancing diverse identities and histories in the context of a political community that is seen as an evolving discursive construction responsive to its members' changing opinions and needs. Rather than mime white supremacists by insisting that America is a "white man's country," African-Americans should build on their already considerable achievement and continue to craft an America conceived as the project of integrating differences without eliminating them, with equal participation and recognition as the watchwords.

Crafting Equality is one of those books that dramatically reorient one's view of the nature of American political life and cause one to appreciate anew the meanings of America's political vocabulary. That said, there is nevertheless something suspiciously anodyne about its picture of the basic character of American political passions. The idea that the American public is firmly committed to democracy and equality is believable if one restricts oneself to the explicitly political discourses surveyed here; but a very different, much scarier, and arguably more penetrating view of the American national psyche would emerge from a consideration of, say, American fiction from Charles Brockden Brown through Melville to Don DeLillo.

More fundamentally, Crafting Equality rests on an understanding of political discourse that is untouched by some of the most powerful contemporary inquiries into the relationship between language and the political,

such as those stemming from Nietzsche, Heidegger, and Derrida. Condit and Lucaites are committed to an understanding of political speech drawn from the ancient Greek rhetorician Isocrates, for whom discourse is seen as an instrument used to produce, fabricate, or (as they put it) "craft" a polity as a work shaped according to discursively established images. While this conception is salutary—inasmuch as it enables the authors to understand meaning as an emergent effect of collective conversation, rather than as a reified, statically given constant-it also leads them to embrace the idea of the polity as a work of art. Condit and Lucaites dismiss Derrida as "dangerously nihilistic" (p. ix), but Derrrida's colleagues Jean-Luc Nancy and Philippe Lacoue-Labarthe might have alerted them to the nihilistic dimensions of the aestheticization of politics, which the latter show to be deeply antipolitical in character. To be sure, Condit and Lucaites's original and persuasive interpretation of the Anglo-African character of American equality stands independently of this productivist conception of political discourse. But their willingness to dismiss Continental thought of extraordinary insight and force owing to the prejudice that its lack of "pragmatism" renders it alien to their subject marks their book as American in another, presumably unintended, sense.

University of California, Berkeley Frederick M. Dolan

The Year of the Woman: Myths and Realities. Edited by Elizabeth Adell Cook, Sue Thomas, and Clyde Wilcox. Boulder: Westview, 1994. 282p. \$49.95 cloth, \$14.95 paper.

Running as a Woman: Gender and Power in American Politics. By Linda Witt, Karen M. Paget, and Glenna Matthews. New York: Free Press, 1994. 330p. \$22.95.

Nineteen Ninety-Two was widely heralded by political observers as "the Year of the Woman." In some ways, 1992 was a breakthrough year for women and politics, while in other ways, 1992 represented politics as usual and continuity with the past. Running as a Woman and The Year of the Woman analyze the distinctive factors and developments that made 1992 an unusual year for women running for office, place the gains made by women in the 1992 elections in a larger context, document women's recent political progress, and examine continuing obstacles to women's political success. In doing so, these two books cut through the media hype that surrounded the so-called Year of the Woman and contribute to a more reasoned assessment of the significance and limitations of the gains women made during the 1992 election cycle.

Both of these books examine women's involvement as candidates, voters, and financial contributors to women's campaigns; and both add significantly to our understanding of how and why 1992 became such a critical election year for women in politics. However, analysis of the 1992 elections is more central to *The Year of the Woman* than to *Running as a Woman*. The Year of the Woman was conceived during—and written immediately following—the 1992 elections. The Year of the Woman takes the 1992 elections as its central focus and uses the elections as a lens through which to view and analyze women's involvement in electoral politics. In contrast, the research that led to *Running as a Woman* was well underway prior to the 1992 election cycle. *Running as a Woman* focuses on

historical and contemporary changes in women's roles in electoral politics and integrates discussion of the 1992 elections into a more general analysis of women's candidacies and campaigns.

In addition, the two books give different weight to various explanations for why 1992 was such an exceptional year for women running for office. Both books point to the treatment of Anita Hill during the Clarence Thomas confirmation hearings and the unusually large number of open seats in Congress as two of the most important factors accounting for the record numbers of women running for, and elected to, Congress in 1992. However, Running as a Woman seems to give more weight to the Anita Hill explanation, while The Year of the Woman stresses to a greater extent the unusual number of political opportunities available to women in 1992. Running as a Woman opens with an extensive discussion of the effect that Anita Hill had on the elections, arguing that "before Anita Hill, there was little reason to think that the change in the 1992 elections would be other than incremental" (p. 2). The effect of increased numbers of open seats is not mentioned until several pages later (p. 6). By contrast, Clyde Wilcox, in the introductory chapter to The Year of the Woman, gives far more emphasis to the existence of a record number of open seats and women's willingness to take advantage of those opportunities, especially in elections for the House of Representatives. Although Wilcox acknowledges that the Senate candidacies of Lynn Yeakel, Carol Moseley-Braun, and Patty Murray were motivated in part by Anita Hill's treatment before the all-male Senate Judiciary Committee, he draws upon the research of other contributors to the book to argue that the candidacies of each of these women were also motivated by unique political opportunities specific to their races (pp. 7-8).

Both books have multiple contributors. The Year of the Woman is an edited volume with 14 chapters, including a chapter on each of the major 1992 Senate races involving a woman candidate (California, Illinois, New York, Pennsylvania, and Washington), the House elections, state legislative elections, political parties, women's political action committees, political advertising, voting, and the impact of women public officials. About half of the contributors are established scholars of women and politics. Most of the other contributors have expertise in

votes. Nevertheless, the very fact that one can attribute certain sections or elements of the book to one coauthor or another highlights a weakness of the book: multiple voices are still evident, and the styles do not flow together as smoothly as one would find in a single-authored work.

The major argument of Running as a Woman— and one well substantiated by the material presented in the book—is that recent elections, particularly the 1992 elections, have witnessed a shift in strategy on the part of women candidates. Most women have traditionally run for office by emphasizing their similarities to men, and consultants have advised women candidates not to call attention to their femaleness. However, in response to a changing political environment, more women in recent elections have chosen to run as women. For example, in 1992 women candidates often focused on the need to elect more women public officials, emphasized women's issues, and targeted women voters.

Some of the topics examined in Running as a Woman are familiar ones that have received considerable attention in other works focusing on women candidates. In some respects, Running as a Woman, especially in the chapters on squaring the personal and the political and funding women's campaigns, provides a 1990s update to Ruth B. Mandel's In the Running: The New Woman Candidate (1981). The gender gap in voting and the impact of women elected officials-two topics to which chapters are devoted—have similarly received serious and sustained examination in other works. However, this book also includes chapters on topics that have received little attention in previous books on women candidates. One particularly original chapter examines how the legacy of Jezebel—"a woman whose sexuality threatened the well-being of the community" (p. 54)—has shaped perceptions of political women and has affected their public presentations of self. Other chapters focus on risk taking and losing and on press coverage of political women-important topics that have been largely overlooked in previous works on women and politics.

Judging from the notes, the coauthors of Running as a Woman conducted interviews with numerous well-known women public officials, candidates, political consultants, and leaders of viewer's political action compared to the conducted of viewer's political action compared to the conducted of viewer's political action compared to the conducted of viewer's political action compared to the conducted of viewer's political action compared to the conducted of viewer's political action conducted to the conducted of viewer's political action conducted interviews with numerous well-known women public officials, candidates, political conducted interviews with numerous well-known women public officials, candidates, political conducted interviews with numerous well-known women public officials, candidates, political conducted interviews with numerous well-known women public officials, candidates, political conducted interviews with numerous well-known women public officials, candidates, political conducted interviews with numerous well-known women public officials, candidates, political conducted interviews with numerous well-known women public officials, candidates, political conducted interviews with numerous well-known women public officials, candidates are not action to the conducted interviews with numerous well-known women public officials and the conducted interviews with

Year of the Woman are based on newspaper reports, observation, and presentation or analysis of publicly available data. Few are based on original data collection. For example, of the five chapters examining U.S. Senate races in specific states, only Jean R. Schroedel and Bruce Snyder's chapter on Patty Murray's race appears to be based, in part, on personal interviews. The most extensive original data collection was done by Leonard Williams, who, in his chapter on political advertising, presents findings from a content analysis of 59 campaign advertisements from U.S. Senate races in 12 states. Elizabeth Adell Cook, Mary E. Bendyna, and Celinda Lake do examine data from the 1992 exit poll conducted by Voter Research and Surveys in their chapters on voting, providing some very interesting insights about women voters' reactions to women candidates and presidential candidates; and Robert Biersack and Paul S. Herrnson analyze Federal Election Commission data in their chapter on the role of political parties.

Both The Year of the Woman and Running as a Woman are excellent choices as supplemental textbooks for courses on women and politics or campaigns and elections. While there is some overlap in content, the two books would complement each other well as classroom texts. Running as a Woman is full of anecdotes and examples to engage students, and it raises most of the issues about women candidates and their campaigns that one would wish to address in a classroom setting. The Year of the Woman also examines many critical aspects of women's involvement in contemporary electoral politics but in a more academically rigorous fashion. Scholars, as well as students, will find both books useful; and both will be frequently cited in future research on women and electoral politics.

Rutgers University

Susan J. Carroll

Representations in American Politics. Edited by Frederick M. Dolan and Thomas L. Dumm. Amherst: University of Massachusetts Press, 1993. 296p. \$40.00 cloth, \$16.95 paper.

Republic of Signs: Liberal Theory and American Popular Culture. By Anne Norton. Chicago: University of Chicago Press, 1993. 195p. \$34.00 cloth, \$12.95 paper.

There is a good deal of overlap between these two engaging, if occasionally dispiriting, books. Anne Norton supplies herself and the editors of Rhetorical Republic (to which she contributes a chapter) with an organizing theme, namely, struggles over the "representations" that do, should or ought not govern politics and culture in postmodernist or late modernist polities. America is the primary empirical focus of these reflections, but the American polity and culture are also treated as the paradigmatic instance of the wider phenomenon taken, somewhat problematically, to be signified by these terms. Historically, phenomenologically, and perhaps "quasi-ontologically" (Derrida), the United States is a "republic of signs" or "rhetorical republic," a polity and culture that are "governed" not by material realities, institutional structures, or even ideas or ideologies but by signifiers and representations.

This theme has two sometimes dissonant strands or elements. Metatheoretically, it opposes empiricist or otherwise realist views according to which there are extralinguistic realities or canons of reason that could and should discipline rhetorics and representations. Substantively, however, numerous of the representations that in fact "govern" America are judged to be regretable in character. Although officially denying that there are divine, natural, or rational signifieds that ought to control our signifiers and representations, several of the authors complain, mournfully and occasionally bitterly, that the representations that regulate America endanger or have already destroyed estimable potentialities.

The second of these tendencies makes a qualified appearance in Dolan and Dumm's introduction to Rhetorical Republic. They acknowledge the impossibility of representations that govern in the strictest sense of logically, ontologically, or otherwise reducing us to one set of possibilities. In principle, all signs and signifiers-and hence all representations-are subject to contestation. But there is a "will to impose" exclusive representations, and it frequently succeeds in stilling competition over them. All of the essays in part 1, "Cold Wars" (though treating many events of death and destruction) and two in part 2, "American Alterities," hammer on this anvil. Avital Ronnel's psychoanalytic reading treats Desert Storm as George Bush's war of termination with himself but also as characteristic of a tendency to despotic closure that Ronnel thinks is evident in democratic theory and practice at least since Kant. David Campbell's powerful "Cold Wars: Securing Identity, Identifying Danger" claims that America manifests an aporia inherent in the identity of republics, one that typically is "closed" by constructing, however fancifully, enemy others. Variants of this argument are also advanced in a Girardian idiom by George, Derrian, and Honderich with reference to the war against drugs; in Diana Rubenstein's Lacanian assessment of Oliver North, Iran/Contra, and national security operations generally; in Stuart Clarke's Gramscian dissection of the Willie Horton and Clarence Thomas episodes; and perhaps even in John Dolan's lament that protest poetry is tiresome in its liturgicality.

On these views, then, the American polity and culture are governed by representations that are effectively hegemonic. In these papers, what I call the "metatheoretical thesis" is employed primarily to claim that the authors themselves have means sufficient to enable them, apparently quite readily, to see through—to unmask-governing representations. (Occasionally an excess of deconstructive and especially psychoanalytic enthusiasm yields intimations that the unmasked representations are not only not true but are false, that they distort or misrepresent a reality that the critic has apprehended and that warrants the critique. Charitably con-

strued, these moments are lapses.)

Anne Norton sometimes participates in the tendencies just discussed. She argues, for example, that American liberal politics and culture features a governing "presentation" of women that subjects them to a "feudal" position or situation (Rhetorical Republic, pp. 126-29; Republic of Signs, pp. 47-48, 54). For the most part, however, the Republic of Signs is more nuanced, adventuresome, and even optimistic than the essays. In a "liberal regime of representations," all representations—all signs and signifiers—at once constitute and provide means of challenging and reconstituting their subjects or signifieds. They are "duplicitous" not in the sense of misrepresenting or lying about some prediscursive reality but in that they enable and invite a doubling,

a more or less continuous movement between passively being made by and actively making, being overcome and overcoming. The activities of naming and representing presuppose a more or less stable setting (the "unself-conscious commonalities of liberalism"); but within such a context, representations re-present not in the sense of presenting again that which has already been established or institutionalized but in the more creative sense of remaking, reconstituting.

Americans are a "people of the text," a people who wrote themselves into being in documents such as the Mayflower Compact, the Declaration of Independence, and (especially) the Constitution. But these and the innumerable less celebrated texts that make up the popular culture (a culture that Norton reads closely and creatively), while having largely made Americans into what they thus far have predominately been, are equally devices enabling Americans to contest and remake what their own texts have represented them as being. In discussions that range from the Constitution and the presidency to money and shopping, voting, lotteries, and survey research, Norton discerns and imaginatively invents possibilities that exceed recognized actualities and already approved opportunities.

Norton's evident affection for America and for liberalism at the best they have been, are, or could become provides a counterweight to the negativity of numerous of the essays in *Rhetorical Republic*. The most engaging of its essays—those by Chaloupka, Honig, Villa, and Connolly—share the inveterately unstable combination of critique and affirmation that Norton seeks and (occasional anodyne moments aside) for the most part sustains.

The Nortonian themes common to the essays can be signaled by the terms *refusal*, *resistance*, and *augmentation*. We should refuse and resist the numerous racist, sexist, imperialist and otherwise overweening tendencies of our liberal American polity and culture. And we should strive to augment the best parts of what has acquired authority or achieved authoritative standing in that polity and culture. Because our culture is replete with ambiguity and dissonance, because it is a liberal culture as well as a republic of signs or representations, it affords resources necessary to critical and constructive initiatives.

In a spirited and compelling counterpoint to widely received interpretations (e.g., Gitlin's) of New Left and later street politics, William Chaloupka reads them as Arendtian theatricalities, as performatives whose "speed" at once depends upon and exceeds the possibilities afforded by generally accepted ideologies and institutionalized structures and roles such as electoral politics and citizenship. It may be true that the "cheeky tynics" (Sloterdyk) displayed in the spectacles they staged were lacking in the definite, substantive goals widely demanded of political actors; but they drew upon and revivified the noninstrumental idealism that Norton shows to be liberalism at its most exalted.

Arendtian themes are yet more explicit and prominent in the essays by Honig and Villa. Honig reads Arendt as attempting to invent representations that ought to govern the American polity. In particular, she argues that Arendt sought to theorize a practice of authority suitable to a politics that must do without foundations, that can have no fixed or unalterable basis or bases. The authority of the Declaration and the Constitution was created by performatives such as "We hold, . . ." but this very fact about them requires that the "truths" thereby con-

stated must be viewed as continuously resistible and constantly in need of augmentation. Similarly, Villa argues that Arendt invented a conception of "the public sphere" that features a "politics of parody." Precisely because it would eschew epistemological and ontological certitudes—would self-consciously internalize resistance and self-critique—such a polity and politics would be protected against the disillusionment and delegitimation to which truth- or reality-claiming regimes are so evidently vulnerable. A republic of signs, representations, or appearances would be, at once and for the same reason, better secured and more fecund than a republic of signifieds.

Chaloupka, Honig, and Villa, inspired by Arendt, seek to make a friend of what many will regard as the enemy, that is, radical uncertainty and indeterminacy. A politics unconstrained by law, truth, or reason would give wings to the human spirit. In his lively argument for a politics that exceeds territorial boundaries, William Connolly is moved by the same idea/ideal but pursues it beyond the limits that Arendt explicitly endorsed. Arendt preceded Connolly in her disdain for the notion of sovereignty. But an Arendtian politics presupposes what Villa calls "spatiality," that is, a community of people bounded by geography. Connolly's insistence on the importance of institutionally based "electoral accountability" sustains, nervously, more than a remnant of this assumption. In the name of a democratic politics appropriate to late modernity, however, he urges us to think of the signs or signifiers that delineate states as every bit as open to contestation and resistance as all other putatively final markers. He concedes (at last!) that "democracy . . . is a form of rule or governance" (and hence must often be resisted) but argues that it is (might be?) also "an egalitarian constitution of cultural life" that should no more be confined spatially or territorially than it should be circumscribed by natural law or class and caste structures.

If I have a general hesitation concerning this last group of essays, it concerns their sometimes intense valorization of the notion of politics. Honig reminds us that whereas, for Arendt, politics is a creative and humanizing activity that "ends with the entry of the . . absolute," on Derrida's Weberian view "politics begins" when absolutes are asserted and especially when attempts are made to impose them by authority or power. On the latter view, politics is much more a matter of resistance than of creation. We turn to politics to protect our creative endeavors from threats launched by the state and other aggregations of power. This viewwhich I think can be discerned in Republic of Signs, perhaps in Chaloupka's essay, and possibly even in Connolly's on egalitarian cultural life—seems to me to be the more estimable of the two.

The quality of the thinking presented in these two volumes helps to explain—and will help to sustain—the gratifying circumstance that numerous talented young people are now attracted to that nexus which is called political theory. It would be pleasing to think that this is because these books, like much in the recent literature, are pervaded by the spirit of one of Wittgenstein's choicest remarks: "When one draws a boundary it may be . . . to prevent someone from getting in or out. . . . But it may also be a part of the game [that] the players be supposed . . . to jump over the boundary."

Johns Hopkins University

RICHARD E. FLATHMAN

Bitter Fruit: Black Politics and the Chicago Machine, 1931–1991. By William J. Grimshaw. Chicago: University of Chicago Press, 1992. 248p. \$24.95.

Since the 1960s political coalitions throughout the country have struggled with some success to overcome white racial resistance by capturing the political resources of city politics. Population shifts have rewarded black voters in their attempts to turn conservative coalitions out of office in city after city over the last thirty years. As the Republicans turned to the suburbs to capture national politics, central cities remained the last bastions of liberal Democratic reform. Recent fortunes have not been so kind to liberal reform. Writing at a time when black dominant coalitions have been overturned in New York, Los Angeles and Philadelphia, William Grimshaw provides a timely and richly documented account of how black politics, Chicago style, though it participated in the rise of the Daley machine and was responsible for its fall, was unable to institutionalize a reform tradition to succeed the machine era

How did blacks face an era of machine politics? The answer, according to the author, is with an awareness that if you were black you could not necessarily depend on the traditional adage that votes would translate into material rewards. Grimshaw questions the dominant model of machine politics, which posits that the machine attracted black voters by simply exchanging economic benefits for political support. Instead, turning the "economic" model on its head, Grimshaw argues that even though the Daley machine largely owed its success to black support, the black community received only "bitter fruit" in return.

The book begins as a critique of the economic model in which machine politicians are assumed to hold a "unitary" interest in winning elections and distributing patronage. Machine politicians are viewed by Grimshaw as operating within a loosely coupled system seeking to win elections by building coalitions among factional rivalries in order to expand their bases of power. When applied to Chicago, the economic model, according to the author, oversimplifies a complex political process that resulted as much from critical local and national events as from assumptions concerning group and elite calculi.

The author argues that from the perspective of citizens the study of machine politics requires a distinction among three concepts: exchange, representation and empowerment. Machines grant small favors in exchange for political support but generally avoid substantive representation. Mayor Daley often provided symbolic representation in order to quiet ethnic and racial rivalries within the organization. Substantive representation that moved toward power sharing was an anathema. And, "needless to say, if the machine looks unfavorably on demands for representation, it abhors any demand for empowerment." Daley heeded black politicians when he needed black support as a way to gain control of the organization. Once gaining control, he often successfully coopted black loyalists who sought benefits in return for political support and shunned those who pushed a reform agenda. His use of William Dawson in order to control black votes became legendary, though "Boss Dawson" was less powerful than often portrayed, in that his power waned through successive battles within factions of the governing coalition. The machine was not monolithic. Even though it was hierarchical, it was filled with factions.

Grimshaw offers a novel interpretation for why black support of the machine came relatively late and was selective. Rather than responding to machine incentives at the local level, black voters favored machine candidates only after the new deal realignment occurred at the national level. Middle class wards in particular had voted less regularly for machine candidates than lower income wards and often provided leadership to resist machine influence. Once racially restrictive housing covenants were outlawed in 1948 middle class blacks began moving out of the black belt providing a more stable base of support for the machine among lower income black voters. Ironically, Daley first ran a populist campaign with black support against downtown business interests in a racially charged campaign organized by the incumbent Mayor. Black support was important to the stability of the early Daley era. Between 1955 and 1963 the Mayor sought to co-opt black leadership by appointing "civic notables" to boards and commissions. After that point, with rising civil rights protests the Mayor subverted efforts to increase black representation and political incorporation by appointing "loyalist" Catholic black leaders who were more insulated from the essentially Protestant Civil Rights movement and held closer ties to the machine's Irish Catholic leadership.

Machine resistance to black empowerment began to crumble as the national civil rights movement gained momentum in the mid-sixties. Grimshaw skillfully demonstrates how middle class black wards began to lay a basis for the movement from machine politics to racial politics, eventually resulting in the election of Harold Washington as Mayor in 1983.

Electoral support for the machine began to unravel with the death of Mayor Daley in 1976. A convergence of forces including growth in the percentage of the voting age population who were black as well as growth in white liberal and black middle class commitment to a more redistributive political agenda resulted in an unprecedentedly active reform electoral mobilization effort around Harold Washington's candidacy. Given these forces, it is still questionable whether Washington would have won the election if it had not been for the lucky accident of an opposition in disarray. Even though Mayor Washington's reelection in 1987 was heralded as proof of the demise of traditional machine politics his untimely death provided an opportunity to reinstate the long standing dominant coalition dedicated to limiting reform efforts by those who would push even a mildly redistributive political agenda.

If the experience of other cities is a guide, the author is right in his conclusion that until blacks come together in a multi-racial coalition with their Hispanic and white liberal allies they are unlikely to be able to reestablish the reform agenda in Chicago. Multi-racial electoral coalitions, as the Chicago experience suggests, though difficult to build and sustain offer the most promise for an increasingly responsive city politics.

San Francisco State University

DAVID H. TABB

Echoes of Discontent: Jesse Jackson, Pat Robertson, and the Resurgence of Populism. By Allen D. Hertzke. Washington: CQ Press, 1993. 293p. \$29.95 cloth, \$19.95 paper.

In this thoughtful and well written book, Allen Hertzke takes the unconventional view that Jesse Jackson and Pat

Robertson are more similar than stereotypes might suggest. Hertzke argues that the two preacher-politicians in fact have much in common, having combined prophetic preaching with church-based political organizing to change dramatically the character of American presidential politics in our time. Moreover, he contends that although they appear to be ideological opposites, they can be seen as sharing a similar political perspective, a tendency to see politics as a process in which discontent, resentment toward elites, and faith in charismatic leadership are channeled into a religiously-based brand of popular protest that he calls "gospel populism." To describe this shared populist strain, the author draws on a diverse set of methods and sources: analysis of biographical, historical, and theoretical texts; extensive interviews with Jackson and Robertson as well as with many of their advisors and selected followers; interpretation of survey data from state and national election studies; and firsthand observation of the Jackson and Robertson campaigns over a four year period from 1988 to 1992. He combines these approaches carefully and at times elegantly, and the result is a book that is scholarly, thought-provoking, and readable, and that would be an excellent choice for classroom use.

At the heart of the analysis is the concept of gospel populism, which is described as a paradoxical blending of economic progressivism and social conservatism. Avoiding the once-popular social and psychological explanations that depicted populist politics as the product of authoritarian paranoia or anxiety about social status, Hertzke goes deeper to place populism in cultural and historical context. He finds its roots in the rural and small-town localism of eighteenth-century America, and follows the growing gap that opened over the course of the nineteenth century between this Christian communitarian culture and an increasingly cosmopolitian and liberal one. The diverging strains came into conflict at the close of the century, giving rise to the populist movement and to William Jennings Bryan, whom Hertzke chooses—a bit too blithely given the atypical character of Bryan's brand of populism—as representative of populist sentiments. Advocate of child labor legislation and enemy of evolution, champion of pacifism and prohibitionism, feminist and fundamentalist, Bryan embodied the paradox of economic progressivism and moral traditionalism, of protest and piety, that shaped gospel populism. So too do Jackson and Robertson, as Hertzke describes them: Jackson the economic egalitarian who denounces the abuse of drugs and decries the decline of the family, Robertson the moral conservative who chastizes corporate elites for their greed and champions the Calvinist virtues of Main Street over the conspiratorial schemes of Wall Street. At times the argument is overstated, as in the description of Jackson's agony over abortion, which the author claims he opposed for religious reasons but supported for political ones. In the same way, it is possible to make too much of Robertson's rantings about conspiracies of international bankers and their allies on the Trilateral Commission. At times, Hertzke seems to concentrate too much on similarities of style and too little on differences between the policy positions of the candidates. Nevertheless, the comparison is enlightening, and the book's most important contribution is the insight it offers into the complex character of these politicians.

After a discussion of the development of Jackson and Robertson as religious and political thinkers, the book

goes on to link personal and intellectual considerations to political and institutional ones. In a series of chapters, it considers the role of churches in the 1988 presidential campaigns, examines the assimilation of religious activists into the major parties, and surveys the characteristics and opinions of Jackson and Robertson voters. In different ways, these chapters treat the tensions that existed in these campaigns between church-based and party-based politics. Borrowing from resource mobilization theory, Hertzke compares Jackson's reliance on the black church—"the church as precinct"—to Robertson's use of television and mass mailings-"the charismatic network"-and he presents both as threats to established party politics. Contrasting Rainbow Coalition with Christian Coalition, he describes the moral motivations that bring the activists in these organizations to politics, elaborating on the costs and benefits that come to the parties when they recruit them. Finally, interpreting information taken from state and national election surveys, Hertzke shows that Jackson and Robertson supporters have much in common, being as a group more typically female, minority, and working class than the backers of other more conventional candidates. The comparisons go only so far, and Hertzke allows that while women supported the two candidates, they did so for dramatically different reasons, and that Jackson backers took ardently feminist stands while Robertson supporters tended decidedly in the opposite cirection. Even so, he is convincing in his claim that the two groups are a lot alike, that they share a religious world view, a perception that elites are not listening to them, and a pessimistic assessment of the future of American politics.

In concluding, Hertzke speculates about the future, and here his argument becomes more challenging and potentially more controversial. Admitting that populist movements usually fail, but citing survey findings that the American electorate seems increasingly alienated and angry, he assesses the potential for a broadly based "resurgence of populism." Here the analysis becomes disconcertingly broad and blurry, including references to recent "populists" such as John Silber, Patrick Buchanan, and David Duke, along with Harris Wofford, Jerry Brown, and even Bill Clinton. Yet by expanding his focus, Hertzke can caution against the dangers, particularly the dangers of anti-Semitism and racism, that can follow from a politics of discontent. Furthermore, he can remind readers of the more positive potential for contemporary populists to take stands that are both egalitarian and morally virtuous. In this regard, he makes an articulate argument that populists of left and right can find common cause in supporting the economic and moral welfare of the nation's children. Ultimately, Hertzke is realistic, resigned to the fact that the promise of populism remains unfulfilled, an echo of discontent. But he is also hopeful that some brand of populist communalism can provide an antidote to the anemic state of American politics, and his book offers a sensible vision and an strong voice in support of a more economically secure and more morally sound politics.

University of North Carolina, Chapel Hill

MICHAEL LIENESCH

Legitimacy and History: Self-Government in American Constitutional Theory. By Paul W. Kahn. New Haven: Yale University Press, 1993. 260p. \$27.50.

The Partial Constitution. By Cass R. Sunstein. Cambridge: Harvard University Press, 1993. 414p. \$35.00.

Both of these books are sharply focused and forcefully argued. Both are by distinguished authors covering much the same ground and are based on broad understandings of their subject matter. As a result, both will well repay careful reading. This is especially true of the Kahn book; because of the power and consistency of its analytic scheme, a course in American constitutional theory could be taught straight from its pages. Sunstein's argument is more particular, but many of his arguments and commentaries would "backstop" such a possible course wonderfully. Yet there is about both these books a tinge of unreality, almost of political naïveté, that must detract from their shared enthusiasm for deliberative (or "discursive") democracy (or "liberal republicanism"). The enthusiasm itself cannot be questioned. The relevance of this vision of democratic life to the extant American political/constitutional system can be questioned—and sharply.

Sunstein has the more explicit statement about deliberative democracy's meaning, for defining its terms is central to his argument. He opens his book with what becomes one of its major themes, a sharp attack on the principle of neutrality by reference to which the Supreme Court, especially in cases involving constitutional review, tries to settle matters in ways that disturb the status quo and its given distributions of power and material benefits as little as possible. Any alternative course, it is often supposed, would necessarily involve some measure of judicial "activism" and "partisanship" on behalf of groups urging change. But, argues Sunstein, the given social order is not given as if from nature itself: it is a social/legal/constitutional construct as much as any other. Moreover, any decision to sustain it is an act of impartiality that not only may perpetuate long-standing injustices but also is a prejudicial judgment that certifies the status quo as legitimate by reference to criteria that are external to itself. The appeal to external standards is inevitable, for no constitutional system can self-legitimize. Sunstein would much prefer that the Court be self-conscious and open about this process and (again, especially in its constitutional review work) strive to render decisions that show a consistent partiality to realizing more perfectly the founders' central original aspiration for the United States, namely, that it should be a deliberative democracy.

Sunstein's general account of this concept is simply that it requires that policy decisions should be justified and shaped by reasons grounded in appeals to the public interest and not by reference to the naked self-interest of particular groups and their raw political power. In chapters 5 and 6, the heart of his book, Sunstein articulates this general conception in terms of four further principles: (1) a commitment to the notion of political deliberation itself, what used to be called "government by discussion," (2) pride in partipatory citizenship, (3) celebration of agreement as a regulative ideal and, (4) the need for genuine measures of political equality. In his treatment of these matters, Sunstein is always careful to point out that the other branches of the government and local government should do more to realize America's democratic ideals than the courts.

Nevertheless, he gives over the whole of the second part of his book to showing how a conscious commitment to deliberative democracy would force the court to change (or at least reargue) many of its decisions in areas ranging from free speech, to reproductive rights, to education.

Even if with considerable pessimism, Kahn argues in his book to something like the same conclusions. His analytical scheme arranges the history of American constitutional theory in a logical progression. The first stage has the founders designing a constitutional structure in terms of a rational response to an agreed political science. The Marshall court, especially, followed suit; and its great decisions are best regarded as evolutions from the same political science. The next stage sees the court now deferring reverently to the word, spirit, and intent of the founders simply to conserve their work. This stage is followed by a more erudite one in which the court, much guided by academicians, sought to place itself and its decisions on the high road of social evolution toward civilization (a self-understanding that to me sounds more like Hegel than Darwin, especially in reference to Woodrow Wilson). The modern period sees a split between the academy and the courts, with the academy increasingly preoccupied with evolving theories of discursive democracy (there is a discussion of Sunstein's earlier work) and the Court more and more locked in the paradox of individual autonomy versus majority rule. Neither side, in Kahn's view, has been able to solve what is essentially a problem of political obligation: Why should a democratic people empowered in the present moment honor a court (and its power of judicial review) whose legitimacy is based on history?

My doubts about these books are essentially two. One has to do with how deep their understanding of constitutional theory goes. Neither book addresses questions about the role of the courts in a democratic society in a manner or profundity that would show that they had learned anything from Aristotle or Hobbes. More particularly, Kahn's analytical scheme seems more to march around an unexamined bedrock of understanding about the nature of courts, adversary proceedings, and the functions of judges and the law in them than to establish any deep line of progression. My second doubt is directed toward their shared understandings of the aspirations of the founders and of the democratic character of the constitutional structures they created.

Both books assert that the founders subscribed to the doctrine of popular sovereignty. They probably did, but the phrase has a double meaning. On the other hand, it refers to that general popular acceptance and daily relegitimization which any constitutional republic, regardless of its particular characteristics, must receive simply to survive. Citizens provide this not just when they vote but whenever they obey the law in any way. The American founders understood and accepted that understanding of popular sovereignty soundly. Whether they accepted the phrase in a second and narrower sense that would have "the people," by deliberate constitutional provision, involved centrally and on a near-daily basis in the policy making and implementation of the government can be doubted. Before even the House of Representatives can be judged discursively democratic (at least in aspiration), the founders' theory for that body needs to be stated explicitly and related to its constitutionally provided characteristics; and the behavior of the House today would have to be related and understood.

The books fail on these two counts, nor do the authors show any conscience about it. Their enthusiasm for discursive democracy transcends: it is as if they had never heard of Rostenkowski—not the congressman indicted for corruption but the chairman of the House Ways and Means Committee, who would have to be invented if he did not exist (as often enough he does not).

New York University

H. MARK ROELOFS

Rethinking Constitutional Law: Originalism, Interventionism, and the Politics of Judicial Review. By Earl M. Maltz. Lawrence: University Press of Kansas, 1994. 148p. \$27.50.

The Constitution and the Courts: Law or Politics? By Michael J. Perry. New York: Oxford University Press, 1994. 227p. \$35.00.

Whatever its merits in law or politics, the 1986 nomination of Judge Robert Bork to be an associate justice of the Supreme Court was a definite boon to the cottage industry of constitutional law scholarship. By endorsing the idea that courts should enforce the framers' original conception of the Constitution, Bork created shock waves that scholars are still trying to absorb or deflect.

Both Maltz and Perry—longtime combatants in the battle over constitutional interpretation—take on Bork in their new books. Maltz's concern is to critique all sides in the noisy debate, urging theorists of both the Left and the Right to "abandon conventional wisdom and reorient their thinking in new directions" (p. 108). Perry takes on the more ambitious task of harnessing the theory of original intention to his own purposes, which are decidedly different from Bork's. Both books are admirable examples of judicial scholarship; but after reading them, one longs for a cessation of the Constitution wars or, at least, for a UN-imposed cease-fire.

Though Maltz and Perry attack the problem of constitutional interpretation in very different ways, both keep Robert Bork very much in their sights. Both in his confirmation hearings and in his subsequent book, Bork advanced the theory of "original understanding" as the only defensible approach to judicial interpretation of the Constitution. Bork's view, in brief, is that judges should endeavor to ascertain what the Constitution was understood to mean in 1789 (or, in the case of amendments, whenever they were adopted). The theory of original understanding is not exactly the same as the theory of "original intention," since the people who ratified the Constitution may have had a different interpretation of the document than the framers themselves. In practice, however, this distinction is of little consequence.

Determining the framers' original intention, of course, is fraught with practical problems, not least of which is the obvious difficulty in agreeing on exactly who qualifies as a framer. Bork's answer is to endorse a theory of judicial minimalism: when judges cannot determine the original understanding, he argues, they should simply refuse to enforce the decree at all, leaving the field open for legislative and executive action. From Bork's point of view, in other words, an ambiguous constitutional provision might as well not be there at all. And since the most important constitutional provisions are ambiguous, that would mean a shift away from judicial activism

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and judicial intervention—lo and behold, exactly the result endorsed by conservatives like Bork!

Both Maltz and Perry point out that the minimalist approach does not follow automatically from originalism. On some matters (e.g., the First Amendment) an originalist might well justify a strong judicial role in restraining government power. In fact, the originalism/nonoriginalism debate is almost wholly separable from the parallel argument on minimalism/interventionism. The first reflects different approaches to constitutional interpretations; the second, different attitudes toward the appropriate use of judicial power.

Maltz is content to the analyze these and other issues and to find fault with scholars on all sides of the debate. He discusses not only originalist defenses of judicial review but also nonoriginalist approaches, including those that derive judicial review from democratic theory, from the need to restrain the federal government, and from the nature of the judicial function.

A list of those whose theories Maltz dismisses would read like a Who's Who of American constitutionalism: Leonard Levy, John Hart Ely, Herbert Wechsler, Cass Sunstein, even Madison and Hamilton. When Maltz's ax falls, moreover, it falls with finality. Arguments "cannot be defended," they "collapse completely," they rest on "fundamental misconceptions" (pp. 31, 42, 63). Much of Maltz's analysis is sound, and his shredding of contemporary judicial scholarship is at least partially justifiable. But critics should keep in mind that pointing out the flaws in complex and difficult arguments is far easier than building them up.

Perry, by contrast, seeks not only to discredit "Bork & Co." (as he calls them) but also to construct his own theories of constitutional interpretation and judicial intervention. He begins by accepting originalism—not least because no other justification of judicial review would be credible in the American context. Thus, a judge's first task is to reach "a plausible conclusion" about the "constitutional directive" that underlies a given constitutional provision. When there is more than one such conclusion, the judge must choose among them. One factor likely to influence this decision is the judge's own view of the proper judicial role: a judge who is uncomfortable with judicial intervention might choose differently from one who takes the opposite approach.

By conceding that the Constitution might plausibly have more than one meaning, Perry deftly sidesteps the most serious objections to originalism. By doing so, however, he also undermines much of the appeal of originalism, which lies in its assertion of a clear and controlling constitutional text. In any event, Perry does not stop there. A judge who has reached a plausible originalist interpretation is then free to engage in "constitutional specification"—to decide, in other words, what the constitutional provision means in a given case or context. Constitutional specification is essentially a matter of "political-moral judgment"; and since judges excel at such judgments (or at least do better than ordinary politicians), they might as well have a go at it.

Perry supports his argument with specific examples from the Fourteenth Amendment. Though the historical record is long and ambiguous, Perry has little difficulty deciding the original intention of its drafters was to ban discrimination against members of any group "if the group is defined, explicitly or implicitly, in terms of a

trait irrelevant to their status as human beings" or "if the group is defined, explicitly or implicitly, in terms of an activity, a way of life, or a set of beliefs-whether that activity, way of life, or set of beliefs be religious, political, cultural, etc.-towards which no state may, as a constitutional matter, express hostility" (pp. 130-31). This grand conclusion is defended in a total of two paragraphs; while plausible, it is hardly self-evident (no matter, though, since judges need only reach a "plausible conclusion" about the meaning of the Constitution). Then the real fun begins. Perry suggests that the Roe v. Wade was correctly (or at least plausibly) decided, albeit on the wrong grounds, while Bowers v. Hardwick, in which the Court refused to extend Fourteenth Amendment protection to gays and lesbians, was not. The result may not be compelling, but one must admire Perry's dexterity in getting there.

After reading Perry, it is easy to sympathize with Maltz's conclusion that modern constitutional scholar-ship has reached a theoretical dead-end. "Proceeding from widely shared but fundamentally misconceived premises," Maltz writes, "originalist and nonoriginalist analysts have been equally unsuccessful in creating intellectually satisfying models describing the appropriate role of judicial review in the American governmental system" (p. 108). In fact, the situation is bleaker than Maltz suggests, since his optimism that progress can still be made if scholars "abandon conventional wisdom and reorient their thinking in new directions" is undermined by his own argument.

Perhaps it is time for constitutional scholars to accept the fact that trying to justify judicial review from first principles is the political science equivalent of constructing a perpetual motion machine. The best that scholars can do is to sketch out plausible alternatives, solving nothing but giving judges of the Left and the Right intellectual support for their competing interpretive styles. Such a result might be good politics (Perry, in effect, can cancel out Bork); but in intellectual terms, all it does is make the waters even muddier.

Judicial review exists not because it is theoretically necessary or even because it is theoretically superior to any alternative. It is a kind of historical accident, the fortuitous by-product of colonial America's strong identification with the common law tradition and its invention of the written constitution as a way of limiting government power. It grew through careful cultivation over many decades by the Supreme Court and by the other branches, who found it an essential tool for the development of national strength and sovereignty. Above all, it flourished because it fit perfectly with what Robert McCloskey called "the deep-seated inclinations of the American political character," which refused to choose between the notions of popular sovereignty and fundamental law (The American Supreme Court [1960]).

The justification for judicial review thus lies in history, not in theory. And while the justices of the Supreme Court might benefit from following the academic debate, they would benefit even more from careful attention to the Court's past and to its ever-evolving role in the American political system. If Maltz is correct that Perry and the other theorists have failed to win their war, it may be because they have been fighting on the wrong terrain.

Clemson University

WILLIAM LASSER

Cooperative Pluralism: The National Coal Policy Experiment. By Andrew S. McFarland. Lawrence: University Press of Kansas, 1993. 219p. \$29.95 cloth, \$14.95 paper.

"What would happen if groups negotiated public policies among themselves, without relying on the state?" (p. 151). Probably most citizens, as well as many political scientists, would recoil with horror at the very thought: Allow the "special interests" to cut their own deals, with no representation of the public interest? Yet this is just the circumstance that Andrew McFarland explores in this book, and his conclusions are remarkably positive about the potential of such a process for enlightened policymaking. He is less sanguine about the political problems that can interfere with the implementation of such an approach.

The book is primarily a case study of the National Coal Policy Project (NCPP), a collaborative effort in 1977-78 between representatives of industry and environmental groups to forge national policy proposals on the mining of coal and especially to address problems associated with strip mining. The NCPP, brainchild of Gerald Decker of Dow Chemical, was extraordinarily successful in forging agreement among the potentially contending policies and in writing recommendations that were more than compromises—being, in at least some cases, innovative solutions. But the proposals for the most part failed to attract policy attention, for two basic reasons. First, the participants in the NCPP failed to involve governmental actors, either from relevant agencies or from Congress. Moreover, a window of opportunity did not exist for the recommendations, because of the passage of the Surface Mining Control and Reclamation Act of 1977 (viewed by most as a victory for environmentalists). The passage of this act did not resolve the matter, because many issues of implementation remained. But its passage, after extended controversy, did move the issue off the congressional agenda and made policymakers less attentive to the proposals coming from the NCPP.

More importantly, the proposals failed because the negotiators were unable to convince the organizations they represented to agree to the solutions in the report of the NCPP, entitled Where We Agree. The failure was particularly acute for environmental groups. Leaders from the Sierra Club had taken the lead in negotiations in the NCPP, but they were unable to convince the club board to support the measure. Once Sierra had opted out, no other environmental group wanted to take the lead and be accused of selling out. McFarland points to an interesting dynamic in public interest groups: they are unwilling to lose substantial membership (McFarland suggests a 20-25% figure) by endorsing a controversial, highly symbolic policy even though the vast majority of members are either supportive or indifferent. That leaves such groups hostage to extremists. Business interests were afflicted by some of the same problems (especially the recalcitrance of the coal-mine owners, in contrast to the more moderate stances of the major utility and chemical companies). But businesses firms are better able to move independently of industry associations.

Most of the book concerns just how the negotiators came to cooperate so productively. The key was the adoption of a set of negotiation principles developed by law professor Milton Wessel and going under the rubric of the "Rule of Reason." Although developed independently, these principles paralleled similar notions stemming from social-scientific study of negotiation and dispute resolution. Basically, the rules stressed respect for an opponent's position and the search for common interests. Most importantly, all participants began to appreciate both the need to pay attention to economic considerations and the need to protect the environment. A search for the facts and their relationship to a common framework replaced the advocacy approach that had characterized the relations among the parties. One of the most positive developments in the process was a major trade-off, in which the environmentalists were willing to move from a command-and-control model of environmental protection to an economic incentive approach that set public goals and allowed companies to pursue them through a variety of means, whereas the industry representatives recognized the desirability of public funding of public interest groups to monitor the process. (Remember that this was 1977-78, at the height of rancor between business and environmental groups.)

In this particular case, then, not only was the collaboration of producer and countervailing groups not a public policy nightmare, but it actually yielded enlightened proposals based on the search for common interests. This cooperative pluralism, although it failed in all but a couple of regulatory suggestions, nevertheless has emerged in a somewhat different form, regulatory negotiation. McFarland reviews several recent cases of regulatory negotiation and suggests the factors that promote success. Not surprisingly, perhaps the major criterion for success is the participation of the state in the form of a federal regulatory agency. The agency seems to play two roles: to encourage the sides to negotiate over the issue (which is generally narrower than the range of issues discussed by the NCPP) and to make the settlement stick by giving it the rule of law.

Cooperative Pluralism is a well-written, well-argued book with clear lessons for a critical issue of politics: What accounts for the patterns of division and cooperation among citizens and the groups that represent them? In the end, McFarland concludes that the narrowness of strategies pursued by groups and their contact almost exclusively with allies (as Heinz, Laumann, and Salisbury have shown empirically) limits the ability of the interest-group system to search for commonground. What is wrong with pluralism, then, is its inability to promote communication across interests; that is, in a sense, it fails to give enough power over to the groups to forge policy solutions. I may be pushing McFarland a little further than he would be willing to go, but I would note the difference between his analysis and the in-vogue attacks on the interest-group structure represented by journalist Jonathan Raup's Demosclerosis, essentially a rehashing of Mancur Olsen's Decline of Nations. Group-hating Raup and Olsen make few positive suggestions about improvement. McFarland's work suggests a far more positive line of reforms, some of which are being put into place (such as regulatory negotiation). I would like to make this book required reading for all the special-interest bashers and gridlock decriers, but I realize that they would rather live in their own cynical world than think more clearly about what would improve the situation.

Texas A&M University

BRYAN D. JONES

The President and the Parties: The Transformation of the American Party System since the New Deal. By Sidney M. Milkis. New York: Oxford University Press, 1993. 404p. \$49.95 cloth, \$19.95 paper.

Before the New Deal, American presidents were both empowered and constrained by their relationship to political parties, but modern presidents broke that tie. The modern presidency's increased autonomy from parties has been addressed in one fashion or another by important scholarship in the last several decades. Yet there is in the literature only partial or speculative explanations for the transformation from a party-based presidency to an institutionally autonomous presidency. In this excellent book, Sidney M. Milkis constructs a theoretically subtle and empirically rich account of that institutional transformation within the American regime.

Milkis treats the relationship of presidents and parties from Franklin Roosevelt to the present. For Milkis, the relationship of presidency to party is a lens for clarifying fundamental changes over time in American politics. In his analysis, the key development, triggering a complex system of changes, was the incorporation of a substantive policy mission into the institutional identity of the presidency. That change alienated the presidency from the parties and Congress and launched presidents into a search for an autonomous political base. In turn, Milkis argues, that search led to the creation of robust political resources for the presidency within the executive branch.

The vision of a reformist presidency was not new to the 1930s. The Progressive Era envisioned the president as a policy innovator, as evidenced by the president's changing rhetorical practice in that period. However, progressivism's visions of reform governance far exceeded the capacities for innovation and leadership of a party-dominated national government. To illustrate, Theodore Roosevelt was constrained by a Congress in which Republican "standpatters" dominated, and Woodrow Wilson could lead only so long as the congressional Democrats supported his initiatives. Indeed, more than Milkis recognizes here, the relationship between expanded administration and presidential power occurred to the Progressives, but their attempts at building increased presidential capacity were stymied by the parties and Congress.

It was the New Deal that fully associated the presidency with programmatic liberalism. Like his Progressive predecessors, Franklin Roosevelt ran up against the barrier of localist and fragmented political parties. So powerful was the reaction against Roosevelt in parts of the Democratic party by the late 1930s that conservatism overcame partisanship in the formation of the congressional coalition of Southern Democrats and Republicans.

Although Franklin Roosevelt won the 1932 presidential election in a realigning party victory, the Democratic party was not a consistent vehicle for programmatic liberalism. Roosevelt fitfully attempted to change the Democratic party, but his central effort was to fashion a base for executive power in the administrative state itself, separate from parties and Congress. Roosevelt created what Milkis terms a new, administrative constitution. Building on the work of New Deal historians, such as Barry Karl, Richard Polenberg, and Otis Graham, Milkis's analysis deepens our understanding of the so-called Third New Deal. He portrays Roosevelt as reconstituting presidential authority. While FDR was apparently beaten on several fronts in the late 1930s,

Milkis convincingly argues that Roosevelt's strategy succeeded in crucial respects. The president lost the court-packing fight and failed to get his reorganization bill in 1937 and 1938. However, by 1939, the Supreme Court had switched sides. The president also got a reorganization bill that gave him major new authority, although less than had been recommended by the Brownlow Committee. Equally important, Roosevelt succeeded in the alchemy of turning a key New Deal policy—the 1935 Social Security Act—into a personal right, protected from later politicians and parties.

Post-New Deal presidents were inheritors of the autonomous presidency that Roosevelt founded, and parties themselves receded in importance in the 1950s. Voting turnout declined after the New Deal realignment, which itself evidenced less intensity of electoral participation than earlier realignments. Additionally, presidential selection was increasingly personalistic, more dependent on individual candidate organizations than upon central party decision making. The advent of televised politics and the importance of primaries heightened the independence of presidents from parties. Thus, parties seem to fade from center stage in American politics.

Milkis warns that weakened parties and autonomous presidents have highly undesirable consequences for democracy. Expanded administration gave presidents more capacity and independence. Yet, as Milkis argues, presidents became politically isolated, dependent on their own organizational apparatus and their fleeting public appeal. Their incentives were to make their policy preferences safe from politics by converting government benefits to rights. The creation of public policies as individual rights, or entitlements, in turn, stifles the possibility of continuing public deliberation and debate about national priorities.

From Truman to Johnson, presidents found in the remaining liberal agenda and (one might add) the Cold War justifications for exercising power independent of party support. However, through his call for more participatory and less bureaucratic government and through his conduct of the national-security state, Johnson undermined the administrative constitution's legitimacy.

How did the administrative constitution fare with Republican presidents who were unencumbered by programmatic liberalism? Milkis shows that they also embraced the administrative constitution but for different ends. Facing hostile majorities in Congress, Republican presidents took the administrative constitution as a means for presidential leverage against the liberal policy establishment in a divided government. As Republican presidents turned the administrative constitution towards the end of redirecting government, they transformed its purposes. Milkis observes that with Richard Nixon it became clear for the first time that "the modern presidency could be characterized as a two-edged sword, which could cut in a conservative as well as a liberal direction" (p. 225). Yet even as Republican presidents embraced the administrative constitution, its legitimacy was sinking. Given divided government and in the wake of the collapse of the liberal agenda that undergirded the administrative constitution, presidential claims to power through it were received skeptically by Congress and the public, triggering new interest group and congressional activism to constrain presidents. Consequently, presidents were tempted to misuse their administrative capacities. Covert uses of executive power might shield the incumbent from public distrust of the administrative constitution but to would weaken the presidency over time.

Has the period since the New Deal, and the administrative constitution, permanently divided the presidency and the parties? Are there any conditions under which presidents and party might share a conception of governance? Milkis discusses the turn toward programmatic conservatism in the Republican party, but he does not consider the consequences of that development for an undivided Republican government, should one occur. It is conceivable that programmatic conservatism might unite what programmatic liberalism divided in American national politics. In the context of undivided government, Republican politics might produce something more akin to a Wilsonian presidential-party government than to the model of presidential autonomy that has formed the executive since the New Deal.

Whatever relations obtain for the presidency, the parties, and Congress in the next decade, *The President and the Parties* will be an important guide to understanding these institutional dynamics. Milkis has accomplished a superb marriage of political history and social science explanation, and his book offers a fundamental explanation of the institutional politics of the modern presidency.

University of Notre Dame

PERI E. ARNOLD

Corporate Political Agency: The Construction of Competition in Public Affairs. Edited by Barry M. Mitnick. Newbury Park: Sage, 1993. 327p. \$46.00 cloth, \$23.95 paper.

Mitnick's edited volume of 11 essays outlines a theory in which political activity by corporations is an important and legitimate part of democratic competition. There are two key concepts that pervade the work in the volume: the analytic concept of agency theory, which Mitnick helped create, and the normative value of contestability, used to judge the quality of democratic competition. While the notion of contestability is not quite as powerful (or as novel) as Mitnick would have us believe, this book sets a new standard in combining current research on practical corporate strategy with normative political science.

Democratic competition is conceived in two very different ways in the literature: (1) as a means for discovering truth and (2) simply as a way in which groups choose. To discover truth, the key component of democracy is open debate, with free access to the means of persuading others. In a New England town meeting, each citizen might be allotted an equal amount of time to speak, but some speakers are more persuasive in both their ideas and their rhetoric. Citizens might willingly yield their time to the articulate few they agree with, who can make better use of the scarce resource. Citizens might contribute money or labor to build a platform so others can see or buy an amplification system so others can be heard. As Walzer in Spheres of Influence (1983) and Graber in Transforming Free Speech (1991) have noted, the contribution of valuable resources may actually help illuminate the best choice.

On the other hand, if democracy is just a choice, any translation of economic power into political power is a

definitional deformation of democracy. Money is spent on amplification not just to reach all citizens but to drown out competing (perhaps more persuasive) voices. Platforms are raised not just high enough so that voters can see but tall and bauble-encrusted enough to attract the eye and dull the mind.

Forty years of social choice theory have lent credence to the "democracy reveals a choice, not the truth" position; yet the U.S. Supreme Court has held (most notably in Buckley v. Valeo of 1973) that in many cases money, even corporate money, is protected speech. The contributors to the volume pay lip service to the notion that democracy is just the way people choose and note that no overall preferences or goals can be attributed to the organic society that makes the choice. Nonetheless, Mitnick's proposed test of the quality of a democratic system reveals a solicitude for truth-value of the outcome: "The test of a democracy is not if its rules seem fair, but that its system is contestable" (p. 21). Political contestability is defined as "the extent to which a collective political actor or a system of such political actors possesses attributes, resources, positions, or other factors, in themselves or in their environments that promote the ability to compete effectively" (p. 12).

If this is the definition of a "good" democratic system, what are the policy implications? Contestability must either be an outcome-oriented criterion or else focus on exactly the fairness of the rules of competition. Mitnick does offer a criterion for judging outcomes by emphasizing agency theory, or the design of a contract that induces goals in an agent coincident with the preferences of a principal. But since Mitnick has recognized the social choice problems inherent in identifying a Rousseauian principal, there is no standard against which to compare outcomes.

On the other hand, if forced to fall back on the rules of process that dictate competition, Mitnick and his contributors rely on fairness, which violates the book's own canon for testing democracy. Mitnick invokes Rawls's A Theory of Justice (1971), noting: "Such reasoning leads to support for built-in protections for individual participants against persistent system bias. In essence, mechanisms are needed to keep the playing field fair and level" (p. 13, my emphasis). At best, Mitnick's definition of a good democratic system appears to rest on the distinction between rules that in and of themselves seem fair based on some ethically derived notion of justice and rules that really are fair, because it is possible for anyone with a persuasive argument (presumably, the truth) to win.

One wishes there were a recognition of the intellectual linkages between this position and more traditional political theory, particularly since Mitnick and his contributors are concerned with the political actions of corporations. Mitnick does recognize the difficulty, but relies much more on theories of the corporation (e.g., Epstein's The Corporation in American Politics, (1969) than on political theory. To be fair, Mitnick's extensive review of Epstein's famous book is revealing; and political theorists would find the arguments Mitnick derives from it to be powerful. Updating Epstein's thought and extending the analogy of economic contestability to politics, with the obvious implications for entry barriers and the sunk costs of reputation, is a valuable enterprise and one that Mitnick accomplishes well in his opening chapter. Though Mitnick's apparent aspiration to lay out "the core of what could be an interesting, explicit, generalized theory of competitive constraint" (p. 36) is not fully realized here, the argument he makes is an interesting and useful one. The perspective this volume offers is a very nice mix of the practical and the prescriptive: "We acknowledge that the playing field is tilted. We see elephants [corporations] among the chickens [voters]. But instead of throwing up our theoretical hands, we simply elect to call for better rules of the game for our playing field, but just the minimal rules necessary to make it level and give the chickens (and any other barnyard participants) an even chance" (p. 61). The mixture of the playing field and barnyard metaphors appears intentional. Perhaps Mitnick is suggesting that in dealing with the corporation in modern democracy, we should continue the game but watch where we step.

University of North Carolina, MICHAEL C. MUNGER Chapel Hill

The Learning of Liberty: The Educational Ideas of the American Founders. By Lorraine Smith Pangle and Thomas L. Pangle. Lawrence: University Press of Kansas, 1993. 350p. \$35.00.

This very important book is original, sweeping, and wise about the relation between education and liberal democracy in the United States. The Pangles have not written a history of education or an account of presentday policies, but they reconsider superior ideas from the founding period in a way that illuminates any serious thinking on American education, whether policy-oriented or historical. That is their intent. Americans, they suggest, have become "uncertain" as to "what the proper goals of education are" whereas the founding generation had to think through afresh such fundamentals (p. 6). While American schools have turned to dwell on useful skills, openness to other cultures, and amelioration of social problems, there is widespread concern about a certain emptiness as to "distinctly American, republican principles" and as to a moral education that might help the young "go beyond fleeting amusements to find lasting and meaningful happiness" in life (p. 7). There is even a widespread skepticism about whether such principles and aims can be known. To help in our uncertainties, even in reconsidering what is a "truly educated and well-formed human being" (p. 285), the Pangles turn to resurrect the educational proposals and reflections of Benjamin Franklin, Thomas Jefferson, George Washington, Noah Webster, Emma Willard, and many others. But the spirit is of reassessment, not of hero worship. They would "reanimate" the "living thought" of only those among the founders who spoke "most compellingly and clearly" on the topic, and they would subject it to "critical analysis and dialogue" (p. 6). The book is a mixture of dramatic American views with sympathetic but continuous interrogation, occasionally seasoned with the reflections of a Milton, Locke, Aristotle, or whoever.

The gist of the story is that education for a liberal order was problematic from the start, and the founders faced the problem. The modern Lockean tradition was not fully adequate to a democratic society, and the premodern educational traditions of classical republicanism and Christianity were thought largely irrelevant. Also, some of the founders' plans had their own difficulties, and a stingy and divided public was reluctant to fund or accept

projects however impressive. But the big story is the founders' resourcefulness. The Pangles discuss (1) plans for schools, textbooks, universities, philosophic societies, libraries, and almanacs; (2) efforts to shape religion, the press, politics, and the economy with a view to civic education; and (3) proposals for civic models to live by, such as the great Washington or the self-made man of Franklin's Autobiography.

The book displays a startling array of proposals and reflections, especially as to moral and civil education; and this is its most obvious achievement. There are fascinating accounts of Franklin's paradigmatic plan for a Pennsylvania academy, Webster's best-selling spellers and readers (laced with civics, including a "Federal Catechism"), Emma Willard's female seminary and her then-radical plan for training female teachers, and of course Jefferson's paternal plans for a whole system of

schools and the University of Virginia.

Amid the particulars, the authors clarify the importance and special difficulties of civic education for a liberal order, and this is a greater achievement. Statesmen like Jefferson and John Adams saw that American education could not be aimed at fostering Protestant zeal or Anglican tepidness; the manners of a chivalric aristocracy; or the strict, patriotic, and religious traditions of a Sparta or an Athens. Liberal democratic citizens were to be autonomous and humane-both liberated from reverence for tradition and enlightened as to the economic and governmental means of progress. But this modern outlook provoked its own problems. In general, the priority of civic education conflicted with the priorities of rights and of democracy. It is hard to evoke reverence for law when law, even constitutional law, is but instrumental to one's rights. It is difficult to foster citizen education by political participation, as Jefferson wished, if one would reduce the purview of government and thus remove incentives for participation, especially by more ambitious citizens. It is hard to balance popular vigilance regarding government and ambition with respect for government and for honorable patriots, such as a Washington. Jefferson raised the now famous wall of separation between church and state. But he also questioned whether belief in the rights of man could survive without their "only firm basis, a conviction in the minds of the people that these liberties are of the gift of God' (p. 193), and he recast Christianity into a humane and civil creed. The Pangles look for prudent "balance." In an enlightened republic, for example, concern for the press's rights should be accompanied by concern for its 'mission." While this mission is partly to act as watchdog guarding against governmental abuses, it is also and chiefly to act as an "auxiliary" of government in civic and moral education and an auxiliary also of the "learned" in making publicly appealing what in their thought is publicly useful (p. 223).

The highpoint of the book is its concluding discussion of education by models of emulation. The authors appreciate the moral heights of a Washington and the models of the common man set forth by Franklin and Jefferson. They are philosophically precise as to qualities and politically grave as to import. Washington was "an object of awe for all the founders" (p. 234). Many held him up, as he actually held and governed himself, as an inspiring personal example for military officers, future presidents, and in general the young who are potential leaders. The authors commend such examples. Perhaps the very core of moral education should be "the study of

rich biographies of individuals who exemplify different or competing versions of human excellence" (p. 287). Besides, sour-eyed cynicism corrodes respect for law, government, and leaders. The kind of model that Washington, in particular, offers "is what is most needed in the civic education of any republic, and especially that of contemporary America" (p. 248). The authors question scholarly reductions of the Washington's stern virtue to love of fame or to a need for public approval. They draw finally on Aristotle's description of the great-souled man. While they review sympathetically Jefferson's moral common man, they find grave difficulties in the crucial doctrine of the "moral sense." It is Franklin who thought out most deliberately and adequately a model common man who can mix independence with public service. Franklin is the "contested genius of American education" (p. 278), and the Pangles' clarification of the genius informing the Autobiography is superior to any other account known to me. Their tale is largely of greatness in the guise of humility. They bring out Franklin's mixture of self-interest, public-spiritedness, and devotion to science; the immense talents at story, ironic self-deprecation, and other arts of popular rhetoric; and, in general, the penetration and sobriety that can produce a moral teaching for the millions that also finds places for the superior. Yet while mostly rejecting the depreciations of Franklin by Max Weber and D. H. Lawrence, they conclude that Lawrence was partly right. Franklin's slyly comprehensive rationality lacks an appreciation of the soul's deeper longings for nobility and clarity and for a transcendence unbounded by human limits.

The Pangles think that general civic education is most obviously important (and especially slighted in our time) but not sufficient for judges, legislators, and other leaders. Nor, they believe, is civic education, common or uncommon, adequate for those drawn to literature, art, or philosophy. These thoughts lead them to clarify the various kinds of education in their American forms and places, the conflicts between them, and the priority and fragility of the most intellectual kind. Such clarifications are the book's highest and most probing achievement. The authors' appreciation of the University of Virginia, for example, concludes that Jefferson's curriculum was intellectually shallow partly because it was too civic. Jefferson gave short shrift to foreign languages and shorter shrift to literature (notably modern literature), dwelling instead on the natural sciences and government. He also excluded political texts alien to a states' rights republicanism, which led the university to become a school for Southerners and the Southern cause and excluded any "premodern or pre-Lockean political theory" (p. 173), especially ancient philosophy and other accounts of ancient republicanism. Jefferson was right to put civic education as a first and last duty of professors of government, the Pangles conclude, but he made civic education too partisan and curtailed philosophic education. He thus left students "rooted in thin soil" and lacking experience of the "zest for intellectual debate, and a consequent joy in learning and reading" that come with confronting "fundamental alternatives" (pp. 174-75).

It is striking that a book so filled with controversial judgments about education for politics and the politics of education is not "politicized" in the current sense. The book seems studiedly undoctrinaire, as if the authors intend to present a model of open and direct inquiry for those seeking a thoughtful yet imaginative path amid the fortified ideological camps of the present. They would educate for liberal liberty, but they are aware that this is a problem and are also aware of other plausible regimes and educations. They seek out some of the best proposals for American circumstances, quietly showing the self-contradiction and irrelevance of a generalized relativism; but they confront the diversity of proposals openly and take arguments for and against seriously. They show why a thoughtful citizen should take a certain stand on behalf of a liberal order, an informed and law-abiding citizenry, and honorable statesmen; but they also show the need for the impressive thoughtfulness of a Jefferson or Franklin and for a Socratic thoughtfulness that regards—seriously but not reverentially-impressive thoughts on politics and education. This book is a model of how to mix practical seriousness about American politics with philosophic seriousness about thinking things through. The style reflects the content. There are 14 chapters, any one of which contains more digested thinking about important American matters than most books; yet the whole text is only 288 pages. Pedantry and complacency are absent, and the reader is drawn into constant consideration and questioning. The Pangles are closer to educationalized politics than to politicized education; but a warm respect for the advantages of a liberal country and a cool awareness of its insistences and necessities keep them also from that extreme.

In a book so packed with controversial thought on controversial topics, there will be plenty of nits perceived and the occasional nit to pick. I wonder whether John Locke is not a bit tougher regarding the disciplining of children and a bit more radical in revolutionary indoctrination than the Pangles say and whether Francis Bacon is enamored of literature for its own sake; and it would have been useful to have had more comparisons with the key educational developments after the founding era. But as far as I can see, the Pangles have the big picture right (and often righter than any other scholarly picture) in every one of their 14 big discussions. To concentrate on nits would miss a superior rethinking of some excellent American political-educational formulations, in which they become again inspiring and instructive in our rather disheartened and confused time.

Boston College

ROBERT K. FAULKNER

Federal Policymaking and the Poor: National Goals, Local Choices, and Distributional Outcomes. By Michael J. Rich. Princeton: Princeton University Press, 1993. 432p. \$39.50.

For policymakers seeking to concentrate resources on the poor, the American political system presents a formidable challenge: federalism offers multiple arenas for arguing on behalf of the poor, yet creates numerous incentives and opportunities for diverting resources and spreading them more widely. Michael J. Rich takes up this central dilemma of American social policy in a multifaceted, empirically rigorous analysis of the Community Development Block Grant program (CDBG).

Rich does a excellent job of capturing the complexity of American policymaking in an analysis that stretches across levels of government and over time. He traces the development of the CDBG program at the federal level from its inception in 1974, showing how the distributional consequences of CDBG grants varied across administrations and noting the ongoing conflicts between Congress and the Department of Housing and Urban Development over how targeted the program should be. His study highlights the importance of examining implementation in the American context: as his cases show well, state and local decisions determine the ultimate impact of federal programs. Rich uses several different approaches to explore the effects of state and local decisions. He examines the influence of states using data from the small-cities program to show how states distributed resources. Local effects are examined in case studies of Chicago, several suburban cities, and two urban counties in the Chicago area. Rich analyzes data from these programs to show the patterns of targeting to poor neighborhoods in these diverse jurisdictions.

The analysis of the state-level programs is particularly valuable. Because states were given the option of controlling the small-cities program in 1982, Rich was able to compare the period of federal control with that of state control to test a long-standing question in American politics: Which level of government best serves the poor? His analysis demonstrates that while some states targeted more than the federal government, on the whole the federal government was more likely to direct funds to distressed communities. In addition, he provides a very interesting and well-executed analysis of the kinds of state institutional arrangements and political conditions that facilitate targeting to needy places. He shows that states with unified political control of the executive and legislative branches and little political competition achieved the greatest levels of targeting. The analysis also reveals that states that had designed targeted programs in the past were more likely to target CDBG funds (although where that initial propensity to

target comes from remains unclear).

The case studies of the Chicago metropolitan area highlight the variability of local decisions. The Chicago analysis shows that mayors can make a difference in how federal funds are spent. Harold Washington, Chicago's first black mayor, targeted funds to needy neighborhoods more than did his white predecessors and relied on community-based organizations more than did previous mayors. Unfortunately, Rich could not fully consider the effects of the return to a white mayor in 1989, since data were available for only the first two years of the new Daley administration. Rich's analysis of the suburban cities and urban counties is especially useful because we know much less about suburban politics than city politics. His data show that targeting varied widely in the suburbs. But perhaps most importantly, he demonstrates that even when suburbs did target, they focused on well-organized constituencies, such as the elderly and the disabled, rather than the poor. Rich restricts his analysis to six very affluent suburban cities. It would have been interesting to compare the distributional patterns in less-well-off suburbs as well, since the politics of these changing suburban areas remains largely uncharted territory.

Rich is at his best in nailing down the distributional consequences of policy. This book is full of careful, detailed empirical analyses clearly presented and effectively displayed. However, its treatment of the politics underlying these patterns is less fully elaborated. Rich uses the notion of benefits coalitions to discuss the political prerequisites for targeting, but we learn little about the conditions under which benefits coalitions in favor of targeting are likely to emerge and become successful. The discussion of benefits coalitions is not systematically linked to the analysis of the institutional features of decision-making systems; the appearance of these coalitions is essentially ad hoc.

One major policy implication flows quite clearly from Rich's analysis. Rich cogently argues that if our goal is to channel resources to the needy, a stronger federal role will be needed. Although federal pressure to target does not guarantee that money will be spent on the poor, it strengthens local coalitions that favor targeting and allows local officials to deflect pressures to distribute funds more widely. However, Rich slides over the important question whether it is better to use federal funds to aid people or places. He simply asserts that it is important to aid poor places, in addition to poor people. Because the CDBG program has proven to be such a "leaky bucket," it would be useful to know more about why we should continue place-targeting. Or, given the difficulties of targeting under block grants, wouldn't it make sense simply to return to the categorical grants of the 1960s?

These caveats aside, Policymaking for the Poor does an excellent job at its main task, which is to show how decisions at different levels of government affected the distribution of CDBG funds. Targeting remains a critical issue in American social and economic policy. Rich's work suggests that the federal government is now less likely to target funds to needy areas. As he shows, the population shift to the suburbs and the decline in urban political power have increased the pressures to spread resources more widely. And as President Clinton found out in 1993 when Congress defeated his request for a major increase in CDBG funding, charges of pork-barrel spending pose new political dangers in a climate of budgetary stringency. Rich's empirically rich and careful analysis illuminates just what is at stake in such decisions and offers us a set of tools for tracing their repercussions across the federal system.

Brookings Institution

MARGARET WEIR

Congress, the President, and Policymaking: A Historical Analysis. By Jean Reith Schroedel. Armonk: M.E. Sharpe, 1994. 240p. \$55.00 cloth, \$22.95 paper.

The study of presidential-congressional relations has undergone something of a renaissance in recent years, marked by a spate of books and reinvigorated arguments over the proper relationship between these two branches of government. This renewal of interest in a very old subject has been spawned by both an empirical and a normative concern: the empirical derives from dissatisfaction with the limitations of existing paradigms, and the normative from both an academic and a political debate (assuming a real distinction between the two) that variously wrings its hands over the impotence of modern presidents or the imperial excesses of intrusive congresses.

While some analysts have done a poor job at concealing their narrow partisan interests, the debate has for the most part been enlightened and enlightening. Into this fray steps Jean Schroedel, who seeks to tackle several particular aspects of the larger analytic and normative debate. She notes correctly several key prob-

lems, including the historical nature of much modern presidential-congressional analysis, its presidency-centrism, and its neglect of constitutional context. Schroedel's primary argument seeks to advance a notion of "dynamic constitutionalism," which attempts to "recognize the Constitution as a major determinant of legislative behavior" (p. 6). Quite simply, this translates into an acknowledgment that the Constitution sets the boundaries, parameters, and the broader political/institutional culture in which the governing institutions operate.

Schroedel applies this principle in both quantitative and qualitative analysis of all banking legislation (whether enacted or not) with which Congress dealt between 1823 and 1986. Her primary quantitative data set is 6578 banking bills spanning this period; her qualitative analysis focuses on three case studies. Although she carefully justifies her selection of banking regulation policy, Schroedel fails to note that the work of Lowi, Tatalovich and Daynes, and others has found that, at least in the modern era, presidents have exercised the least degree of political influence over regulatory matters in Congress, meaning that this policy area almost certainly represents the nadir of presidential influence in the domestic realm. Nevertheless, the long historical span does allow for viable longitudinal analysis, and her emphasis on analysis of all bills, rather than merely those enacted or those emphasized by presidents, sets this study apart from most others on the subject.

Complementing the historical perspective, Schroedel also scrutinizes vital elements of the legislative process too often ignored by those whose analysis begins and ends with roll calls, including the pivotal role of congressional committees and committee chairs, consideration of the full range of bills from minor to major, and careful scrutiny of decision making on the floors of the House and Senate, leading up to and including final votes. Schroedel's full view of the legislative process leads her to conclude that the overwhelming majority of bills (over 90%) proceed untouched by presidential influence. She does observe, and therefore confirms, familiar historical patterns of rising presidential influence and involvement, especially around the turn of the century and since the New Deal era. Still, she helps to deflate some of the exaggerated claims of presidential power that are too frequently repeated, and reassert the truth of James Madison's dictum that the legislative branch is still indeed the first branch of government—at least when it comes to the full array of legislative affairs. She also offers some insightful criticisms of such current theories as Peterson's tandem-institutions perspective, and Skowronek's political time view. Her effort to empirically verify Skowronek's perspective actually does more to buttress Walter Dean Burnham's parent theory of realignments, upon which Skowronek's work rests.

Schroedel's return to constitutionalism is welcome, but also ironic, because her use of the constitutional perspective is contextual, not textual. That is, her constitutional analysis is narrowly operationalized as political influence over congressional politics, as shaped for example by constitutionally mandated term lengths and differing constituencies of the president versus members of Congress. Missing is any inter-branch analysis based on statutory law, constitutional law, or administrative law. I do not seek to fault her analysis for failing to examine these matters (which is well done by such

scholars as Louis Fisher), but to note that an approach claiming to be "constitutional" can hardly carry such a label absent these or similar considerations.

The most engaging portion of the book is the three case studies. Yet Schroedel fails to use these cases to best advantage. The cases are described with great care, yet barely two pages are devoted to a synthesis of the cases, and their lessons seem to contribute little to the larger analysis. In addition, the three cases—creation of the postal savings bank (1873–1910), the depositors' guarantee fund (1886–1933), and the federal reserve act (1886–1913)—all span roughly the same time periods, if one considers the starting point the initial introduction of the bill, and the end point as the program's final passage. The empirical analysis could also benefit from more extended discussions of findings, significance, and implications.

Schroedel's analysis also underemphasizes the extent to which presidents take an interest in parochial political matters, especially as they pertain to the president's own re-election concerns, and presidential efforts to influence members of Congress. Similarly, she underestimates Congress's ability to grapple successfully with broader national and international policy concerns. The four central assertions Schroedel offers at the outset of the book as "testable propositions" are hardly that. For example, the "proposition" that "Presidents will need the support of key congressional actors if they are to see their initiatives enacted" (p. 12) is both self-evident and axiomatic. At best, these are assumptions about where analysis begins.

The centerpieces of Schroedel's analysis—its historical bent, and its sweeping look at all legislation within a policy category—represent an admirable and useful approach worthy of replication by other scholars. The prudently-imposed limitations of the study increase analytic feasibility, but decrease the study's generalizability, and her conclusions stretch well beyond her data base and chosen policy area. The empirical analysis is intriguing, but not conclusive. In sum, Schroedel has widened some important new avenues of exploration, but has left existing questions and controversies pretty much where they were.

SUNY, Cortland

ROBERT J. SPITZER

Burying Uncertainty: Risk and the Case against Geological Disposal of Nuclear Waste. By K. S. Shrader-Frechette. Berkeley: University of California Press, 1993. 346p. \$40.00 cloth, \$15.00 paper.

If the technological capabilities of humanity have outstripped our moral and political capacities to control and create our collective future, part of the problem may result from the fact that more research time and money is committed to the empirical study of environmental policy than to the normative ramifications of science, politics, and public administration. Most of the published literature relies on the axioms of economics and/or physical science as the "certain" foundation of policy analysis. But if our future requires sound and comprehensive collective choice that takes the normative dimensions of decisions seriously, then we must hope that there are scholars out there willing to push the envelope of empirical presumption—scholars, that is, like K. S. Shrader-Frechette.

This is a unique and exceptional book on many levels. It is a consummate case study of the Yucca Mountain High-Level Nuclear Waste Depository siting that not only criticizes current policy but suggests an alternative course of action. It is a detailed and revealing analysis of scientific assumptions and their implications in considering the geological disposal of nuclear waste. It is a fascinating study of the political and administrative strategies that have produced our current approach to environmental law. It is an ethical argument about the affects of science policy decisions on democratic government. It is a complete and timely examination of quantitative risk assessment. Lastly (but most importantly), it is a comprehensive normative study of an environmental risk policy that not only makes ethical arguments about nuclear waste disposal but also points out the normative character of the epistemological and methodological assumptions that science brings to policy decision making.

Environmental policy inevitably involves science. Anyone approaching a public choice regarding humanity and nature must consider the physical and biological ramifications involved. They are rightly assumed to be a critical component of proper policymaking. However, there is much written on the utility of scientific data and the question of policy as a "transscientific" phenomenon; and when one is operating within the uncertainty that is environmental risk, these questions become more critical to both *which* scientific assumptions are given credibility and *how* these are used in the formulation and analysis of policy alternatives.

If imitation is the highest form of flattery, the scientific community has been envied by many in the humanities and social sciences for the "factuality" and "certainty" of its "empirical" subject-matter. For decades, quantitative risk assessment has provided the science in the formulation of environmental risk policy and, for the most part, has been either accepted or rejected whole-cloth, by the public administrator or analyst, without a detailed inquiry into its underlying assumptions.

Throughout her book, Schrader-Frechette alternates between a general analysis of the presumptions of quantitative risk assessment and a specific criticism of the proposed Yucca Mountain High-Level Nuclear Waste Depository. The author contends, persuasively, however, that the entire policy argument for Yucca Mountain, including its science, is grounded in normative presumptions and arguments; and during this guided tour, one is amazed at both the narrowness of current thinking and the number of alternative assumptions and improvements in scientific inference that are accessible to the analyst in synthesizing a more complete policy argument.

The major focus of the book is, therefore, ethical and scientific value judgments. In terms of the normative values of science, the author takes many angles of criticism (priority of false positives or negatives, use of worst-case scenarios, de minimis inferences, linearity, begging the question, two-valued epistemic logic, the appeal to ignorance, acceptability of current magnitudes of risk, etc.) and makes her case in simple, concise and inherently logical terms. In addition, the author's discussion of moral values (e.g., intergenerational justice, consent, and the general reliance on preference based utilitarian moral principles when noninstrumental, non-preference values are at stake) is enlightening and shows that both types of values share degrees of "sub-

jectivity," "objectivity," and uncertainty, as well as inherent points of contention that must be debated within the context of fully integrated arguments. Together, the combination of these two avenues of normative criticism convince the reader that "facts" and "values" are both creatures of persuasive argument that (as demonstrated in the case study contained within this book), united in the right order and combination, can open doors to the comprehensive understanding of a policy issue—doors that were previously assumed to be either nonexistent or already shut.

In addition to its critical argument concerning Yucca Mountain, the book also contains an interesting constructive argument. The author contends that "negotiated, monitored, retrievable storage (NMRS) facilities" will "not misrepresent our scientific uncertainties or misappropriate our ethical burdens" (pp. 215, 251) and are preferable to permanent geological storage like that planned for Yucca Mountain. Although only one chapter is devoted to this constructive argument, it is adequate, given the critical argument that preceded it, to make the author's point. By the time one is finished with this book, its prescription seems eminently reasonable.

Overall, this book contains a complete and unique policy argument. It gives insight into the scientific assumptions at the foundation of current policy decision making and goes further to recommend a solution built upon the criticisms of current policy analysis. More than anything else, however, this book reveals the normative character of scientific inquiry and is an indispensable guide to the uses, abuses, failures, and potential of science in environmental policy decision making. If it did nothing else, it would be a seminal case study in the field of nuclear waste policy analysis and administration; but it does so much more.

University of Hartford and Vermont Law School JOHN MARTIN GILLROY

The White House Speaks: Presidential Leadership as Persuasion. By Craig Allen Smith and Kathy B. Smith. Westport: Greenwood, 1994. 288p. \$55.00.

The White House Speaks is another in Praeger's useful (but over-priced) Series in Political Communication. The series includes titles dealing with the media and public policy, the communication practices of citizen interest groups, the rhetoric of the cold war, presidential campaign films, journalistic ethics, and the modern campaign's emerging communication technologies (satellite dishes and the like). Many of the books in this series are genuinely thoughtful while others are not. Almost all, however, direct attention to the rather extraordinary intertwinings that have developed between governance and the communications industry. Some political scientists still cringe at the electronic Valhalla that has resulted, but with the first floor of the White House now devoted almost exclusively to communications staffers and with over 60% of all campaign expenditures now media-related, there seems little doubt that we need to know more about how political persuasion works.

The Smith and Smith book moves us down that road, but perhaps only a mile or two. In part that is a function of the book's genre—it is a collection of essays. Six of the nine chapters present individual case studies of the persuasive problems faced by the chief executive: Jerry

Ford telling the VFW's faithful that he will pardon draft dodgers; George Bush winning a campaign via the rhetoric of divisiveness and then losing a presidency with the same approach (the snarling Pat Buchanan and all that); Jimmy Carter explaining to the American people why they should be willing to give up their canal in Panama; Harry Truman orchestrating a war-time rationing program for the nation; Ronald Reagan justifying why he behaved so unpardonably during Iran-Contra; and Richard Nixon explaining why he erred, pardonably as it turned out, during Watergate. Each of these stories, and a few others, is retold by Smith and Smith with enviable clarity and, often, with a deft critical touch. But there is no getting around the fact that their assemblage is an assemblage, and that it causes one to lurch a bit unsteadily when moving, say, from John Kennedy's civil rights rhetoric to Jimmy Carter's meditations on the nation's energy supplies. Each of these stories is interesting in its own right but one still comes away from The White House Speaks a bit dizzy.

The authors seem to have sensed that possibility and so present in chapter two their "interpretative systems approach" to matters political. Ostensibly that puts the book in theoretical perspective. Actually, the chapter does little more than lay out the various terms and concepts the authors use when probing the texts on which they focus. The result is more an interpretive orientation than true systematicity. For example, the authors describe how political logics differ from philosophical logics and how both differ from rhetorical logics. They explain how political stories work and why propositionality often takes a back seat in modern politics to narratives-well-wrought (as wrought, say, by a Ronald Reagan). And the authors explain how the nation is made up of a variety of "interpretive communities" that consume political messages in exceedingly curious ways, although it is not clear if such communities are politically unique or epistemologically unique (the distinction is a not unimportant one).

At various places in the book the authors nicely deploy these exegetical notions but the critical system as a system rarely functions. When that fact is combined with the volume's chain-of-beads structure (here Roosevelt, there Clinton), the book's bookishness dissolves considerably. This leaves the reader pining for some sort of over-arching argument.

Nevertheless, the book has its attractions. The authors write in an unpretentious manner, their case-studies are intelligently chosen, and their criticisms are functional (although they probably contain too many right-angles for the poststructural reader). Moreover, the authors make some interesting claims. They note, for example, that unlike Ronald Reagan, Bill Clinton has often become the centerpiece of others' narratives rather than the master of his own. The authors display the extraordinary range of rhetorical duties now imposed on the chief executive (priest, prophet and king, in Michael Novak's memorable phrase) and they describe the rhetorical, not just emotional, utilities of political passion-it enforces a coherent narrative (as, for example, during the Panama Canal debate). And the authors explain how presidents often paint themselves into political corners via their semantic choices, as did Jerry Ford when making himself a too-central figure during the absolving of Richard Nixon.

The White House Speaks will serve especially well for the new student of presidential discourse. The advanced student will need more. One especially important need is to determine what sorts of case studies should be done and why. From a theoretical perspective, not everything a president says deserves book-length treatment and not every political situation has rhetorical importance. We need to know better which studies of political persuasion should be undertaken and which abandoned. We need smarter reasons for doing critical work and fresher arguments after having done it. We need to tap political theory more aggressively, asking what a Rousseau or an Arendt or a Rawls might say about the new communications revolution (we have already heard from Foucault on the subject). We need to be less cavalier when positing simple effects from discursive acts and we need to reckon in more complex ways with the concatenations between rhetorical and non-rhetorical forces in political life. We need to acknowledge that presidents persuade, but we need to treat that as a premise and not as a mantra.

Right now, too many bad questions are being asked about political persuasion and too many simple answers given. We need to resist both conditions but we must also be bold enough to take on the dauntingly complex questions raised by mediated politics. It is easy to be silly about a great thing, especially a great new thing like modern communications. But it is even sillier to leave the matter unstudied.

University of Texas, Austin

RODERICK P. HART

Media and Public Policy. Edited by Robert J. Spitzer. Westport: Praeger, 1993. 235p. \$47.95.

The mass media have been blamed for everything from erratic policymaking to biased and unfair coverage. According to critiques by Polsby, Hess, and others, common patterns of media coverage create several problems for policymakers. Most problematic is the tendency of reporters to emphasize "the game" over the substantive issues of politics. Far more attention is devoted to personalities and specific events than to longer-term policy development. This creates a faddishness to reporting that makes it difficult for policymakers to devise clear, comprehensive, and coherent policies. Taken to its extreme, this interpretation places a substantial portion of the responsibility for policy inaction squarely at the feet of the media.

Yet despite the widespread acceptance of this critique, there has been surprisingly little systematic analysis of this relationship. This volume edited by Robert Spitzer provides an important corrective to that state of affairs. Based on papers delivered at recent conferences, the book features contributions from 15 scholars, who address the link between the media and public policy. It should be required reading for every researcher with a serious interest in public policymaking.

The primary contribution of the book lies in its integration of media studies with the field of policymaking. As pointed out by Spitzer in his introductory essay, the two fields have developed independently with little cross-fertilization of ideas. Contributor Julio Borquez notes that leading models of policymaking (e.g., the garbage-can approach of John Kingdon) curiously downplay the role of the media. Alternative perspectives such as iron triangles and issue networks include no explicit recognition of political communications. The

result is that policymaking studies devote much greater attention to Congress, the presidency, policy entrepreneurs, political parties, and interest groups than to the news media even though the latter construct the interpretations that are so vital to the behavior of other actors.

In this book, the analysis of the media-policy nexus is organized in three parts. The first part provides a theoretical perspective by Doris Graber on the extent to which the media support or subvert the political status quo. Her conclusion is that there is evidence of short-term effect on individuals and groups but that absent sustained and saturation coverage, many factors limit the ability of the media to alter the status quo at the systemic level.

The second part of the book presents case studies of media impact on domestic policymaking. Particularly useful are analyses of Federal Communications Commission decisions from 1975 to 1990 by Wenmouth Williams, Jr.; media agenda-setting in regard to economic development by Michael Hawthorne; invasion of the right to privacy in a media-driven era by Dean Alger; media images of environmental policy by Jerry and Michael Medler; and the use of racial appeals in the 1990 Harvey Gantt–Jesse Helms Senate race in North Carolina by Montague Kern.

These case studies make the point that the media cannot determine outcomes but that by the manner in which particular interpretations are put forward and symbolic imagery expressed, subtle impressions can be fundamentally altered. In these ways, reporters exercise substantial sway over how policies are formulated.

The third part reviews studies of the media in a comparative perspective. In this section are pieces on source selection by Robert Sahr, AIDS reporting in the United States and Britain by Holli Semetko and Edie Goldenberg, South Africa reporting by Marion Just and Ann Crigler, and media influence on U.S. foreign policy by Patrick O'Heffernan.

The Sahr piece is a powerful demonstration of media power through the mechanism of "credentialing experts" (p. 153). By the types of individuals chosen as commentators and experts, reporters determine how stories get framed. As Page and Shapiro (1991) have demonstrated, these choices have consequences. Citizens view commentators and experts as more independent than many other sources and this credibility heightens the ability of experts to mold public reactions.

Researchers will also find fascinating the comparative coverage of AIDS in the United States and Britain by Semetko and Goldenberg. Using surveys of reporters in the two countries, the authors find dramatic differences in reporting on AIDS. Because American reporters function in a highly decentralized market, there was greater variation in press response to AIDS than in the much more highly centralized British media system.

This book has much to recommend it. Scholars will appreciate having, in one volume, a range of media case studies on timely subjects. Individual selections are well written and clearly organized. The authors have compiled informative case materials. For those seeking a review of domestic and comparative policymaking, this volume provides a plethora of material.

My only complaint about this book is its relative inattention to the structural revolution currently taking place in the media industry. The rise of cable, the use of satellite technologies, the 30-percentage-point loss of audience share by the three major television networks,

and the emergence of new networks is an enormous change from a system dominated by three networks and a few prestige newspapers.

Competitive pressures based on these structural changes are driving the media toward more tabloid coverage, and this has blurred the boundaries between news and entertainment. As a result, citizen confidence in the news media has been undermined. When the National Enquirer has the means to dictate coverage by the New York Times (as has occurred repeatedly in recent years), it is a dramatic sign that the core no longer dominates the periphery of the media system. We owe it to ourselves as scholars to determine how these important structural transformations will affect public policymaking.

Brown University

DARRELL M. WEST

Black Faces, Black Interests: The Representation of African Americans in Congress. By Carol M. Swain. Cambridge: Harvard University Press, 1993. 275p. \$37.50.

From Protest to Politics: The New Black Voters in American Elections. By Katherine Tate. Cambridge: Harvard University Press, 1993. 221p. \$32.50 cloth, \$15.95 paper.

Carol Swain and Katherine Tate introduce a great deal of important new data to the African–American/black politics literature and propose distinctive approaches to our understanding of black political values and political behavior. Swain considers substantive and descriptive representation in congressional politics. Tate examines black political attitudes and values as the group has shifted from forcing its way into the political system through collective protest and confrontation to coming to terms with the complexities of party, electoral, and institutional politics. Although they address comparable issues and use different methodologies, Tate and Swain reach considerably different types of conclusions primarily because of their distinctive philosophical approaches to the study of black politics.

Swain examines a political subject of great complexity—representation of racial interests—in her study of the processes by which black members of Congress are elected and by which they represent their constituents in congressional districts of varying racial proportions. She also reviews the processes by which black districts elect black or white representatives. She critiques the appearance of group-based orientations in vote-mobilization strategies, in drawing congressional districts, and in public policy formulation.

Tate reviews the philosophical and theoretical perspectives held by this constituency about electoral politics and public policy, which measure African–Americans' attraction toward, or rejection of, public life and value. Tate inverts the goals of her study from the more conventional examinations of the impact of blacks on American political life, concentrating instead on how their efforts in recent decades to integrate national politics and political institutions have shaped and reshaped their own political values. Tate analyzes data collected by the Program for Research on Black Americans (PRBA) at the Institute for Social Research at the University of Michigan. The PRBA conducted 1984 and

1988 National Black Election Studies in which "1150 voting age Blacks were interviewed by telephone prior to the 1984 national election, and 872 were reinterviewed prior to the 1988 national election" (p. 19). Another 473 were reinterviewed before—and 418 after—the 1988 election. Gurin, Hatchett, and Jackson's Hope and Independence (1989) is based on the 1984 survey.

Swain studies descriptive and substantive representation by assembling a series of profiles of black and white members who represent districts of varying racial combinations: majority-black districts (including historically and newly black, heterogeneous districts) and majority-white districts. She codes districts according to *contemporary* racial characteristics rather than the characteristics at the time the district first elected a black representative—which removes some districts from the majority-black category. This weights her findings in favor of her argument that black representatives do not face difficulty winning election outside of majority minority districts, more than is merited by the data.

Swain combined qualitative examination of the representative and his or her performance in office, both in Washington and in the district, with quantitative measures of their voting on redistributive and civil rights issues for the 100th Congress (1987-89). In the development of her qualitative materials, Swain the researcher is the most important instrument in evaluating the members as she assembles biographical and political profiles, describes their districts, evaluates their political strategies, reports on their performance in the Congress, judges their overall effectiveness in representing their constituencies, and comments on civil rights activists' choice to design black districts. Swain's profiles vary quite dramatically in tone. The variance in sentiment seems to arise from several sources: whether the members granted her an interview and the opportunity to observe them at work, whether they represent majorityblack (especially historically black) districts, and whether they aggressively seek white electoral support. Swain is also quite sensitive to class issues: she is critical of representatives she perceives as middle- or upperclass, although her affect toward them seems somewhat mediated if they make strong appeals to white voters. She concludes that black representatives from majorityblack districts do not work as hard at representing their constituents as do whites or blacks from majority-white districts but offers no hard measures to substantiate her conclusion.

It is unlikely that any researcher could avoid some personal reactions to the subjects under study, but Swain clearly has a preference for certain types of racial political strategies. Any representative who chooses these strategies (e.g., direct appeals to white voters, especially in a non-majority-black district) is praised. Any representative who appeals strongly to black voters in a majority-black district is critiqued.

She is also highly critical of majority-black districts, a strategy adopted by the voting-rights bar to increase black opportunities for descriptive representation. Swain even argues that majority-black districts have an independent impact on crime rates: "Large concentrations of poor black voters . . . are often plagued by high crime rates" (p. 203). Such districts have recently become a subject of increasing controversy and deserve careful review if they are proposed as the sole strategy for improving black political representation. Swain con-

cludes that the election of black representatives from nonblack districts is proof that majority-black districts are not a prerequisite for blacks' election and that whites do not vote in racially polarized fashion to defeat black candidates. In fact, her case studies illuminate the considerably greater complexity of electoral contests when race is involved, a factor that affects both the success ratio of black candidates and the seriousness with which candidates debate policies of interest to black voters.

There has been much speculation that the development of greater class differentiation among blacks would lead to growth in political conservatism and in support for the Republican party. Neither Tate nor Swain finds evidence for this. Tate also shows that socioeconomic diversity has changed and made more complex—but not eliminated—the importance of race, civil rights, and group identification (group fate) for African-Americans: "Fully 75% of Blacks in the survey felt that what happens to Black people will shape their lives" (p. 25). Upward mobility has a "mixed effect on one's racial identity, simultaneously undermining it and reinforcing it." The college-educated therefore have the strongest common-fate identity: those with manifest middle- or upper-class identification scored less strongly, while income was statistically insignificant. On policy measures, socioeconomic status "strongly affected Blacks' policy attitudes" (p. 46), with the higher status less likely to support higher spending on social welfare, food stamps, public schools, and medicare. Blacks are also much more likely to be conservative on public morality issues. Yet Tate concludes that only 10% "may be" economic conservatives (p. 48).

The norms of empirical research, at base, require the researcher not to allow personal values to confound the findings. Swain's work has this fault, much of which has to do with the way she decontextualizes her research. She also makes highly critical, even pejorative, statements and other observations, absent adequate documentation or substantiating background information (pp. 90–91, 171). Researchers of African–American politics must understand the historical context within which African–Americans function politically in order to have a grasp of their distinctive values and uses of politics. That context is much broader than conventional American political science, as Lucius Barker noted in his 1993 presidential address to the American Political Science Association.

Swain contextualizes her analysis of the performance of African–American and other representatives in their districts, solely within the political science literature on Congress and on voting behavior. Tate's study clarifies how one can integrate these dimensions. It is thoroughly grounded in the political science literature, and it explores the historical traditions that have distinctively shaped black political attitudes and behavior. In chapters on group resources and on black power, for example, Tate describes how African–Americans managed and organized their "politics" in a society that prohibited democracy across racial lines.

Tate's analysis of blacks and the Democratic party provides an invaluable introduction to the importance of race in the national party's politics after the 1930s. She also identifies the independent role that black political activists took in forcing the party to reorganize its policy agenda and political appeals from the 1960s through the 1980s, thereby challenging Carmines and Stimson's treatment of that subject. In her analysis of the 1984 and

1988 data, she found few significant differences in the policy attitudes of black Republicans and Democrats; and blacks were about equally likely to be Democrats, regardless of income. Gender and age are significant differentiating factors affecting partisan identification, with men and younger cohorts more likely to be political independents or Republicans. Tate shows that the Jackson campaign had a conflicting impact on blacks' relationship to the party, with increases in blacks' efficacy and position within the Democratic party and in their dissatisfaction and distrust after Jackson's loss at the conventions. However, because of the sharp rightward movement of the Republican party during the Reagan presidencies, Tate found no appreciable change in their interest in the Republican party.

By contrast, Swain's critical treatment of a series of members of the Congressional Black Caucus and of the caucus as a group addresses the historical configuration of black political life subjectively. Black group identification is normative and problematic, rather than an empirical issue. Swain treats any appearance of groupbased mobilization and black nationalism as aberranteven immoral-behavior, while white racial attitudes are not a major factor. She misunderstands the significance of group identification for substantive policy in previous literature on the caucus: "Marguerite Barnett's indictment is most severe. . . . The caucus is failing in its mission to deliver the types of legislation most needed by blacks" (p. 38). Barnett, in fact, argued very differently from Swain's central thesis: "The Congressional Black Caucus, in order to be effective within the electoral context, would have to understand and directly attack the structural conditions of black subordination. Otherwise, structural constraints will continually undermine seeming advances won through electoral politics and incremental strategies" (Barnett 1982, 52)

If there are weaknesses in Tate's study, they fall within her chapter on black power and electoral politics, which is somewhat cursory in its treatment of black power. That chapter nevertheless contextualizes the experiences and efforts of black elected officials who were learning as a group, for the first time in the 1970s and 1980s, how to represent. The creation of an electoral cohort where there had been none before generated a complex set of new political issues for blacks, individually and collectively.

Tate's finding that support for black nationalism increases as conditions within the African–American population deteriorate helps explain the variables that shape its strength over time and within the population at specific points in time. Nationalism therefore has greater strength among poor blacks and the less-educated and has increased as socioeconomic conditions have worsened in recent decades. Those who strongly express nationalist views are also less likely to participate in electoral politics.

Tate and Swain conclude their studies with discussions of similar subjects (the problems of leadership within the black population, nationalist issues, and conflicts over majority black districts); but the overall framework within which they place these issues and the objectivity with which each approaches her subject are profoundly distinct. Swain reports on what she thinks ought to happen—that blacks need not limit their political campaigns to predominantly black locations and that whites can represent African—Americans even more effectively than blacks. She reaches conclusions not fully

merited by her data and simplifies a very complicated political phenomenon. Tate balances empirical social science with historical perspective. She clarifies that while the black population has moved from protest to politics, racial self-identification is still strongly valued within the group; and dissatisfaction with social, political and economic status crosses class and party lines. The African-American population is in no sense undifferentiated: variations in levels of education, economic status, gender, and age reveal important partisan preferences, attitudes towards electoral politics, and support for Jackson. Protest and racial politics will therefore continue in national life. African-Americans and all other Americans have just begun to learn the difficulties involved in balancing descriptive and substantive representation.

University of Illinois, Urbana DIANNE M. PINDERHUGHES

The Making of American Exceptionalism: The Knights of Labor and Class Formation in the Nineteenth Century. By Kim Voss. Ithaca: Cornell University Press, 1993. 290p. \$39.95 cloth, \$16.95 paper.

In her new book, Kim Voss, an historical sociologist, addresses a hoary issue in U.S. historiography, the theme of "American exceptionalism." Simply put, the concept of "American exceptionalism" posits that the United States, unlike all other industrial societies, has lacked a working class conscious of itself and hence capable of advancing its interests through a mass socialist or labor political party. Voss instead argues that until the end of the 1880s workers in the United States, England, and France followed a common path "as activists in each country built new inclusive unions and endorsed new radical creeds in reaction to similar fundamental transformations in the organization of work and social life" (p. 234).

Voss's book consists of two parts, the first an overview of the history of workers and labor movements in nineteenth-century England, France, and the United States; the second, a case study of the Knights of Labor in New Jersey constructed from a statistical analysis of 323 local assemblies. Voss uses the research of labor and social historians to demonstrate that from the initital emergence of market-driven capitalist societies in England, France, and the United States until the end of 1880s, workers in all three nations acted similarly. During what she characterizes as the first moment of class formation, the era between the 1820s and 1840s, skilled workers, who were exploited by their masters, built craft unions and political protest movements. The subsequent growth of industrial capitalism diluted artisanal culture and its craft unions. In what Voss characterizes as a second moment of class formation between the 1840s and 1880s, semi-skilled factory operatives came to dominate the labor force and the further development of trade unionism depended on the ability of skilled workers to ally with the less skilled through new forms of organization. According to Voss, the "great depression" of the late nineteenth century resulted in "a significant convergence of experiences and grievances across all sectors of the working class" (p. 70). She suggests that in the 1880s English workers created a "new unionism" that drew less skilled workers into a labor movement inclined toward socialism; that French artisans organized their nonskilled compatriots into militant community-based labor federations that adopted revolutionary syndicalism; and that American workers built the Knights of Labor to amalgamate the skilled and less skilled for common economic and political action on the principle of "working-class republicanism." Thereafter, labor's trajectory diverged in the three nations. In England and France the "new unionism" and radical working-class politics survived to promote the British Labour Party and French syndicalism; in the United States, however, the Knights of Labor collapsed, and was replaced by the anachronistic craft unionism and antipolitics of the American Federation of Labor. The failure of the Knights, Voss suggests, "marks the moment when, from a comparative perspective, the American labor movement began to look exceptional" (p. 235).

Voss uses the history of the Knights in New Jersey to prove her hypothesis concerning the origins and timing of "American exceptionalism." Working with local newspapers, state and federal reports, census data, and a variety of local labor history archives, she constructs a composite portrait of industries, cities, and the Knights of Labor in New Jersey. Her data-gathering and sophisticated statistical techniques proved to my satisfaction that the New Jersey Knights was an organization that recruited few non-working-class members; that its skilled workers acted as spark-plug unionists to recruit their less skilled brothers and sisters; that the Knights organized primarily on the basis of industry and craft rather than as mixed assemblies; and that the organization was strongest in larger, more diverse, and economically advanced cities where interactions among heterogeneous workers fostered a community-based consciousness. Voss's portrait of the Knights in New Jersey renders them comparable to the British "new unionists" and the French Syndicalists in their desire to liberate workers from wage slavery. What distinguished U.S. workers and their labor movement from their European analogues was not the singularity of American historical development (exceptionalism) but rather the inordinate power and antiunionism of U.S. employers. More so than any previous scholar of the Knights, Voss demonstrates that neither organizational paralysis, weak leadership, the antagonism of craft unionists, the ethnoreligious diversity of workers, nor the alleged utopian middle-class character of the Knights caused its failure. Examining a struggle between employers and Knights in Newark in 1886-87, Voss shows how the rising strength of labor prompted employers to form an association that precipitated a conflict with the Knights during which public authorities helped capital crush labor. What happened in Newark, asserts Voss, was writ large across the nation wherever and whenever employers organized to fight the Knights. Having lost the class struggle against employers during the second moment of class formation in the United States, the nonskilled workers found themselves bereft of organization while their skilled brothers retreated into defensive enclaves of conservative, antipolitical craft unions (the AFL), thus establishing the conditions for "American exceptionalism."

Voss's portrait of the New Jersey Knights, their history, and the causes of their failure rings true. Her analysis of "American exceptionalism" raises questions. Like many other contemporary historians, I find "exceptionalism" a tired concept, one that explains both too

much and too little about U.S. history. Like Aristide Zolberg, I believe that every national history is in part "exceptional" and that there is no single historical model against which to measure a nation's development and declare it wanting. I think that Voss also exaggerates the differences between French and British labor, on the one hand, and American labor, on the other, after the collapse of the Knights. The "new unionism" in Britain remained relatively weak until World War I while the British Labour Party and socialism scarcely attracted most workers. In France, unionism remained feeble and most semi-skilled workers lacked unions until the 1930s when France experienced its own labor upheaval during Leon Blum's popular front version of a New Deal. My reading of French history suggests, moreover, that employers and the state were as antilabor as their U.S. counterparts. Indeed, between the 1880s and 1920, the U.S. had a more powerful union movement than France, and a socialist movement at least as large and influential as those in France and England. A final aspect of Voss's comparative approach troubles me. I understand her choice of England for purposes of comparison. But why France rather than Germany, when the latter nation, according to most economic and business historians, had an industrial history and structure most like that of the United States and when it is conventionally considered the model for socialist and trade union development from which other more "exceptional" nations allegedly deviated?

Binghamton University

MELVYN DUBOFSKY

Regulation in the White House: The Johnson Presidency. By David M. Welborn. Austin: University of Texas Press, 1993. 354p. \$45.00 cloth.

David Welborn's discerning examination of the ways in which President Johnson conducted his "business in regulation" sheds much needed empirical light upon the translation of presidential power into regulatory behavior, from the public image projected by a board or bickering commissioners, to the substantive details of international airline routes and residual oil controls. Welborn's analysis provides a crisp picture of one president's perceptions of his regulatory responsibilities and the context within which he operated, defined (somewhat oddly) by time constraints, political necessity, an ambitious political agenda, and a hefty respect for the autonomy and competency of the independent regulatory commissions. Yet Welborn's concentrated effort to study regulation in the White House yields insights into the institutional powers of the presidency that extend beyond the business of regulation, and that illustrate a president's understanding of the public interest, his mandate to serve, and the implications for exercising presidential authority.

Welborn's study builds from a simple premise: The president has inescapable regulatory responsibilities related to his role as chief executive, as a leader with a legislative agenda, and as the designated authority for deciding particular substantive regulatory matters (pp. 4–6). Welborn asks, in exercising these various responsibilities, "what do presidents actually do and how do they go about doing it?" (p. 256). He argues that the business of regulation in the Johnson White House was motivated by three objectives: 1) to prevent harm that

regulatory activities might bring to the administration; 2) to "enlist regulatory assistance" to facilitate the administration's "basic economic policy objectives," and 3) to pursue "consumer, health, safety, and environmental protection" by expanding social regulation (p. 21). These objectives were pursued in a manner that "confounds the conventional wisdom about presidents and regulation and also about Johnson the president (p. 19). Welborn documents a White House actively involved in managing its regulatory business at a variety of levels, influential in its ability to affect regulatory outcomes, and a president whose depth of substantive and political knowledge of regulatory affairs provided ample means to facilitate a broader political agenda. Regulatory business, in other words, was not an end in itself for Johnson, but a means to broader ends.

Following an introduction providing an overview of the institutional and political constraints affecting presidential management of the regulatory process, and a profile of LBJ's leadership style, the book probes Johnson's regulatory role in three contexts. Chapters 2 and 3 focus upon the independent commissions. An examination of Johnson's strategic use of the appointment process, and the "linkages" between the White House and the commissions on a day-to-day basis provides a convincing argument for the "balance" Johnson tried to achieve "between agency and presidential business," and between an activist White House seeking cooperation and teamwork with the commissions, and the need to respect commission autonomy (p. 25). Regulatory activity within the executive branch is examined in chapters 4 and 5. In contrast to Johnson's "arms length" rule in dealing with the substantive agendas of the commissions, the White House played an integral role in the regulation of oil imports through the Department of Interior, commodity prices through Agriculture, and the coordination of banking supervision through Treasury. Further, these regulatory leaders readily assumed the roles of "advocates," in contrast to the expectations for commissioners. The direct links between these areas of regulation and economic performance (particularly the effort to hold down inflation), brought the White House into the substantive details. In matters of antitrust (chapter 5), however, the ties between the White House and the Justice Department, as well as the perceived connection between antitrust policy and economic performance, were more "tenuous." Chapter 6 examines the administration's legislative agenda, particularly the ambitious expansion of social regulation. The final chapter categorizes Johnson's regulatory involvement across the three contexts as "high," "moderate," or "low," and examines the presence of three factors in individual cases that tended to draw Johnson into regulatory issues: "duty, compulsion, and disagreement" (p. 261). When the president had a de jure or de facto "obligation to act," a compulsion to act for furthering his (political) interests, and there was disagreement between the White House staff and an agency, for example, Welborn argues there was a likelihood of high presidential involvement in the issue.

Welborn's use of the memoranda and working papers of the Johnson White House provides direct links between presidential priorities, the use of presidential influence, and (often) the shift in regulatory behavior—at times substantive, at times stylistic. Yet it is precisely his very detailed empirical work that would lend itself to a much richer and more generalized anal-

ysis of the institution of the presidency as an influence upon regulatory activities, in particular, but the administration of the executive branch, more generally. This reviewer was hoping for a more systematic return to questions raised early in the book about presidential influence over regulatory behavior, versus that of regulated interests and the Congress. What does the Johnson experience tell us about the limits of presidential influence, and are those limits personal (related to style and agenda) or institutional? If the authority is there to be exercised, what determines its restrained use? The complicated debate over "who controls the bureaucracy" (long fought on the field of formal models) could be enriched by an analysis such as Welborn's that demonstrates the exercise of influence as well as its limitations. Further, how does Johnson's approach to the business of regulation contrast or parallel the approaches of more recent presidents, particularly Reagan? Was Johnson's use of the BOB, and his formula for regulatory appointments a precursor to the more centralized and systematic approach to regulation taken by later presidents? A second, minor criticism is Welborn's hesitancy to elaborate upon Johnson's governing style, such as his willingness to buck the demands of "special" interests in the attempt to facilitate fairness. We often argue that presidents are motivated by their more broadly based political coalitions in their conduct of executive branch business (in contrast to the parochial concerns of Congress), but a systematic investigation of their actions in trying to serve that broader interest (particularly when confronted by powerful, more particular interests) is scarce. Welborn provides some insights, although without elaboration.

This discussion about the potential for extending Welborn's analysis should not take away from his important contribution to the study of regulation and his vivid presentation of the very personal and political management of regulatory affairs that is possible from the White House. The book is thorough in its coverage, consequential in its findings, enlivened by its empirical base (in part, the quotes and commands of LBJ on regulation), and a must-read read for anyone interested not only in the business of regulation, but in the institution of the presidency as a tool for managing, directing, and influencing bureaucratic behavior.

University of Wisconsin, Madison Anne M. Khademian

From Outrage to Action: The Politics of Grass-Roots Dissent. By Laura R. Woliver. Urbana: University of Illinois Press, 1993. 195p. \$36.95.

Laura Woliver analyzes four Wisconsin community controversies that transpired between 1977 and 1984 when citizens reacted angrily to perceived failings in the criminal justice system. Two of the controversies stemmed from egregious remarks made by local judges about the victims of sexual assault. Dane County Judge Archie Simonson blamed sexual assault on the provocative way that women dressed. He was swiftly ousted in a recall election. On the other hand, Grant County Judge William Reinecke narrowly survived a recall election after suggesting that a five-year old girl had made sexual advances toward a man who molested her. Woliver's third case revolved around the death of an African-American man, Ernest Lacy, who was beaten by Mil-

waukee police officers and died while in their custody after being arrested on suspicion of rape, a charge that later proved unfounded. Following sustained community pressure, five Milwaukee police officers were either fired or suspended for their part in the Lacy death, and the Lacy family eventually won financial compensation through settlement of civil suits they filed. The fourth case of collective action that Woliver studies was motivated by the abduction and murder of a ten year old girl named Paula McCormick by a man who had a history of criminal conduct, including an earlier conviction for sexual assault. Community pressure placed discussion and study of the criminal justice system on the agenda and prompted meetings between public officials and citizens on this subject, but did not produce many concrete changes.

Woliver's book adds usefully to our catalogue of studies of community activism. In tracing each case, she supplements newspaper accounts with material gathered from in-depth interviews conducted in 1984–85 with leading participants. She chronicles the development of the movements; discusses how the incidents were portrayed in the media; analyzes the process of issue definition, agenda setting, and coalition formation; reports the views and reactions of participants; compares the different outcomes of the movements; and reflects on the impact of political activism and the ability of ordinary citizens to be heard. Throughout, Woliver makes an admirable effort to apply to her cases insights drawn from a wide range of literature on social movements and political activism.

There is, unfortunately, not always a clear summary of the events surrounding each of the cases. Sometimes the facts of the precipitating event trickle out in the course of her analysis, instead of being presented up front. The most serious instance of this occurs in the chapter on the Simonson recall, where crucial details of the Judge's remarks during the sentencing hearing that instigated the public furor are not reported in the introductory sections, but rather more than half way into the chapter.

In the Simonson and Lacy cases, the activists were largely successful in reaching their objectives. Woliver argues that community activists in those instances were able to draw respectively upon the broader feminist and civil rights communities in support of their goals. The Simonson recall effort drew heavily from experienced women's organizations (including the National Organization for Women) that were centered in the liberal Madison university community. The Lacy protests combined dozens of existing political, social, and religious organizations that banded together in a broad coalition. Both movements successfully tied the local incidents to the larger feminist and civil rights agendas.

In contrast, the organizers in the less successful Mc-Cormick and Reinecke cases appear to have been less imaginative political entrepreneurs. They were unable to frame the controversies using the kind of general ideological language that would have activated wider support for their cause. Woliver contends that their inability to ground the movements in an established activist community not only undermined political mobilization, but made it harder for those who did participate to receive the social support they needed to retain their convictions. In addition to contrasting degrees of organization, there were several mundane political reasons why the Simonson recall succeeded whereas the Rei-

necke campaign didn't. The Reinecke recall election was closely contested, but Reinecke survived because he ran in a more conservative county and there was no strong alternative candidate to him as there was in the Simonson recall. Simonson also had a more checkered past record that added to his vulnerability, in contrast to Reinecke who was more highly respected by the local elite, including the legal profession. The evidence also indicates that Reinecke's misconduct was judged by opinion leaders and the public to be less serious and offensive than Simonson's.

A few activists involved in the Reinecke and McCormick controversies told Woliver of their bitterness over their experience with local politics. The McCormick activists felt that politicians had given them the runaround. Those who worked to remove Reinecke recalled facing hostility and accusations of political extremism, which stung all the more in the small town atmosphere of Grant County. Perhaps the inexperienced anti-Reinecke forces were naive to think that they could wage their campaign without generating counterattacks from the opposition, since the attacks on them were not unlike some of the charges hurled at the leaders of the Simonson recall. One of the main conclusions Woliver draws from her analysis of the four movements is that some interests will remain unorganized because individuals fear social censure in communities where they feel outnumbered. While this must often be true, neither the Reinecke or McCormick case appears to bear heavily on this point. Although the Reinecke campaign was hard fought, with harsh rhetoric used by both sides, it does not seem from Woliver's account that the local majority "silenced" (p. 138) the dissenters as she claims in her conclusion. It is difficult to see how the activists could have felt unduly isolated and stigmatized given the relative ease with which they gathered thousands of signatures for the recall petition and the well-publicized calls for Reinecke's removal issued in major state and national newspaper editorials. As it happened, the activists came very close to winning their campaign and, even in defeat, appear to have modified the Judge's subsequent behavior on the bench in a manner to their liking.

Similarly, I was not convinced by Woliver's claim that the activists in the McCormick movement had been "hushed" (p. 111) by insincere politicians. Organized protest was stalled because there was no obvious culprit in the criminal justice system who could be fingered and held responsible for the girl's murder. Local politicians may have, as Woliver claims, stalled the activists with meetings, symbolic gestures, and speeches, but the activists were also sobered by the difficulty of trying to

reform a criminal justice system that they only vaguely understood. Not all was lost by any means. Participation in the local movement allowed people to show solidarity with their neighbors in expressing outrage over what had happened in their community. Woliver speculates nevertheless that the negative experiences of the few activists she interviewed might discourage any future endeavors, but this seems an overly pessimistic conclusion if applied to the entire movement. One wonders what would have happened if there had been a second McCormick case on the heels of the first. Quite possibly the coordination of protest would have been more rather than less likely given the networks created and lessons learned in the original case. A factor that primed participation in the campaign to recall Simonson, after all, was the recent occurrence of several rapes in Madison, which had made sexual assault a topic of discussion in the media and the public and thereby heightened sensitivity to Simonson's comments.

In general, I was surprised that the interviews did not seem to play as central a role in the analysis as I imagined they would. They provide insight into the feelings and perceptions of activists, but they are not used to make any particularly telling theoretical points. Although it is reported that Judge Simonson was interviewed, I could not find reference to that conversation in the chapter on the case. Also, the small number of interviews conducted were concentrated on protest leaders, leaving us to speculate about the motives and reactions of the more casual participant. One issue raised in the Simonson controversy, for example, was that the activism was motivated partly by racism, given that the victim was white while the accused juveniles were black. The only evidence offered against this proposition is the denials of recall leaders. Similarly, the handful of leaders interviewed from the Reinecke campaign were disillusioned by the rough treatment they received, but was their reaction shared by the average participant who was not on the front lines of the dispute and who, therefore, may have enjoyed a more favorable cost-benefit calculus? In contrast to the results from her interviews, newspaper reports following the Reinecke recall election cited by Woliver (p. 137) indicate that some participants were content with having given the Judge a scare. On these matters, the in-depth approach, while providing valuable detail and a unique perspective, may actually have been too close to the action, sacrificing representativeness and obscuring more general observations and conclusions.

Northwestern University

DENNIS CHONG

COMPARATIVE POLITICS

God's People: Covenant and Land in South Africa, Israel, and Ulster. By Donald Harman Akenson. Ithaca: Cornell University Press, 1992. 404p. \$29.95.

Unsettled States, Disputed Lands: Britain and Ireland, France and Algeria, Israel and the West Bank-Gaza. By Ian S. Lustick. Ithaca: Cornell University Press, 1993. 576p. \$37.50.

How are we to explore cases of intransigent conflict in societies where ethnicity, race, and religion are, at the core, group differences? Theoretically rooted comparison is increasingly relied upon by scholars, political activists, and conflict resolution practitioners in thinking about what were once understood as a series of unique situations. The two books under review here are interesting in that despite the fact that Ireland and Israel are two of the three cases that each treats, the specific theories driving the comparisons are very different. As a result, one sees how different theories, when applied systematically, lead to very different—but not necessarily incompatible—insights.

Akenson, a historian, focuses on the role of what he calls "covenantal thinking" as it has shaped the political beliefs and actions of three dominant groups: Afrikaners in South Africa, Jews in Israel, and Protestants in Northern Ireland. The covenant that lies at the core of the world-views of each of these groups is told in the Exodus story in the Old Testament. Its "if-then" character, "If Israel will be his people, Yahweh will be their God" entails obligations and responsibilities for all Chosen People that are manifest through an emphasis on social law, a clear distinction between enemies and allies, the warlike character of the Deity, attachment to specific pieces of land, and group purity (p. 13).

Lustick's main concern is in the dynamics of state expansion and contraction associated with shifts in boundaries. He explores how Britain and France went from seeing Ireland and Algeria as integral parts of their national territory to viewing Catholic Ireland and Algeria as independent political entities. His analysis of these two cases then provides the basis for an examination of the prospects for the creation of an independent Palestinian state on the West Bank and Gaza.

Despite the different core questions driving these studies, both authors address a number of common issues. For example, each is concerned with the central role of cultural metaphors and shared interpretations of the social and political world. Akenson develops the idea of the *covenantal grid*, a conceptual schema that offers a culturally constructed explanation for past events and a guide to future action for the politically dominant groups in each society under study. He explores the grid's biblical roots and explains its social and political power in these three societies through both the general fit between the grid's central metaphors and the human condition and its more specific capacity to provide relevant guidelines for social and political action and group survival.

Land, exodus, and blood sacrifice are three central "points of great sensitivity within the grid and anything that touches one of them energizes the entire grid and calls forth an immediate, focused, sometimes violent

response" in each culture under examination (p. 31). In his discussion of political myth, Akenson, like Geertz, suggests that myths both reflect everyday societal functioning by providing a psychocultural framework for the interpretation of the world and, at the same time, shape the social and political order.

Lustick, interested in shared, culturally constructed beliefs about the state and the political order, inquires into the sometimes convulsive dynamics associated with state boundary redefinition. In doing so, he reminds us that while we readily think of boundaries as unchanging, the historical truth is that state boundaries are political constructions. The expansion and contraction of states, he shows, occurs more often than we believe, accompanied by political upheaval in which the foundation of the political order and make-up of the political community are at stake. His analysis, built upon Gramsci, emphasizes three stages (incumbency, regime, and ideological hegemony) and two thresholds involved in state expansion or contraction. The ideological threshold involves struggles over definition of the political community, while the regime threshold marks struggles over the structure of the political order.

Akenson and Lustick emphasize very different-but not necessarily incompatible-mechanisms for understanding the politics of domination and change in the cases they examine. Akenson pays more attention to the culture of the dominant group, and Lustick stresses the political dynamics in the dominant state. Akenson makes a strong case for the power of culturally shared beliefs as motivators of action through the construction of in-group solidarity and the development of in-group institutions and practices to exercise power and ward off outside enemies. He points out that none of the groups he examines has shown any real interest in gaining new adherents through conversion, despite the fact that all consider themselves as besieged minorities. In fact, there is great concern, almost obsession, with the problems of impurity and dilution of the group through too easy and too-casual contact with outsiders.

A crucial mechanism Akenson identifies for achieving social and political separation is through religious institutions and practices that inform daily life. Also critical is the role of language: he reminds readers that both Afrikaans and Hebrew were dead languages, a few generations ago only spoken by a handful of people. Language, like other exclusive cultural practices, comes to serve as an emotional focus for group solidarity but also becomes the central mechanism for achieving that solidarity through its central role in education and socialization of successive generations. (Interestingly, in Northern Ireland, where language does not separate Protestants and Catholics, there are separate school systems for each group offering distinctive content and values to students.)

Lustick's inquiry into the process of changing state boundaries begins with detailed discussion of an important problem—the extent to which Israeli control over, and settlement in, the West Bank and Gaza has created "facts on the ground" that effectively preclude the emergence of an independent Palestinian state in the area. His conclusion is that while facts on the ground may be difficult to undo, they are not enough to fix political boundaries in the absence of both ideological hegemony and an effective regime. Crossing both of these thresholds, it turns out, was crucial, in somewhat different ways, in ending British rule over all of Ireland and French control of Algeria.

He details both the movement across the ideological threshold in Britain in the 1830–86 period, when the idea of Irish self-rule moved from the absurd to government policy, and the changing conception of where and what was France from 1830 to 1958. In each case, the collapse of a particular hegemonic definition of the state as necessarily including the relevant peripheral regions provoked a second conflict, namely, over the structure of the regime itself, which, for a time, takes precedence over issues of incumbency and power holding.

State contraction (what Lustick calls rescaling) occurs in different ways in response to specific challenges. In Britain, problem decomposition (dividing Ireland into two parts) and in France, regime recomposition (the birth of the Gaullist Fifth Republic) turn out to be crucial mechanisms to the management, if not the resolution, of the regime and ideological crises. These cases, he persuasively argues, have important lessons for the Israeli situation; and he proceeds to develop some thoughtful and imaginative scenarios about the future of the Middle East (some of which have already come to pass) based on his theoretical analysis.

Despite the popular image that caricatures covenantal cultures and colonial societies as head-in-the-sand, ostrichlike creatures always resistant to change, both studies show this view as too simplistic. Akenson devotes three chapters to the significant shifts in the political landscape in Northern Ireland, South Africa, and Israel in recent years. He argues that while covenantal cultures' immediate response to outside threat is to assume they can outlast it, new relationships can be built, and great changes are possible within the covenantal grid when these cultures are persuaded that a friend (or foe) is there to stay. However, he warns that "because within covenantal cultures compromise is taken as a sign of weakness, the leaders of these nations must be given plenty of room to talk to themselves and their constituents, to become accustomed to the potential for change, and to convince themselves that the idea was theirs all along" (p. 356). Lustick, too, speaks to the issue of change, arguing that sometimes it is sudden, swift, and nonlinear, as one can see in the transition between the Fourth and Fifth French Republics.

Both books under review are important studies showing the merits of theoretically driven, historically rooted, comparative analyses. Akenson's will probably be viewed as more unconventional by political scientists skeptical of the power of ideas (especially religiously rooted ones) to shape political order in the modern world. Although Lustick, too, emphasizes political actors' conceptualization of their worlds and how this shapes the alternatives they take seriously, his analysis will be more familiar in terms of both its concepts and its problems. Both books are long—in Lustick's case, I think, longer than necessary. Each provides theoretically informed cases and shows the value of comparing what often, to both participants and observers, seem like unique, unrelated cases.

Bryn Mawr College

Marc Howard Ross

The Domestic Politics of German Unification. Edited by Christopher Anderson, Karl Kaltenthaler, and Wolfgang Luthardt. Boulder: Lynne Rienner, 1993. 254p. \$28.00.

From Bundesrepublik to Deutschland: German Politics after Unification. Edited by Michael G. Huelshoff, Andrei S. Markovits, and Simon Reich. Ann Arbor: University of Michigan Press, 1993. 396p. \$55.00 cloth, \$22.50 paper.

The unification of Germany is clearly one of the most significant events of the twentieth century. These two new edited volumes take stock of the changes in Germany's internal and external politics and their meaning and begin to piece together a portrait of the "new" Germany. In spite of striking differences in their approaches, both collections make it clear that the united Germany is more than the sum of its parts, though the complexity of unification and its status as a "work in progress" make it difficult to define precisely the character of the new Germany.

From Bundesrepublik to Deutschland is the more ambitious of the two collections. The volume considers the contributions that different research agendas in comparative politics and international relations have made to our understanding of German politics, and then questions their applicability to the study of Germany after unification. Huelshoff and Markovits note that prior to 1989, the study of German politics evolved into the study of a "normal" democratic regime, anchored within a relatively stable world order. Initially, practitioners of comparative politics stressed variations on the theme of democratic stability that were central to the subfield in the 1960s and 1970s. As the Bundesrepublik matured and fears of democratic breakdown waned, research on Germany came to emphasize institutions, parties, evolving public opinion, and other issues associated with the study of stable democracies. The themes of normalcy and stability have survived the subsequent growth of political economy, albeit in somewhat different forms, such as corporatism's focus on class compromise and consensus. The editors also point out that the normalcy of the Bonn Republic was critical to the Cold War security system's structure and the associated emphasis on bipolarity in international relations during the

The editors believe that these perspectives have overemphasized structure and continuity in their explanations of German politics. These approaches are not well suited to the context of German unification, which embodies a break with the normalcy of the Bundesrepublik. Theoretical and substantive orientations that were heretofore dominant in the study of Germany are likely to decline in importance. This is especially true of theories in international relations that stress system structure. While some approaches, such as those in comparative politics focusing on the evolution of liberal democracy, remain useful in considering the implications of the new historical context, the editors maintain that they are not too helpful in identifying its causes: comparative history is better equipped for both tasks, especially the latter. The contributions to this volume are united by their consideration of continuity versus change, structure versus process, and the role of ideas and ideology in the study of German politics. After a review of past research on the Bundesrepublik and its extension to united Germany (parts 1 and 2), these

themes are explored in four different substantive settings: "Political Economy," "Women, Politics and Society," "Inter-German Relations and the Politics of Unification," and "Germany and Its External Environment" (parts 3–6, respectively). Some sections of From Bundesrepublik to Deutschland are more unified and coherent than others, and there are differences in the clarity of the book's central themes, which emerge with the greatest lucidity in part 6.

The Domestic Politics of German Unification is more modest in scope. Eschewing explicit involvement in epistemological debates and evaluations of theoretical approaches, this volume centers on the substance of domestic politics before, during, and after German unification. Four key questions serve as the focal points of this collection: How did the process of unification take place and how can it be understood? What are the effects of unification, both short- and long-term, on the democratic process in Germany? What are the consequences of unification for German political institutions? What are some concrete examples of policy problems facing the united German? (p. 10). Thus, this collection centers on a number of issues that Huelshoff, Markovits, and Reich believe will recede in importance, although question one occasionally leads the authors onto similar terrain. When this occurs, they reach similar conclusions regarding the impact of the "undigested" legacies of German history

The book is organized into two parts. Part 1 focuses on the process of unification and the politics surrounding it. With the exception of Lembruch's treatment of regime change in East Germany, the individual contributions center on parties and electoral politics. Part 2 focuses on institutions and policy alternatives. The emphasis in this part is on existing and potential institutional changes. Two recurring themes emerge: the impact of unification on essentially West German institutions (including the possibility of a new German constitution) and the problems of transferring these institutional arrangements to the eastern Länder.

Differences in approach make these books useful companions on a number of different dimension. At the level of larger themes and approaches to understanding Germany, Domestic Politics reinforces From Bundesrepublik to Deutschland's argument that great attention should be given to the interplay of historical continuity and change, and that united Germany represents a synthesis of Germany's various pasts. Domestic Politics also reinforces the editors of From Bundesrepublik to Deutschland's faith in continuing work on the evolution of German democracy as a useful social-scientific counterpart of historical analyses. The very insightful contributions found in Domestic Politics suggest that rather than receding behind concepts such as nationalism, national identity, and power, the study of Germany's domestic institutions is integral to understanding them.

These books also complement each other in terms of coverage. The sheer number and complexity of issues surrounding German unification make it impossible for a single volume to provide a comprehensive treatment. Out of necessity, the contributors to *Domestic Politics* do not address issues involving international politics. There are important insights to be gathered here, given the role of external forces in contributing to unification itself and lingering questions concerning Germany's role in world politics. This gap is closed, at least partially, by several of the contributions to *From Bundesrepublik to*

Deutschland. For example, a chapter by Markovits and Reich and one by Huelshoff examine Germany's position within post–Cold War Europe. Their accounts provide solid support for the thesis that Germany is becoming an economic (rather than a military) hegemon within Europe and a key actor within a multipolar world. Furthermore, Markovits and Reich link Germany's relations with, and partial dominance of, Europe to key characteristics of its domestic politics, most importantly its commitment to liberal democracy and commercialism, which provide a more or less reluctant Germany with ideological leadership. Huelshoff's contribution also stresses domestic politics via his application of Putnam's two-level game metaphor to Germany's behavior within the European Community.

On the other hand, the coverage of domestic politics per se provided by From Bundesrepublik to Deutschland is rather weak—troubling because the book's subtitle implies a substantial treatment. Unfortunately, there is no sustained consideration of many of the core issues of German domestics politics, though certain elements of it, such as women and politics, the transformation of the eastern Länder, and the rise of the radical Right, receive considerable attention. The emphasis on parties, electoral politics, and institutional arrangements found in Domestic Politics fills these gaps admirably.

In addition to filling unavoidable gaps in each other's coverage, these books compliment each other in the areas where they overlap. In some cases, the overlap provides a more complete picture of issues that are fairly well understood. An example of this may be found in the fit between From Bundesrepublik to Deutschland's historical analysis of the women's movement, East and West, and such contributions to Domestic Politics as Anderson's presentation of trends in the representation of women in the Bundestag and Kommers' consideration of the constitutional dimensions of the abortion issue. In other cases, overlaps between the two books remind us of just how tentative our understanding is and help to identify areas where a relatively greater amount of research will be needed. For example, the contributions by Allen (From Bundesrepublik to Deutschland) and Hancock (Domestic Politics) leave the reader uncertain as to whether macro- or mesocorporatism will constitute the dominant model of industrial relations in Germany, not to mention the possibility of a breakdown in the generally amicable relationship between business and labor. Of course, this is the case because industrial relations, like many other aspects of German politics, are still in a state of flux. However, suggestions and speculations regarding the likely alternatives provide future research with useful points of departure.

Obviously, it will be some time before German unity and its implications are properly understood. Each of these edited volumes makes solid individual contributions toward that end. Taken together, their contributions are even stronger.

Southern Methodist University W. DAVID PATTERSON

Soldiers and Politics in Eastern Europe, 1945-90: The Case of Hungary. By Zoltan D. Barany. New York: St. Martin's, 1993. 243p. \$65.00.

This is one of the best of the "year 1989" studies of the complex relationship between the Communist party and

the military organization. What characterized this relationship in Eastern European polities at this time was the effort to depoliticize the military.

The Party is the most combative superstructure of Soviet regimes. The military clearly is subordinated to a hegemonic Party that tolerates no political or ideological opposition. Yet the cleavages are internal—within the military and the Party, rather than between the two. Even if the military is the third structure of power (after the secret services and the Party), its elites are well represented in almost all levels of Party leadership.

What makes Eastern European armies even more subservient to the Party is the role played by the Soviet Army in "the transformation of the East European armies according to Moscow's desiderata" (p. 18). Barany takes us through all the stages of transformation that the Hungarian military underwent—from Sovietization (1945–53), to subordination (1953–68), to accommodation and recalcitrance (1968–88), to the final democratization (1988–90). Unfortunately, this is the weakest section of an otherwise well researched study.

The author is methodologically sound and has carefully followed the literature that dealt with civil—military relations in communist and totalitarian systems. Once again, the study confirms the hegemonic role of the Party over the state and the military.

In examining the utility of various models on civil-military relations in communist systems (e.g., Colton and Odom in Herspring and Volgyes's Civil Military Relations in Communist Systems [1978] and also Roman Kolkowicz's Soviet Military and the Communist Party [1967]), Barany finds they are "at best . . . clusters of generalizations and, for the most part, unsubstantiated hypotheses," since most relate to the Soviet model (p. 169). I find that even so far as the Soviet model is concerned, much of the "modeling" in the 1960s and 1970s bears no resemblance to what we know now about the Soviet military. The ethnic and regional divisions, as well as the nature of the relationship between the Party and the army, has been oversimplified.

What is most significant in this study is the author's painstaking effort to analyze the methods employed by the Party to gain and maintain control over the military institution. Of great interest is the final comparative chapter that deals with civil-military relations in Eastern Europe. Especially interesting is the analysis of the mechanism of party control over its armed forces in Hungary, Poland, and Bulgaria before 1990 and the adoption of the Soviet model as an instrument of its domination. What is unfortunately missing (although the publication date is 1993) is an explanation of the role and attitude of the military at the end of Party domination and, above all, whether the army split on, encouraged, or rejected reform.

Now that the reformers have been electorally defeated and Russia has become a rival and not an enforced ally, I wish the author would have elaborated on what portends for future civil—military relations in Eastern Europe.

This excellent comparative study of the role of the military in Eastern Europe (especially Hungary) confirms the subordinate role played by the military in communist countries. Barany confirms the view that the military did not act independently to forestall the democratic revolution of 1989 once its courses of authority collapsed. Barany's thorough and profound analysis clearly demonstrates that the military never acted as a political pressure group, as some prominent theorists of

civil-military relations of communist countries (mainly the USSR) incorrectly claimed. This study, hopefully, will pave the way for new works, as well as new approaches, in the field of civil-military relations in former communist countries.

American University

Amos Perlmutter

Hemmed In: Responses to Africa's Economic Decline. Edited by Thomas M. Callaghy and John Ravenhill. New York: Columbia University Press, 1994. 573p. \$50.00 cloth, \$18.50 paper.

The 1980s were a decade of economic crisis in sub-Saharan Africa. Beleaguered by stagnant growth and mounting debt, many countries embarked on World Bank and IMF-sponsored adjustment programs. Several years of erratic reform produced hesitant stabilization in several countries, but few signs of a robust return to growth. As the 1990s dawned, Africa was gripped by a second regional crisis, this time political in nature. Dozens of single-party states and military dictatorships, confronted by resurgent domestic opposition and international pressure, liberalized their polities or initiated transfers to competitive electoral rule. The wave of political transitions produced equally tentative and uneven results, ranging from democratic reform to civil war and state collapse.

There has been a dearth of analysis linking these epochal changes. This volume is an indispensable contribution, for it provides a wide-ranging, theoretically informed effort to address the complex issues of economic and political reform in the region. The studies collected here examine Africa's position in the global economy, the role of international financial institutions (IFIs) in guiding regional reform, and the economic and social impact of orthodox adjustment policies. Several authors also assess the political context of economic policy change, and the implications of political transition for future reform.

If there is a shared perspective among these writers, it is this: reform was inevitable for Africa, given the disastrous trajectory of economic decline in the early 1980s. Some of the central elements in the structural adjustment package (fiscal restraint, price reform) were necessary, though not sufficient for economic recovery. However, the orthodox agenda in Africa has been a substantial failure. This is partially attributable to the inherent limitations of underdeveloped markets and weak states, but it is also a consequence of global economic trends and the policy myopia of the IFIs. While Africa faces a limited array of options, feasible alterations in the domestic reform agenda and the international economy might significantly improve the region's discouraging prospects. Sustained recovery is also contingent upon a dramatic change in the political context of economic management.

The opening chapters examine Africa's experience with debt and adjustment. In his overview of the IFIs' approach to reform, John Ravenhill notes that the IMF and the World Bank have undergone a limited process of learning during the past decade. The Bank in particular has moved away from a simplistic adherence to "getting prices right" and stabilizing fiscal and external balances. By the end of the 1980s the Bank readily acknowledged the importance of domestic political reform, institutional

development and international debt relief as essential components of economic recovery. They also called for more flexible implementation and appropriate policies to offset the social costs of adjustment. Despite the rhetorical shift, Ravenhill admonishes the IFIs for a doctrinaire insistence on price reforms and a rigid application of neoclassical principle to diverse African countries.

A more pointed criticism of orthodoxy is offered by Reginald Herbold Green. He stresses the inherent tension between the contractionary stabilization policies of the IMF and the ostensible growth objectives of Banksupported structural adjustment. The Bank, though more adaptable than the Fund, has been excessively sanguine about the capacity of African economies to adjust rapidly in response to policy changes, and naive about the serious distributional and welfare costs of structural adjustment. Green notes that the alternatives proffered by African regional organizations are not very compelling, but he argues for a reform strategy which accords a greater role for state tutelage and productive public investment, a more variegated approach to adjustment, and an appreciation of the domestic political constraints on reform.

David Gordon and Matthew Martin focus on the central role of external debt in the African crisis. Gordon traces the genesis of the global debt crisis and its acute impact on African economies. Intractable debt obligations entailed a dominant regional role for the IFIs, who, as "lenders of last resort," provided resources linked to the adoption of orthodox reform. While recognizing the pervasive influence of IFI conditionality, Gordon refutes a monolithic view of these institutions. In the course of the 1980s, IMF and World Bank stipulations were often disparate, and the growing scope of conditional lending tended to fragment and dilute the impact of conditions. Most African regimes, through circumstance or design, failed to comply with important performance requirements, and the IFIs possessed limited leverage to enforce compliance. The result has been a pattern of partial and uneven reform, as weak institutional capacities and domestic political imperatives undermined commitment to externally-driven reform.

In a detailed comparison of Ghana and Zambia, Matthew Martin examines the negotiation process among governments and the IFIs. He finds inherent flaws in the way these institutions deal with debtor countries, noting especially the proliferation of complex and contradictory requirements attached to adjustment lending. Aggressive conditionality by the IFIs ultimately hindered sustainable reform, as governments adopted politically risky adjustment policies, without obtaining compensating external resources or witnessing dramatic economic improvement.

Turning to the political dimensions of the African crisis, Naomi Chazan and Donald Rothchild examine the effects of economic decline on state-society relations. The regional economic crisis was rooted in the predatory domination of weak authoritarian regimes. Economic stagnation paradoxically eroded the foundations of these regimes, thus aggravating state enfeeblement and political instability. Chazan and Rothchild discern a corresponding strengthening of civil society, as communities and social networks fashioned independent coping strategies amidst economic privation. Revitalized civil societies successfully challenged the ancien regime in numerous countries, yet the ultimate results of this struggle have been inconclusive. Throughout much of Africa, the absence of effective public authority is mir-

rored by a nascent yet fragmented civil society, which is unable to provide a coherent alternative to bankrupt states. This political impasse remains a hindrance to economic restructuring.

Taking up the pivotal relationship between political and economic reform, Thomas Callaghy challenges the guiding assumption of the IFIs and bilateral donors, who envisage a natural and positive synergy between economic liberalization and political democracy. He argues that successful economic reform is contingent upon the nature of institutions and the character of state power, rather than ideology or regime type. A pervasive political logic" of regime maintenance in Africa has typically contravened the logic of economic rationality, and Callaghy does not foresee an easy resolution of this tension through the process of political transition. New democracies are especially vulnerable to distributive demands and rent-seeking pressures, and political liberalization could easily weaken state capacity to sustain reform. In an instructive comparison of Ghana, Nigeria and Senegal, Callaghy observes Ghana's adherence to economic liberalization under authoritarian rule; Nigeria's increasingly shaky (and ultimately unsuccessful) effort to pursue simultaneous economic adjustment and democratic transition; and Senegal's weak economic reforms under democratic auspices. The key distinguishing feature of these cases was the relative ability of political elites to insulate technocratic reformers within the state and to resist ubiquitous distributive claims from social groups.

Callaghy's comparative insights are underscored by Nicolas van de Walle, who provides a compelling account of "non-reform" under Cameroon's authoritarian regime. The central features of orthodox reform (fiscal compression, market liberalization, privatization) threatened the Biya regime's discretionary control over resources, a crucial pillar of neo-patrimonial rule. Pressed by domestic fiscal crisis and IFI conditionality, Biya's government temporized on key policies, placated donors to sustain a limited flow of external resources, and provided rents and sidepayments to regime supporters. Lacking both the commitment and institutional capability to seriously implement reform, the Cameroonian government manipulated adjustment measures to preserve the clientalist underpinnings of the state.

Michael Lofchie demurs from the latter authors, offering a more favorable evaluation of orthodox policies and the liberal reform scenario. In a comparison of Kenya and Tanzania, Lofchie notes that these countries virtually "traded places" in development strategy and performance during the past decade. Tanzania, a moderate socialist regime mired in economic distress, shifted from a strongly ideological orientation to a more pragmatic, liberalizing stance in the mid-1980s, with salutary effects. Kenya, for many years regarded as a successful bastion of African capitalism, experienced declining performance as the Moi regime undermined growth in the quest to reconstruct ethnic clientalist networks and bolster the basis of single-party dominance. Accepting the virtuous relationship between political liberalization and economic reform, Lofchie sees political pluralism as the key to better performance in both countries.

When examining the sectoral impact of structural adjustment in Africa, agriculture justifiably commands greatest attention. Three chapters respectively examine micro-level, meso-level, and macro-level dimensions of the rural sector. Focusing on the behavior of rural

households, Sara Berry illuminates the problem of civil society in the countryside, and offers a skeptical assessment of donors' claims regarding the benefits of reform for agricultural growth. Observing that rural survival strategies center on diversification, Berry portrays the ways in which agricultural producers hedge their sources of income and seek access to resources through social networks. In circumstances of economic austerity, producers shift production and intensify off-farm employment, possibly lowering returns in labor and commodity markets and limiting long-term productive investment. Household economies in Africa do not engender a clear productive response to price reform. Moreover, commitments to kin groups and community networks tend to diffuse the elements of civil society.

The circumstances under which rural producers mobilize to defend their collective interests is a crucial issue taken up by Jennifer Widner. Many of the survival strategies cited by Berry were unavailable to cocoa producers in Cote d'Ivoire during the commodity crisis of the late 1980s. Unable to shift to new sources of income, or to draw on the resources of kinship networks, Ivorian villagers increasingly attributed declining rural conditions to government mismanagement, and voiced their grievances through organized dialogue and protest. The propensity to engage in "proto-politics" was influenced by the relative strength of traditional authority structures and state institutions in the villages, as well as the return of better-educated younger people from the urban areas. While highly significant, the exercise of political "voice" by rural producers did not signal the emergence of a coherent rural lobby in Cote d'Ivoire. Rural political initiatives were scattered and ad hoc, and villagers did not readily affiliate with opposition parties in the early 1990s.

Jeffrey Herbst sketches the general features of Africa's declining agricultural performance, and the leading reforms essential for sustained recovery. He then considers three countries which have seemingly avoided the "urban bias" typical of regional economic policies. In Kenya, Zimbabwe and Ghana, regimes fashioned programs comparatively supportive of agriculture. Herbst attributes these favorable policies to an idiosyncratic array of factors: in Kenya, the political salience of rural elites and the nature of the (formerly) single-party electoral system: in Zimbabwe, the historical influence of

Hemmed In is an ambitious work, and one might observe both shortcomings and excesses. There is a good deal of repetition here, and several chapters might have been trimmed of redundant material. Conversely, there is relatively uneven material on civil society and the social impact of adjustment, especially in the urban sector. The volume also lacks a comparative empirical overview of regional stabilization and adjustment experiences. A profile of this sort would provide a useful context for the cases which follow. Cavils aside, this collection offers the most important recent contribution to the literature on Africa's political economy. These chapters, rich in analytic insight, impressive in their thematic scope, and replete with important case materials, represent the best current work on the region. Indeed, the value of this collection extends well beyond its regional focus, as the authors address the political dynamics of economic reform, sectoral problems of structural adjustment, comparative political transitions, and important trends in the international economy. It will be of longstanding value for comparativists and students of international political economy.

American University

PETER M. LEWIS

Politics, Feminism, and the Reformation of Gender. By Jenny Chapman. New York: Routledge, 1993. 315p. \$79.95.

After years of neglect, the recruitment of women to public office has become a lively topic for scholars on both sides of the Atlantic. American scholars have focused on the structure of incentives and institutions affecting women's candidacy, whereas Europeans have examined the socioeconomic and partisan factors determining their presence among political elites. Jenny Chapman's research is in the latter tradition; but it raises issues about the interplay of social status, party organizations and gender roles that are relevant on both continents.

Chapman develops and tests a theory of recruitment to account for women's exclusion from elective office, and she places her analysis within a comparative and historical context: "A universal problem requires a universal explanation" (p. 8). Her broad perspective and

according to male gender roles, which inhibits women's entry into the public sphere.

Chapman analyzes the occupational bind for women-the first blade of the scissors-through an extensive survey of winning and losing candidates for local office during 1984 in Strathclyde, Scotland. On the ideological Right, she argues, women's increased educational attainment does not lead to the high professional status or the control of land and small businesses that characterize politically successful males. On the Left, where egalitarianism is embraced at least in principle and educational and social credentials are less important, women's absence from the industrial sector denies them participation in the dominant unions that promote lowstatus men into the political elite. Whether educated or not, then, women fail to fit the profile that dominates the major parties and end up being indistinguishable from losing men. Furthermore, this result holds for minor parties and independent candidacies in Strathclyde. The author bolsters these findings with comparisons from the United States and the former Soviet Union teased from case studies conducted primarily in the 1970s. Unfortunately, they are of limited value because of outmoded and inadequate data—a limitation Chapman herself raises. Consequently, the Strathclyde survey, with all its local idiosyncrasies and problems of scale, carries the burden of her argument.

To complement the modest empirical foundations of her analysis, Chapman turns to the reproductive role of women and its implications for the development of female leadership. This is the second blade of the scissors, but it actually has two cutting edges. The first is the male value system that subordinates women because it discounts their experiences and traits as mothers. In effect, Chapman argues, to be successful in the competitive, hierarchical world of men, women must reject the very qualities that define their gender. She illustrates this dilemma with a fascinating comparison of the political advancement of women in postindependence Finland and postrevolutionary Russia, which demonstrates again why neither education nor egalitarian ideology is sufficient foundation for women's entry into the political elite.

The added bite of this scissor's blade comes from the failure among feminists to recognized the inherent conflict between the private sphere of mothering and the public sphere of electioneering. Chapman charges that feminists' belief in equal rights and social democracy is misconceived and uses the experiences of the West German Greens, the Norwegian parliament, and the Women's party in Iceland to make her point. These are success stories in the sense of above-average representation for women as candidates and members of the legislature, but feminist organizations played only a small part in bringing them about. Instead, the author argues, each advance occurred because of conscious decisions by party activists to recruit women (even to the point of quotas, for the Greens) and by female candidates to stress their differences from men, rather than their similarities.

Although some might argue that Chapman distorts feminist thinking, this aspect of the argument has profound implications for women and for Chapman's own analysis. It suggests that efforts to recruit more women to legislative office will ultimately fail if they depend on women's becoming more like men in terms of credentials. Furthermore, it indicates that women will be unable to compete for political office unless women's work as caregivers is recognized and compensated through a

reordering of social values. In short, men and male roles are what need changing before women will be able to assume their share of responsibilities for democratic governance.

Chapman's conclusion is not only profoundly pessimistic but also raises questions about the relevance of the first half of her book. Her argument that women are excluded from office not by discrimination but by the lack of attributes characteristic of politically successful men loses much of its force when juxtaposed with her assertion that women's role places them "outside" the entire structure of economic and political life. In the end, she suggests that it is not economics or social status that bars women from political office but gender as it is constructed by men.

Political scientists, accustomed to looking for causation among economic, social, or structural variables, may find this conclusion difficult to embrace, particularly since it demands a cultural revolution of unprecedented scale to achieve political parity between men and women. Whether or not the reader is persuaded by Chapman's arguments, there is much to ponder here: the distorting effects of traditional, education-based measures of status when applied to men and women; the historical ebb-and-flow of women's struggle for equality; the mediating effects of parties in promoting women into leadership roles; and the influence of ideas on political competition.

Syracuse University

LINDA L. FOWLER

The Nation and Its Fragments: Colonial and Postcolonial Histories. By Partha Chatterjee. Princeton: Princeton University Press, 1993. 282p. \$49.50 cloth, \$15.95 paper.

Religious Nationalism: Hindus and Muslims in India. By Peter van der Veer. Berkeley: University of California Press, 1994. 247p. \$40.00 cloth, \$14.00 paper.

One of the major trends of this decade is the rise of new movements for religious nationalism in such places as the Middle East, Northern Africa, Central Asia, and South Asia. Their encounter with the secular nationalism of Europe and America has led to what Samuel Huntington has called "the clash of civilizations," or what might be called the beginning of a "new Cold War."

The two books under review are important contributions to the understanding of this encounter. In both cases, they give insights into the pivotal role played by culture in the current efforts to redefine national identity in India, their major point of reference. In Chatterjee's analysis, India is the prime example of how the "colonial and postcolonial histories" experienced by a people lead to new, albeit fragmented, national identities. In Veer's book, case studies of the rise of Hindu and Muslim nationalism in the Indian subcontinent provide ways of understanding how culture-based politics fit into the modern world.

Because India looms so large in both of these books, they may not get the attention they deserve: one may be tempted to shelve them in an "area studies" category reserved only for Indianists. This would be a great mistake, for their attempts to present a non-Western perspective on world politics are of universal interest. Both books are written in an engaging style and should

be read by anyone concerned about the fate of the contemporary world, perhaps especially by those least knowledgeable about non-Western cultures.

Partha Chatterjee is one of India's leading political philosophers and a well-known member of the subaltern school—a group of scholars, many of them Indian, who have applied Gramsci's notion of nonelite, or "subaltern," views of society to the historical and contemporary political experience of formerly colonial countries such as India. Fundamental to their scholarly enterprise is the attempt to resuscitate the perspectives of peoples who have been oppressed or marginalized due to gender, class, caste, and local frames of reference.

In an influential previous book, *Nationalist Thought and the Colonial World*, Chatterjee claims that nationalism of the type that the British bestowed on India was a "derivative discourse." According to Chatterjee, the secular nationalism of Britain and America, which was so admired by Congress leaders such as Nehru (but not Gandhi, who had a more traditional vision of Indian nationalism), is a political vestige of Enlightenment ideology. Increasingly, this paradigm has been questioned by postmodern theorists and challenged by local political leaders.

In his latest book, *The Nation and Its Fragments*, Chatterjee presents a series of essays that advance this line of thinking, and address the question of how a noncolonial form of nationalism is possible in postcolonial countries such as India. He argues that the construction of an authentic nationalism—one rooted in the experiences of a people—requires an appreciation of the fact that the social whole has, in countries such as India, been seen only from fragmented perspectives and that there has been no single national culture.

Chatterjee observes that the absence of an authentic Indian nationalism is, to a large extent, the result of colonial rule, which denied to Indians the ability to develop unified economic and political centers of power. Although the British presence brought the infrastructure of unity in the form of trains, roads, and an administrative bureaucracy, it was in the interest of the colonial powers to keep the social divisions of caste, class, religion, and region intact. Only a small westernized elite in India experienced a sense of being part of a social whole; and by taking over the economic and political systems that the British developed, this elite stratum of Indian national leadership simply replicated colonial national

Yet although India remained colonial in its business and political practices, Chatterjee argues, it has developed its own independence in the spiritual and cultural spheres, which the British were largely unsuccessful in altering. For that reason, the search for an authentic postcolonial nationalism begins with culture: "Here nationalism launches its most powerful, creative, and historically significant project: to fashion a 'modern' national culture that is nevertheless not Western" (p. 6).

At least at the beginning, however, this national culture is to be found in a fragmented form, as various groups within society give voice to their own distinctive experiences. It is for this reason, presumably, that India has undergone such violent tensions of competing cultural nationalisms: Hindu, Muslim, and Sikh.

To understand these religious rivalries in their recent manifestations, one should turn to Peter van der Veer's insightful *Religious Nationalism: Hindus and Muslims in India.* Like Chatterjee, Veer regards secular nationalism as an example of the "Western discourse of modernity" that is profoundly rejected by religious nationalists in places such as India. In turning to their own traditions for a new sense of national identity, these new political activists necessarily build on traditional cultural ties to Hinduism, Sikhism, and Islam. This does not mean, however, that these cultural nationalisms are ancient or entrenched. In their current form, they are rather recent inventions, products of the political searching of the present age. For that reason, Veer sees them as possibly transitional steps toward an emerging, panethnic, indigenous nationalism.

The recent conflict at the Indian pilgrimage city of Ayodhya is the book's central case. In recent years, nationalist Hindus have yearned to destroy Ayodhya's old mosque, built by the Moghul emperor Babar on the site of what many have come to believe is the birthplace of the God Rama and replace it with a Hindu temple. The Hindu political party, the Bharatiya Janata party (BJP) based much of its support on its ability to champion this demand; and when an unruly mob finally did destroy the mosque on 6 December 1992, the BJP was blamed for the action. In Veer's perceptive analysis, Ayodhya becomes a metaphor for religious nationalism, where in Veer's frame of reference, the control over the definition of public space is a central element.

To the credit of both of these authors, they are able to deal dispassionately with a subject that usually generates more heat than light. In attempting to reserve judgment about the ultimate value of religious nationalism, they will undoubtedly be criticized by those who adopt what Veer describes as the usual "liberal" position of comdemning religious politics out of hand. The extremists among these critics—the "new Cold War" hawks—will claim that Veer and Chatterjee have been duped by the religious facists in the BJP. Yet neither of the authors are directly supportive of any particular group of religious nationalists. In fact, their analyses could be used to criticise those leaders of the BJP and other movements who exploit for their own personal self-interests the legitimate popular desire to see cultural values enshrined in public pronouncements.

These are important books for understanding the cultural and political changes of the contemporary world, especially that part of it formerly known as the Third World. In their quite different but equally insightful ways, Veer and Chatterjee have helped to blaze the trail for a scholarly assessment of what will increasingly become a salient feature of postcolonial politics—the emergence of nationalistic ideologies and political theories based on local cultural ties.

University of California, Santa Barbara MARK JUERGENSMEYER

Democracy in the Caribbean: Political, Economic, and Social Perspectives. Edited by Jorge I. Domínguez, Robert A. Pastor, and R. Delisle Worrell. Baltimore: Johns Hopkins University Press, 1993. 312p. \$48.00 cloth, \$14.95 paper.

Democracy in the Caribbean: Myths and Realities. Edited by Carlene J. Edie. Westport: Praeger, 1994. 320p. \$65.00.

Two identically titled books on democracy in the Caribbean within a year is either a remarkable coinci-

dence or evidence of rising scholarly and policymaker concern, or both.

There is indeed cause for concern. However vigorously some may celebrate functioning democratic systems in the English- and Spanish-speaking Caribbean (Dominican Republic and Puerto Rico) and signs in the French-speaking (Haiti) and Dutch-speaking (Suriname) areas that democracy under siege may yet return, the danger signs are plentiful: declining economies, declining international transfers (including remittances), and rising un- and underemployment may combine with inability of ruling parties (related to the above) to dispense the patronage that has bought them electoral support. The prospect: instability and violence. Furthermore, Guyana and Trinidad-Tobago have shown a proclivity for injecting race into electoral politics, dividing their societies along communal lines at the same time as Apartheid was being dismantled in South Africa. And Cuba remains staunchly authoritarian. These are only a few of the cloud banks that are massed on the horizon. All these issues are ably dissected in both books.

These two volumes round out a half-dozen Caribbean-wide analyses in English in an equal number of years since The Caribbean After Grenada: Conflict, Revolution, and Democracy (1988) by MacDonald, Sandstrom and Goodwin. Caribbeanists have been blessed with The Poor and the Powerless: Economic Policy and Change in the Caribbean (1988) by Thomas, Conflict, Peace and Development in the Caribbean (1991) edited by Beruff, Figueroa, and Greene, Society and Politics in the Caribbean (1991) edited by Clarke, and Storm Signals: Structural Adjustment and Development Alternatives in the Caribbean (1991) by McAfee.

This is quite extraordinary, multi-focused, and multidisciplinary for an area not known for drawing such attention from scholars. One of the books here reviewed (the Edie volume) fits with the rest in being written by academic social scientists, happily including some junior faculty. The Domínguez volume is divided roughly in half between academics (mostly senior people) and policy analysts with institutional affiliations. Both focus on democracy. However, with some qualification it is probably fair to say that the Domínguez authors are content with conventional western notions of liberal democracy while contributors to the Edie volume exhibit more skepticism and critical analysis.

Since all definitions are arbitrarily true, it may seem trivial to discuss them. However, as all Caribbeanists know, there is little agreement among them on what makes sense to include under the "Caribbean" rubric, not to mention treatises on democracy in the region. The Domínguez volume rules out a Caribbean Basin definition as too heterogeneous, including as it does "Central America, Mexico, Colombia, Venezuela, and, indeed, the United States" (p. xi). Exclusive focus on the Commonwealth Caribbean, on the other hand, is seen as too narrow, as it leaves out Cuba, Puerto Rico, the Dominican Republic, and Haiti. Neither book deals with Associated States such as the other Francophone societies, Aruba and the Dutch Antilles, or with dependencies such as Anguilla, Bermuda, or the British and American Virgins presumably because one cannot properly speak of democracy in a non-self-governing territory. Yet both books contain chapters on Puerto Rico.

While we are on definitions, the following amalgam of formal or institutional criteria to define democracy represents the consensus between the books: regular, free and fair elections offering meaningful competition for government power, without use of force, through wide popular participation (universal suffrage and nonproscription of parties), with guarantees of civil and political rights (free speech, association, petition, and religion), plus an independent judiciary and respect for the rule of law. Evelyne Huber, in the Domínguez volume, emphasizes that substantive political and socioeconomic equality does not necessarily flow from fulfillment of the formal criteria.

Edie's volume goes beyond these criteria by including social justice, and emphasis on participatory democracy and mobilization to balance the typical Caribbean pattern of educated middle class control of the state through clientelism and racial politics. Here the difference in emphasis and tone between the books comes out most clearly. Edie's (and Hintzen's) stress on control by the bourgeoisie of the less educated masses as one of the chief legacies of the Westminster parliamentary model is countered by Domínguez (and Payne) focusing on its utility as a defense against authoritarianism. Both views have merit.

The books operate on a different scale. The Edie volume seeks to contribute to better understanding of Caribbean democracy, and perhaps to a theory of democracy in developing nations. It certainly achieves the first goal. The attainment of the second is debatable, especially since so little theory is discussed. The contributing authors for the most part remain content with spelling out the particulars of their assigned nation case study without, regrettably, even looking across the boundaries for illuminating comparisons. Thus the most that can be said is that the pieces may serve as a data base for someone to weave together more general propositions.

Ironically, the best theoretical contribution (beyond the Domínguez introduction, about which more presently) is Evelyne Huber's "The Future of Democracy in the Caribbean" in Domínguez et al. That book is offered as an amalgam between the two typical books on the Caribbean: those aimed primarily at specialists (which certainly seems to fit the Edie volume), and those addressed to crafters of a new Caribbean policy in the international system.

The Edie volume, however, geared so much more than Domínguez et al. toward detailed case analyses of internal politics, provides some of the more trenchant policy insights. For instance, we may extrapolate from Noguera's analysis of democratization and foreign intervention in Grenada, Nicaragua and Panama that "restoring democracy" in Haiti through external intervention is not likely to be successful.

Each book attempts more than the other, in different senses. The Domínguez volume presents more general analyses of problems such as drugs, corruption and violence, and economic performance. The Edie volume seeks comprehensiveness in scope: only four of fifteen chapters (beyond the introduction) are general commentaries. The rest are case studies, including Suriname, Cuba, and Haiti—the latter explicitly eschewed by Domínguez et al. because of their isolation from the rest of the region and the uncertainty about their future, as well as Cuba's experiment with statism which has "proven itself no more effective . . . than elsewhere . . ." (p. xii).

Given the special prominence in people's consciousness of Cuba and Haiti, the rationale of the sponsors, publishers and editors of the *Perspectives* volume for excluding them is both regrettable and rather thin, and

particularly surprising in view of Domínguez's well-known expertise on Cuba. Even more surprising is the absence of commentary on Cuba in the general-topic chapters where, except for brief mentions of its independence, only negative foci draw attention, e.g., the trial of highly placed Cuban authorities for corruption and drug trafficking (p. 50–51), U.S. security concerns due to the pro-Cuban stance of Nicaragua and Grenada after 1979 (p. 111), and, ironically, "Cuba is too big to ignore" (p. 274). Remarkably, the only reference to Fidel Castro in the Domínguez et al. volume is a clever wisecrack about Cuban change awaiting "either a heart attack or a change of heart" (p. 274). Without questioning the sincerity of the rationale for exclusion one might be forgiven for wondering what role ideological disposition had in the decision.

By contrast, Edie's volume includes excellent chapters on Suriname, Cuba, and Haiti. Betty Sedoc-Dahlberg's piece on Suriname is one of the best in the book. She is a seasoned and courageous analyst operating under extremely delicate conditions. Kenneth Boodhoo is no less so. His chapter on Haiti offers depth and perspective in these days of agonizing debate over international options for stemming human rights abuses and restoring the democratically elected Father Jean-Bertrand Aristide to power. Carollee Benglesdorf's chapter on Cuba is perceptive and balanced, offering keen insight into the post-Gorbachev (post-Perestroika) and post-Cold War restructurings of Cuba's economy and even party. She provides helpful perspective on the Fourth Party Congress in 1991 by comparing its discussion of decentralization and participation with the 1970s discussions of these issues. She concludes that "Decentralization is not a synonym for democratization, nor, for that matter, is increasing participation. . . . Indeed, no form of nascent opposition outside the party will be tolerated. The leadership's harsh . . . treatment of the tiny human rights groups . . . underscores its confiscation of the political arena" (p. 193-194). This is fine analysis, and extremely helpful in understanding the region. One hopes Domínguez et al. and Johns Hopkins will reconsider the Cuban exclusion for the second edition-by which time perhaps the original rationale will have disappeared anyway.

The best feature of either book is the Domínguez introduction. It is a masterful survey, genuinely comparative not only within the Caribbean, but also with South America, Africa and Europe. It systematically sets forth the gist of the contributions that follow while placing them in a contextual and conceptual framework. Not only does it relate Caribbean socioeconomic conditions across the board, but it gives the reader a comprehensive perspective on possible explanations. This is a model of what an introduction to a book on comparative politics of a region should be. It alone is worth the price of the book.

Domínguez's best contribution is perhaps his discussion of the "statist bargain," beholden to Keynes, according to which "democracies delivered material gain" with the state serving as midwife: "economic growth gave birth to social welfare," typically in the form of patronage for the party faithful. Capitalism and democracy married, with labor gaining rising living standards through accepting profits, markets and political expression, while business paid the price of accepting the welfare state in order to buy peace. Foreign consumers and governments helped pay the price in the days of

decent demand and prices for Caribbean export commodities and Cold War driven aid generosity. But those days are no more. The state can no longer afford the statist bargain. Whether liberal democracy can survive its end, says Domínguez, "could be the Caribbean Question for the 1990s" (pp. 11–12, 22). At the other end of the book Robert Pastor and Richard Fletcher, in an edited version of their Foreign Affairs (1991) article, take up that policy question and deliver a set of recommendations that may in the future be seen as the post the Caribbean states rounded on their way from economic decline and threatened instability to peace and relative plenty. Remarkably comprehensive, the plan calls for 'self-reliance that is outward-, not inward-, oriented," with U.S. support in terms of opening markets to exports, "perhaps as part of a broader extension of a North American Free Trade Area" (p. 257). The book thus begins and ends with major contributions. That the material flanked by these offerings has a hard time chinning up to that level is hardly surprising.

The issues of security and future instability emerge in several forms in both books. The Domínguez volume, being more (U.S.) policy oriented, expresses more extensive concern. Such a discussion is not standard operating procedure in comparative politics, but entirely appropriate in a region characterized by small states in a strategically sensitive location that has drawn unwelcome present and past U.S. and European external dominance.

The books come up with different answers on invasion for democracy. Domínguez sees what is now clearly "a pattern of international behavior in this subsystem" of intervention "on democracy's side" as more than good luck (p. 18). Edie, citing Noguera's chapter on U.S. interventions in Grenada, Nicaragua and Panama, asks rhetorically, "Can democracy still continue to be a convenient pretext for the United States to extend its power in the Caribbean" (p. 5)? (Although the books do not, and could not, address a Haiti invasion since they were published before that became an option, we may infer from Noguera's analysis that "restoring democracy" in Haiti through invasion is not likely to be successful. He points to a "rapid decline in U.S. interest toward countries it has invaded or destabilized" [p. 228].)

Anthony Maingot, in the Domínguez volume, makes a major contribution by arguing that drugs and corruption present the most severe security threat to Caribbean mini-states that appear more concerned about preserving "sovereignty" than about coordinated international action against what clearly is an internationally coordinated challenge. Such a redefinition of security in this region is long overdue.

There are several regrettable features about these two good books.

(1) The Edie volume occasionally sounds intolerant in not giving the benefit of the doubt to fellow scholars' or political practitioners' intellectual integrity. Thus, for instance, mobilization is seen as "thinly veiled attempts to camouflage the reality of middle-class power" (Hintzen, p. 20). The "mere existence of the formal mechanisms of democracy such as political parties, periodic elections, and representative bodies" is contrasted with "genuine representation" (Edie, p. 11) in a way that appears to assume the non-genuineness of the practitioners of the formal mechanisms. And scholars like Samuel Huntington are lambasted for having produced "accommodating reformulations," creating the

modernization approach emphasizing order rather than democracy when dictators in LDCs turned out to be pro-Western in the Cold War (Hintzen, p. 9–10). Interestingly, considering this attack, Domínguez cites his Harvard colleague positively (p. 6).

Huntington's early work *Political Order in Changing Societies* (1968), the usual focus of his detractors, may indeed have overstated the priority of stability and order. However, let us acknowledge its brilliance and assume his sincerity in trying to find answers to extremely complex problems while noting that all of us are prone to modifying our thinking according to our milieu, our understanding of the world, and our ideology. Those who are prone to leftist social critique are doubly vulnerable to the temptation of "vanguardism," with which we now feel comfortable charging the New Jewel Movement. Caribbean scholars who exhibit such neocolonial traits should be embarrassed.

(2) It is shocking that neither book contains a map of the region, this despite Richard Bloomfield's assertion in the Foreword to the Domínguez volume that "the same characteristic that has made the Caribbean important to the United States in times past—its geography—will make it impossible to ignore in the future . . ." (p. viii). All comparative politics books should enable the reader to lay a spatial foundation for comprehending the subject. Needless to say, that applies at least as much to IR books.

(3) For whom are the books intended? Apparently fellow academics, graduate students, and policy makers. There seems to be slim concern with accommodating the undergraduate student or general reader, especially in the Edie volume, where unnecessarily complex language is used by Edie herself in her Jamaica chapter and by Hintzen in his Trinidad and Tobago study. Vaughan Lewis in the Domínguez volume also excessively obfuscates and complicates his subject. By contrast, it is sheer pleasure to read the prose of Franklin Knight in the same volume.

(4) Finally, an interesting set of questions about the role of the state are raised by implication in these books, but not answered. Is there a relationship among (a) the (vanishing) statist bargain, (b) the perceived need to protect the sovereignty of mini-states from international domination through coordinated action against drug cartels, and (c) the unwillingness of analysts to raise questions about the assumption that the traditional state model is still the most relevant and appropriate one for the twenty-first century? Euraque and Niemann's pioneering "Regional Economic Integration in the Periphery: A Comparison of Central America and Southern Africa, 1870–1990" (Southern African Perspectives 31, 1994) calls upon us to raise such questions about the proper role of the state, to explore regional solutions and, accordingly, not to accept sovereignty incantations at face value. Those of us with more than passing professional interest in the Caribbean have some work to do in

Having said all that, it is well to reiterate: these are good books that should be wisely read. In their own way, and especially together, they contribute significantly to our understanding of democracy in the Caribbean, democracy in general, and the Caribbean in general. That is no small feat.

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The Elite Connection: Problems and Potential of Western Democracy. By Eva Etzioni-Halevy. Cambridge: Blackwell, 1993. 239p. \$39.95 cloth, \$19.95 paper.

Etzioni-Halevy intends The Elite Connection as a text or supplement for political science and sociology courses, as a contribution to democratic elite theory, and as a warning about threats to democracy. The first four chapters review and criticize many theories of democratic politics and assert the advantages of democratic elite theory. The next two chapters present Etzioni-Halevy's demo-elite perspective, stressing "relative elite autonomy" from domination by other elites, especially state elites, as the overarching metaprinciple of democracy and highlighting the egalitarian potential of elite autonomy. The last four chapters apply the demo-elite perspective to a number of empirical cases: democratic stabilization in Britain, democratic breakdown in Weimar Germany, the recent democratic transitions in Poland and Russia, and recent politics in Western democracies illustrating both the threats to democracy by elite actions that reduce autonomy—"subjugation" (e.g., union bashing) and "collusion" (e.g., government-business coziness)-and the egalitarian possibilities stemming from social movements and established elites' accommodating responses.

For students, the book provides a reasonably comprehensive overview and critique of social-scientific theories of democracy and a succinct statement of the demoelite perspective. However, undergraduates would find these six chapters opaque, because the discussions are rarely tied to empirical examples. By the time empirical cases are discussed in the last four chapters, the typical student will have drowned in a sea of metatheory, and the cases are often treated so briefly that most undergraduates would need supplementary information. Graduate students, on the other hand, should find the literature review useful (though not definitive) and the theoretical explication and applications thought-provoking.

As a scholarly contribution, the book makes some important points that others have made but that need repeating and developing. Etzioni-Halevy persuasively challenges negative images of elite theory, asserting that its presumed flaws are not inherent, especially not in her demo-elite version. Elites are not viewed as superior beings: they simply have great power and influence because they command resources—coercive, material, organizational, symbolic, and "psycho-personal" (e.g., charisma). Elites include leaders of protest movements, which gives the theory an egalitarian bent. Elites are not omnipotent; they must contend with "subelites" and the "public," which have significant influence in a democracy. Moreover, Etzioni-Halevy highlights the vital point that most competing theories erroneously attribute motives and actions to abstractions (classes, interest groups, organizations, civil society). Elite theory's distinct advantage is that its actors are real people.

Etzioni-Halevy locates her core idea of elite autonomy in the liberal concept of the separation of powers and in the works of Weber, Mosca, Schumpeter, and Aron, which emphasize countervailing elite power as the foundation of democracy. Their mistake was failing to challenge economic inequality, which her demo-elite perspective avowedly corrects but actually says little about. Elites (and subelites) are "relatively" autonomous (never absolutely) when their resources are not controlled by

elites outside their boundaries. Relative autonomy is evidenced mainly by elites confronting other elites—for example, by working against their interests or undermining their legitimacy. Etzioni-Halevy tries to distinguish her approach from those emphasizing elite consensus on "rules of the game," arguing that elite cooperation is more important to democracy and that cooperation rests primarily in elites' self-interested desire to preserve the system. Still, she admits that "elite and sub-elite cooperation in a democratic regime may well require some consensus about the desirability of democracy through free elections and civil liberties as such" (p. 110).

Many scholars accept that democratic stability depends on all major elite factions' feeling secure from attack by a hegemonically inclined elite. Relative autonomy helps describe this condition. However, I doubt that making it the core concept is a theoretical advancement. For one thing, Etzioni-Halevy's main empirical indicators of autonomy are rather vague. In reading them, I kept thinking that Western democratic elites are frequently less autonomous-more interdependentthan these descriptions suggest; and I do not think this typically amounts to subjugation or collusion. Military elites (the clearest example) are substantially less autonomous than her abstract descriptions imply. The military also illustrates difficulties with her interest-based conception of autonomy: many analysts think the general elite commitment to values requiring military subordination to civilians constitutes a major reason for democratic stability; but in distinguishing her contribution from previous works, Etzioni-Halevy unconvincingly downplays elite values.

Furthermore, while she argues persuasively that elite autonomy or its absence helps explain Britain's successful democratization, Germany's democratic breakdown, and democracy's greater promise in Poland than in Russia, she does not say why autonomy emerged in one context and not another. She comes closest in comparing Britain and Germany, stressing the "different stature of the state" (p. 148). After the sixteenth century, British state power was gradually checked, which did not happen in Germany. But one still wants to know why and why not. That requires probing deeper into elite structure and values than Etzioni-Halevy seems willing to go.

Additionally, her analysis of elite autonomy in contemporary Western democracies contains some troubling elements. She sees established elites' autonomy as weakened in the 1980s by instances of subjugation and collusion, making democracy less secure. While giving plausible examples, she lacks evidence that these more seriously threaten elite autonomy than past instances. She then argues that the proliferation of social movements in Western democracies since the 1960s and established elites' cooptative responses have made these democracies more dynamic, more flexible, more egalitarian—seemingly contradicting her "warning" that democracy is less secure today. A final difficulty with Etzioni-Halevy's emphasis on elite autonomy is that while acknowledging the need for elite cooperation, she mainly sees it as threatening autonomy. Yet a fundamental problem for Western democracies today is that elites are so bent on advancing their autonomy and interests that they cannot cooperate to serve the com-

Despite these shortcomings, Etzioni-Halevy manages to steer rather clearly through some exceedingly murky

waters and ultimately makes a good case that relative elite autonomy is at least an intuitively useful concept for thinking about the foundations of stable democracy.

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Politician's Dilemma: Building State Capacity in Latin America. By Barbara Geddes. Berkeley: University of California Press, 1994. 246p. \$35.00.

Barbara Geddes's book is an ambitious attempt to apply collective action and rational choice arguments to efforts at reforming the state in Latin America. In particular, she is concerned with explaining why state reform takes place under some circumstances and is thwarted under others.

This is a significant question given the importance the state has assumed in Latin American development since the 1930s, and also given the growing conviction that a healthy state is a sine qua non for successful development strategies. Yet this question has not been systematically explored, notwithstanding the existence of some good monographs on individual countries. Indeed, in general the state has been a poorly researched topic despite its importance. Thus a first key contribution of this book is the search for a systematic comparative answer to a major research question.

Geddes assumes that 1) under democracy, whether state reform occurs depends primarily on politicians and 2) politicians are self-interested rational actors, primarily concerned with their own reelection. Therefore, the viability of state reform depends primarily on the extent to which it is in politicians' self-interests.

The dilemma from which the title is taken is that on the one hand, politicians depend on selective incentives (patronage) to build up their political constituencies and enhance their chances for reelection. On the other hand, an effective state must be somewhat insulated from politicians in order to promote development. How, then, will politicians behave? Will they thwart or support civil service reform and other measures that would presumably strengthen the state but reduce patronage opportunities?

Geddes convincingly claims that looking at this issue as a collective action problem leads to largely pessimistic conclusions regarding prospects for state reform. Nevertheless, as the game theoretic literature on cooperation suggests, cooperation for a better collective outcome (i.e., state reform) is not impossible. These broad arguments build in intelligent fashion upon the works on collective action and cooperation. Geddes then devotes one lengthy chapter to the Brazilian case (pp. 43–82), which underscores the contentions about the difficulties of state reform while also pointing to specific cases of effective bureaucratic insulation.

Much of the rest of the book argues that institutional arrangements and political contexts determine whether rational politicians would be more or less likely to support state reform (especially civil service reform). Two chapters look at the logic of legislators in supporting or thwarting state reform. Beginning from a prisoner's dilemma model, Geddes deduces that a majority of politicians will support reform only if there is a situation of relative parity among the parties. Given such parity, the members of different parties will more or less equally lose patronage opportunities and thereby avoid losing a

comparative advantage over electoral competitors, while simultaneously reaping the small benefit gained from supporting state reform. Geddes then tests this model with data from Colombia, Uruguay, Venezuela, Chile, and Brazil.

This argument on why legislators support or oppose state reform is problematic for three reasons. First, Geddes assumes that the benefit of supporting reform (e in her model) is quite small. But if e increases in value, then members of the majority party or coalition would support reform even in a situation of non-parity. So the conclusion that reform will be supported only in situations of relative parity rests on assuming that e is low.

It seems likely that e rose considerably relative to the historic pattern with the crisis of the state in most Latin American countries in the 1980s and early 1990s. Far reaching state reform occurred under democracy in Argentina (1989–present) and Bolivia (1985–89). Less sweeping but still significant reform occurred in Venezuela (1988–93), Brazil (1990–92), and Peru (1990–92). None of these cases approximated the conditions Geddes stipulated as favorable or necessary for state reform. Stated in more prosaic terms, it is possible that broad regional trends are more important than parity among parties in shaping prospects for state reform.

Second, the model is predicated on the assumption that politicians' only interest is (re)election. Granted, this assumption is made in the name of parsimony and simplification, but there are good reasons to wonder how reasonable it is for many Latin American cases, and therefore whether a model based on that assumption would prove compelling. In Brazil, for example, 30% of incumbent deputies regularly do not seek reelection, making it hard to claim that reelection is their motivation

for policy positions.

Third, Geddes assumes that access to patronage is crucial for most politicians' chances of winning reelection. As an assumption made in the name of parsimony, this makes sense for Colombia, and to some degree Brazil and Chile. In Colombia, party organizations have little control over the nomination process, and election depends primarily on each politician's linkages to constituencies. Because of the open list system in Brazil, politicians of the catch-all parties typically rely extensively on patronage. Chile also has an open list, providing an incentive for politicians to develop their own clienteles via patronage. But relatively disciplined and centralized parties have controlled the nomination process, making politicians also dependent on party elîtes. Venezuela until recently had a closed list system for the lower chamber, so reelection depended more on a politician's position on the electoral list-and hence on her/his good relationships with national or provincial party bosses—than on patronage. Uruguay also has a closed list system, albeit one in which dozens of party factions present their own list, making candidates dependent on faction leaders for their reelection. Moreover, television has increasingly displaced patronage as a factor in senate elections in Brazil, Chile, and elsewhere. In brief, the extent to which politicians depend on patronage for election in these five countries varies considerably according to institutional arrangementsas Geddes herself powerfully argues elsewhere in the

After examining the logic of legislators contemplating state reform, Geddes also analyzes presidents' appointment strategies. Do presidents primarily make appointments to achieve administrative efficiency or to satisfy "hungry parties?" She argues that presidents' strategies depend on features of their political environment. More specifically, presidents who are creating their own party vehicles are likely to concentrate appointments among party and personal loyalists, with less regard for administrative competence. Presidents from well organized parties generally prefer to maintain personal control over appointments rather than delegating them to the party. Finally, presidents who won the nomination despite the opposition of most party leaders, or who are independents, are likely to make appointments on the basis of personal loyalty or merit and to eschew party based nominations.

Geddes argues persuasively that three factors affect presidents' ability to follow their preferred appointment strategies: the legislative strength of their party, the degree of party discipline, and the amount of competition the president faces in her/his own party. The micrologic she ascribes to rational actors is generally compelling, and she presents empirical evidence showing that her model is quite powerful.

Although many parts of the argument are questionable, this is an important and intelligent book on a timely subject. It is among the best rational choice works on Latin American politics.

University of Notre Dame

SCOTT MAINWARING

The Portuguese Military and the State: Rethinking Transitions in Europe and Latin America. By Lawrence S. Graham. Boulder: Westview, 1993. 153p. \$34.95 paper.

As usual, U.S. policy in Latin America and many of the scholars who write about the area have it wrong—in this case concerning the role of the military institution, civil-military relations, and the strategy to be used to help consolidate transitions to democracy. Americans, both scholars and policymakers, often tend to have a myopic, wishful, and ethnocentric view of Latin American—and, perhaps by extension, southern and eastern European-civil-military relations in which the U.S. system is the model and all other countries must try to emulate the U.S. pattern. But of course, the U.S. model of strict subordination of military authority to civilian leadership is not the only model of civil-military relations (even in the United States, not the most prevalent form) and probably not exportable. It is the strength of Lawrence Graham's new book on the Portuguese military that not only does he point out the inadequacies of the U.S. model for countries where the level of development and the institutional setting are far different, but he also shows how the Portuguese case may constitute an alternative model especially suited for the similarly fragmented countries of Latin America and Eastern Europe.

At least two major problems come to mind in thinking about the relevance of the American military model to Latin America. First, outside of the Southern Cone (Argentina, Chile, Uruguay), there tend to be immense social, cultural, economic, and racial gaps between the military officer corps and the civilian elites. The civilians, mainly white, tend to look down on the military as "peasants with guns," useful in constituting a political alliance but never to be entertained in one's home.

Meanwhile the officer corps, usually mestizo or mulatto, often disdains the haughty civilian elites and seeks to use the military career as a route to power and wealth—which usually flow from seizing the national palace. Where a U.S. model of civil—military relations fits in this charged, social-racial context is often hard to discern.

The second aspect of this dilemma is institutional, and that is where Graham's book makes its major contribution; for the fact is that in southern and eastern Europe and Latin America, the military constitutes a separate institution, almost a fourth branch of government. While enjoying its own special rights and responsibilities in a corporate and quasi-feudal sense, it is also integrated with the state and with political parties and interest associations in many and complex ways. To expect that these complexities can be subsumed under the simplistic slogans of "military subordination to civilian authority" or "educating the Latin militaries to their proper role" is naive and ignorant of historical or current realities.

Our expectations in these regards have been raised by the apparent successes of the Argentine, Chilean, and Spanish cases in subordinating (for the most part) the military to democratically elected governments. But as Graham points out, these societies are relatively homogeneous and prosperous and reached a widespread consensus on the goals to be achieved. In more fragmented societies, however, which are ethnically and socially stratified and often lack consensus on ends or means (the majority in Latin America and Eastern Europe), this model may not apply—hence Graham's argument that for *most* countries seeking to make the transition to democracy, the Portuguese case may be more relevant than any others.

Graham portrays Portugal since the 1974 revolution that destroyed the old Salazar regime as a highly fragmented society with a variety of groups—civilian and military, often overlapping—competing for political power. It is not sufficient, Graham says, to comprehend Portugal only in terms of the reconstitution of civil society and the emergence of civilian leaders and political parties with democratic values. Instead, one must also understand, empathetically, the military as a prime and independent actor, as a main national institution, and as a catalyst for change. In addition, one must view the state not just as a unitary institution but as a vast bureaucratic structure with distinct civilian and military "households."

Graham is masterful at tracing the different military and civilian factions in Portugal since 1974, how they overlap as well as separate, their interpenetration, and their virtually constant negotiations and accommodations. Elections in this context were like opinion polls: they showed the direction of popular sentiment and offered some legitimacy to the winner, but the results were often viewed as tentative and as offering only a partial mandate. Of greater or at least equal importance were the factional disputes that involved both civilian and military elites, the personalistic rivalries, and the series of tentative but always evolving political pacts that represented each new adjustment of the balance among them. Meanwhile, a gradual evolution toward a more consolidated democracy and greater civilian control did occur. But note also the tentative language in which the accomplishments are expressed, since the military-civilian (and other) negotiations are still ongoing.

Clearly this process and complexity cannot be sub-

sumed under the too-simple notion of military subservience to civilian authority. Note, too, how close is Graham's model of a "bureaucratic state" constantly adjusting, accommodating, coopting, and balancing new elements to the model of "multiple power contenders" first set forth by Charles Anderson in the 1960s and to the "corporative" or "organic state" models of the 1970s. Graham has rediscovered some old truths, but his statement of them is nonetheless vigorous and original and adds detail and insight to our understanding.

Nevertheless, Graham fails to slay the whole dragon. While highly critical of the standard, U.S.-based system and model of civil-military relations in terms of its applicability to other nations and culture areas, his conclusion backs away from fully elaborating the new and innovative model that his own analysis would seem to suggest. One final criticism of this otherwise extremely significant book needs mentioning, and that is that it is often stiffly written and sometimes too elliptically argued. One almost needs to know Graham's own research journey (Brazil, Portugal, Angola, Mexico, Romania, Yugoslavia) and his previous writings to appreciate fully the arguments he makes here.

University of Massachusetts

HOWARD J. WIARDA

Orpheus and Power: The *Movimento Negro* of Rio de Janeiro and São Paulo, Brazil, 1945-1988. By Michael George Hanchard. Princeton: Princeton University Press, 1994. 203p. \$29.95.

While long a staple in discussions among political scientists working on the United States, race has not figured prominently within the work done on Latin America by political scientists. This is a gap that Michael George Hanchard seeks to fill in Orpheus and Power. Quite appropriately, the central question Hanchard addresses is why no Afro-Brazilian social movement of national scope developed in Brazil in the post-World War II period. To answer this question the author adopts a Gramscian framework, which is favored on the basis of its ability to emphasize and grasp the cultural dimension of racial politics. After introducing the book's conceptual framework and presenting an overview of the debate on Brazilian racial politics in Chapters 1 and 2, the author starts to present his argument. The lack of a social movement among Afro-Brazilians, Hanchard states, is due to the "racial hegemony" embodied in the ideology of Brazilian "racial democracy" that began to take root in the early part of the nineteenth century. Racial hegemony, by promoting "discrimination while simultaneously denying its existence" effectively neutralized racial identification among Afro-Brazilians and made mass mobilization unlikely (p. 6).

To develop this argument the author first traces, in chapter 3, the origins of the notion of racial democracy, turning primarily to the writings of authors such as Gilberto Freyre. In the next three chapters more empirically-oriented material is presented. The author draws upon archival material and interviews, particularly with activists from Rio de Janeiro and São Paulo. Because of the lack of any phenomenon that could be called a social movement of national scope, this book lacks the unifying focus the civil rights movement, for example, provides U.S.-based studies on race. Instead, a variety of incidents from everyday life, as well as the potentially

most consequential manifestations of Black identity, are considered. The material is cogently presented and the story persuasively developed. The author shows how the vision of racial democracy impeded recognition of racially specific problems, which in turn undermined group solidarity around race and race-based mobilization of national scope.

This accomplishment notwithstanding, there remain many loose ends of a conceptual nature in this book. Hanchard may be right in stating that hegemony is a useful concept in that it allows one to grasp the cultural dimension of racial politics. Group identity, indeed, can be thought of as a precondition for collective action. But, as the author himself acknowledges, "the collective identification with a particular group or collectivity is an insufficient basis for political mobilization" (p. 98). A theory of collective action, in other words, involves more than the issue of collective identity. Recognizing this point, the author points to the need to look beyond the cultural dimension of racial politics and to consider a "broader social totality" and see "cultural practices" as "part of whole social processes—at once ideological, cultural and material" (p. 98, 139). Unfortunately, this sort of cryptic comment, never really spelled out and clarified, does little to shed light on a complex conceptual problem. The link between group identity and collective action thus remains untheorized.

What is most glaringly absent is a concerted effort to consider the link between national politics and potential social movements. In other words, there is no sustained attempt to consider the strategic dilemmas presented for incipient social movements by different patterns of national politics, a factor closely linked to the likelihood of social movements being formed. To be fair, reference is made to actions being "informed" or not by the "new logic of the abertura," that is, the political opening whereby Brazil moved from military rule to democracy starting in the mid- to late-1970s (p. 150). But such comments are made only in passing and do not form part of a theoretically guided consideration. There is simply no attempt to review and draw upon the insights that the voluminous works on transitions from authoritarian rule and democratic consolidation have produced and to consider how they could be incorporated into the study of race relations.

The main theoretical problem of *Orpheus and Power*, then, is that it focuses primarily on cultural politics. If institutional politics is of little importance to the study of race relations because, as Hanchard states, "much of Brazilian racial politics occurs outside of channels of representative democracy," this is something to be considered and assessed (p. 25, 57). And such an assessment can only be made if institutional politics is not brought in in an ad hoc fashion but as part of a conceptual framework that seriously addresses this dimension of politics. While this may be a theoretically challenging enterprise, there certainly is no good argument for the a priori rejection of the insight developed in various bodies of literature that stress how political institutions play a role in defining how groups become structured and relate to one another.

Such a theoretical critique does not mean that we must start from scratch, because this line of thinking is developed within the recent theoretical literature on social movements. Indeed, this body of literature, developed by sociologists as well as political scientists such as Sidney Tarrow, has clearly emphasized the need to

conceptualize the linkage between national politics and the strategic choices faced by potential social movements. This literature, then, seems to be the natural frame of reference for a study that purports to analyze social movements in Brazil. But the entire body of literature on social movements is summarily dismissed (p. 157-58). The problem for Hanchard is not that he is opposed to cross-disciplinary work, as his advocacy of a political-anthropological" approach indicates (p. 27). Hanchard's rejection of social movement theory, rather, is based on the lack of attention in this literature to the specific problem of race. This depiction of the social movement literature is accurate, to a certain extent, but to fully reject the relevance of social movement theory to race-based social movements entails a fundamental error: to assume what must be shown. If racially based social movements operate in a different manner than gender based or ecological social movements, this is something to be established through a general theory of social movements. In other words, the distinction between race and gender, or race and ethnicity, is something to be established through theoretically guided work, not through pre-theoretical assertions. Until this has been done, there is no reason not to see different social movements as sharing some basic characteristics inasmuch as they all seek to challenge the status quo.

Because of these shortcomings, this reviewer would characterize Hanchard's effort to fill the race gap in political science work on Latin America as one that meets only mixed success. If Hanchard rightly points out that Alfred Stepan's (1989) edited volume Democratizing Brazil could be blamed for not touching upon the impact of race on the process of democratization, his own work in effect replicates the mistakes of those he criticizes and does nothing to build bridges between mainstream political concerns and the study of race relations (p. 8). In some sense, then, we are back where we started: rather than a Latin American-based political science that ignores race we are given a study that focuses on race to the exclusion of the dynamics of democratization. This failure to engage the work done by political scientists on the regional move toward democracy is not fatal, but it does limit the likely impact of Orpheus and Power. Most damagingly, it is unlikely that it will affect the democratization debate to any great extent. Its significance, then, will be restricted to its contribution to the debate on Brazilian race relations and the comparative study of race relations. In this sense, Hanchard provides a worthwhile addition to the revisionist historiography that has questioned and sought to dispel the image held in the literature for some time that Brazil was a racial democracy. Countering this image, the author shows how the notion of racial democracy has prevented the politicization of racial issues and a frontal attack on the problem of racial discrimination.

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GERARDO L. MUNCK

The Health of Nations: Public Opinion and the Making of American and British Health Policy. By Lawrence R. Jacobs. Ithaca: Cornell University Press, 1993. 259p. \$34.50.

The national debate on President Clinton's health care plan has once again put the spotlight on America's

unique mix of welfare state policies. Unlike other advanced industrial countries, America has no national health insurance and its income maintenance and social service benefits are comparatively much lower. The underlying reasons for America's distinctive welfare state is a source of long-standing debate within political science. Some scholars suggest that America's liberal political culture fosters a distrust of the state which makes major government social welfare initiatives very difficult. This cultural explanation of politics and welfare state differences has been challenged by state-centered theorists who contend that state administrative capacity and government officials play key roles in shaping welfare state policies. In this view, America's fragmented, "weak" administrative structure is a serious obstacle to sweeping policy change and reform, especially in the face of well-mobilized groups antagonistic to the welfare state. By contrast, many European countries with their large centralized bureaucracies are capable of acting independent of societal interests.

In his excellent, timely book, Jacobs develops an innovative theoretical framework which builds upon the cultural and state-centered theories. His book is a careful, comparative study of two historic health care reforms—the British National Health Service Act of 1945 and the Medicare Act of 1965 establishing America's health insurance program for the elderly. He suggests that state-centered theorists (whom he terms Weberians) cannot adequately explain the British or American experience with these two major health care reforms. While British policymakers did nationalize the entire health care delivery system, they made major concessions to key interest groups such as the British Medical Association even though Britain had a powerful health ministry. In the United States, policymakers often resisted the demands of influential medical producer organizations, despite the weak administrative capacity of the national health bureaucracy. Further, Medicare represented a significant expansion of government's role in health care.

For Jacobs, the key determinant of the outcome of these two health care reforms was public opinion rather than state capacity. In Britain, the citizenry had learned to trust the state through decades of progressively more extensive government involvement in social welfare. Initially, British policymakers resisted the idea of a government-run health system and universal coverage. But public opinion pushed policymakers in this direction during the war years. With the landslide Labor electoral victory of 1945, the government had the political opportunity to respond to public opinion through the creation of the National Health Service.

In the United States, most citizens were highly skeptical of a major government role in health and social policy. Throughout the 19th century and the early 20th century, the federal government's involvement in social and health care policy was quite limited. Public social programs at the state and local level were viewed as inferior and reserved for the indigent. Jacobs observes that policymakers in the 1930s and 1940s failed to enact comprehensive health care reform because it was out of touch with the citizen's distrust of the state. In the late 1950s and 1960s, politicians responded to public opinion with a modified reform package, restricting benefits to the aged and emphasizing private administration. Johnson's landslide victory of 1964 gave him the votes he needed to push the Medicare legislation through Congress.

In this book, public opinion represents a country's political culture which evolves over a period of time as citizens interact with political institutions and private organizations. Yet Jacobs does not simply regard policy as the "epiphenomenon of culture" (p. 227). Instead, policymakers' reactions to pubic opinion are filtered through policy networks. Powerful producer groups had privileged access to government officials which affected the content of British and American health legislation. Elections profoundly affected the timing and character of legislation. Political culture, in other words, does not predetermine policy outcomes. Politics is about contest and different groups vying for power. Policymakers are sensitive to public opinion but the give-andtake of the political process means that political culture does not influence policy outcomes in a straightforward

Jacob's book is a valuable contribution to political science for three important reasons. First, his analysis is a creative re-thinking of prevailing theories of welfare state development. As he points out, comparative research on the welfare state has tended to neglect public opinion or use rather simplistic notions of political culture. His book introduces a sophisticated model of the relationship between public opinion, political institutions, and policy outcomes that will be of great interest and utility to future researchers.

Second, his discussion of public opinion is relevant to our understanding of contemporary domestic policy. Some theories of American politics suggest that public opinion sets the broad parameters of policy debate, leaving elite policymakers broad discretion to decide the content of policy. With this latitude, policymakers may be highly responsive to well-organized producer groups, even if the goals of producers are at variance with popular sentiment. Jacobs provides a needed corrective to this view. He suggests that for highly salient issues with substantial citizen interest, policymakers are frequently able to resist even fierce producer group pressure. Producers' groups are more likely to be successful on issues characterized by citizen apathy or disinterest. In these situations, elite policymakers are unlikely to see a political risk in making concessions. This dynamic has recently been in evidence in high profile issues such as drug policy whereupon Congress has tended to disregard political pressure from professional and service provider associations.

Third, the book contributes to our understanding of health policy formation. In this century, several failed attempts have been made to enact universal government health insurance in the United States. We are, as of this writing, trying once again. Jacobs' compelling, detailed research offers insights into the pitfalls and dilemmas facing contemporary policymakers as they try to craft politically feasible health care reform.

Jacobs' analysis is bound to spark healthy debate within political science. Skeptics might argue that the sweeping National Health Service Act stands in stark contrast to the Medicare Act which left the American health care system essentially intact and limited government insurance benefits to the elderly. Would a larger, more powerful health bureaucracy at the national level have fostered a more comprehensive solution to health care?

Also, the current contrast between Britain and the United States in health care is quite striking. In the last few years, Britain has instituted far-ranging—some

would say radical—changes in its health care system, closing large hospitals, introducing market competition among hospitals, and limiting the power of some producer groups. This restructuring of health services has been implemented by the Conservative government despite broad public unease and the concerted opposition of the Labor party. How can we account for the relative speed and comprehensiveness of the British reforms? In the United States, we struggle to enact universal health coverage even with substantial public support. A plethora of health care reform proposals are pending in Congress, making the prospects for the enactment of any legislation very uncertain. How is public opinion mediated by key political institutions to shape the various bills and the public debate on health care?

The Health of Nations should be required reading for anyone interested in answering these questions. More generally, this important book should be on the bookshelf of any scholar interested in the development of the British and American welfare state, comparative politics, and the impact of public opinion and political culture on public policy.

Duke University

STEVEN RATHGEB SMITH

Language Repertoires and State Construction in Africa. By David D. Laitin. New York: Cambridge University Press, 1992. 205p. \$42.95.

David Laitin is a comparativist of remarkable breadth of intellectual scope and research experience. Relatively few scholars carry out field inquiry in more than one world region, and hardly any in so many diverse sites as Laitin has explored. Somalia, Kenya, Nigeria, Ghana, Spain, India, Estonia, Bashkortostan: skilled trackers can spot Laitin footprints in all of these strikingly different locales.

Equally diverse are his conceptual sources: Wittgenstein, Gramsci, Weber, and Geertz have supplied important theoretical resources for his earlier books (Politics, Language and Thought: The Somali Experience, 1977, and Hegemony and Culture, 1986). In the present work, he makes creative and effective use of game and rational choice theory, and draws extensively upon sociolinguistic research. the result is an elegant and compelling analysis of language politics and policy choice in Africa, by a long distance the most comprehensive undertaken in any field.

Laitin examines language use patterns throughout the African continent, demonstrating the dialectic between state policy choice and language market forces. But the comparative African analysis is constantly enriched by insights drawn from other settings, both those bearing intriguing similarities (India), and those radically different (Switzerland, Japan). Extended vignettes offer informed summaries of language processes in a number of countries (Morocco, Ghana, Nigeria, Zaire, Central African Republic, Kenya, Zambia, Zimbabwe). In short, the book—although of brief compass—is remarkably broad in its coverage.

The core thesis—entirely persuasive—is that the *longue duree* linguistic equilibrium in most of Africa is what Laitin labels the 3+/-1 outcome. This means a stable multilingual society, with a hierarchy of speech domains. The language of the former colonizer is likely

to retain its status for many governmental purposes, higher levels of education, and as a medium of elite communication. One or more lingua francae will occupy an intermediate (or perhaps co-equal) position, be officially recognized as "national" languages, be promoted in the educational system (except at the university level), be used for some official purposes, and be praised in populist discourse. Many (though not all) vernaculars will hold their own, remaining as vehicles for local, kin milieu and household communication, often used in the first years of education. For those dwelling outside their area of origin, social necessity may require acquisition of the local vernacular; those who speak the lingua franca as mother tongue will only need command of two languages: thus the Laitin +/-.

Many language planning activists would have it otherwise; African language specialists and populist politicians have often entered impassioned briefs for elevating an indigenous language to exclusive national status. Laitin astutely deploys game theory to demonstrate why such efforts are almost certain to abort, as they did in India. State policy choices are important, and certainly have an effect. However, any policy prescription which overlooks the social marketplace is doomed to failure, and indeed may be counterproductive (as illustrated by the fiasco of the Sekou Toure effort to impose six Guinean languages as official tongues to be enforced through the educational system). Strategic calculus of the ordinary citizen enters the picture; what language repertoires are important for social mobility and economic advantage? And why should one not expect to acquire three languages (more or less), if this is the "normal" pattern?

State language policies tend to be burdened with the premise of a monolingual natural end point, one of the axioms of "national integration" as sacred vocation of the contemporary state. Many historic states have indeed largely accomplished this; Italy, at the moment of unification, had no more the 3% of its populace which spoke the official version of Italian, and no more than 10% understood it. But "successful" language policies are not necessarily transferable across world historical time and space.

Nor, Laitin argues, is there any reason to assume that the 3+/-1 formula is politically risky. Indeed, its greatest virtue is that it recognizes and endorses a reality produced by a citizenry unobtrusively registering in the social marketplace its own consumer preferences. Perhaps the greatest threat to its stability might come from active recognition and promotion of local vernaculars in preference to the lingua francae at the local level; one sees some trace of this in Nigeria and Ethiopia, where one hears claims in language and other spheres that public goods belonged only to "indigenes" of the state or region, and that "strangers" from other parts of the country should depart.

In the use made of game theory, Laitin acknowledges the important limits of rational choice concepts. He assumes disequilibrium to be a natural condition, and recognizes the primordial aspects of language as a component of social identity. He recognizes as well the social construction dynamic in language processes and politics. In freeing game theory from the shackles of unrealistic simplifying assumptions, which so often mar the work of its practitioners, he makes of it a genuinely useful analytical instrument.

In a mere 164 pages of text, Laitin sketches an amaz-

ingly comprehensive portrait. In its scope and succinctness, this comparative study is a model. More fundamentally, the volume is a masterful analysis of one of the most important aspects of identity politics in Africa and elsewhere. It is must reading for all students of African politics, as well as those concerned with the comparative politics of cultural pluralism.

University of Wisconsin, Madison

CRAWFORD YOUNG

The Sources of Social Power, Volume 2: The Rise of Classes and Nation-States, 1760-1914. By Michael Mann. New York: Cambridge University Press, 1993. 826p. \$79.95 cloth, \$24.95 paper.

Michael Mann has produced a second massive tome in what is now to be a four-volume series on the sources of social power. Unlike the last volume, which ranged from "the beginning" to 1760, this one covers a mere 154 years—the "long nineteenth century" of "mostly peace," bracketed at the beginning by the Seven Years' War and at the end by the World War I. Unlike the first volume, which was largely noncomparative, in this one, Mann explicitly compares social and state development in France, Britain, Habsburg Austria, Prussia-Germany, and the United States. And unlike the last one, which was limited by the nature of the available source material, this one draws relentlessly on both statistical and qualitative sources. Not the least of Mann's achievements is his ability to gather and analyze comparable statistical material for much of this period for all five

Mann's basic preoccupations are the same as they were in volume 1; but apart from occasional crossreferences, The Rise of Classes and Nation-States can be read on its own. However, the book will be difficult to read without a basic grounding in both the classical Western social theorists and their more recent successors: Moore and Skocpol, Dahl and Tilly, Giddens and Poggi, Rueschmeyer and Stephens-even, at times, Foucault. The book is also made more difficult (its formidable length and complexity aside) because of Mann's maddening proclivity for adding new terms to the jargon of comparative macro history, and using

others in apparently personal ways.

Nearly every chapter of The Rise of Classes and Nation-States is larded with typologies and outlines. But its historical depth and empirical richness reward the effort it takes to comprehend them. Mann has a consummate ability to blend historical monographs and works of theorized history and historicized theory with his own insights and experience. He has apparently read almost everything written in English and French (though not in German) on these big subjects written over the past 30 years, synthesizing their authors' views into his own formulations. He does gloss over or miss some important historical debates (e.g., the emergence of markets in revolutionary America); but although this will leave some historians cross, it may be a good thing in a work of historical synthesis. This is a book to sweat over and argue with; it is not a book to ignore.

Not only that, but The Rise of Classes and Nation-States is refreshingly lacking in the obfuscating simplifications that are characteristic of the social theories of our decade. In fact, its tone and ambition are resonant of the 1970s, when Mann began writing it, though its problematic is more enduring. What are the sources of social power, and how do they relate to one another? Are one or more of them primary in structuring society? How did the development of these societies relate to one another during the periods in which their major classes and states arose? How did their different paths of structural development affect how they related to one another (e.g., in explaining the outbreak of World War I and the collapse of the long nineteenth century)?

The insights and observations produced by Mann's fecund mind as it ranges over a century-and-a-half for five major countries are legion, but several stand out. His readers will be prepared for his familiar fourfold typology of power (ideological, economic, military, political), one type being permanently primary. For Mann, the forms of power are not like bowling balls, but are entwined (a favorite term). At times, one form reaches out beyond the others (military power in the late eighteenth century) with a powerful effect on states and classes. But the forms of power are not "pure," not ranged against one another in a permanent game of primacy for the benefit of social theorists. The characteristic structural developments of the century emerged from their entwinings. Mann's abiding theoretical instinct is the systematic attack on all forms of determinism.

Nevertheless, states and their crystallizations turn out to have a shaping force on classes and nations that the other forms of power normally lack. This is in part because Mann, like Tilly, is impressed with the power that war has had to transform states and thus to lead them to transform their societies. States made war, and war remade states, deeply affecting the classes and nations they represented. War is also the major way in which these states related to one another, a theme Mann chooses not to develop fully until his final chapter on World War I.

Why this predominance of political power over other forms of power? In his crucial central chapters on the rise of the modern state, Mann declares himself an "institutional statist" (chap. 3). Only by specifying the institutional-political differences between states, he argues, can we adequately explain the dramatic differences in national development—and in class conflict that he finds in his period. Thus, economic power, reinforced by ideological power, produce historical impulses; but they need to be translated by political and state power to understand the shape of the final outcomes.

The effects of Mann's institutional statism are seen most dramatically when he turns to the effects of industrialization on the West. "Capitalism and industrialism have both been overrated," he declares roundly in his conclusion. Their diffused powers exceeded their authoritative powers; and although they enormously increased collective capacities, they had remarkably little effect on distributive powers, which were largely the same at the end of the period as at the beginning. The varying outcomes of capitalism and industrialism from country to country were determined not by nations' stocks of resources, comparative economic advantages, or greater or lesser entrepreneurial endowments but by "authoritative political crystallizations that had been mostly institutionalized rather earlier," for example, by states (p. 726).

Students of comparative political economy may find Mann's conclusions about the industrial similarity of countries ranging from Habsburg Austria to the United States somewhat hasty. His reasoning about the causes of state differences will also leave many readers unsatisfied. One in particular will raise many an eyebrow, namely, that territorial particularities are enhanced by agrarian economies but diminished by industrial ones (pp. 726–27). But for those interested primarily in social theory, the most interesting problem is how the statism in Mann's historical chapters meshes with the antideterminism in his theoretical instincts. For although he misses few opportunities for determinist-bashing, the final instances in his historical narratives often turn out to be the state and the effects of war and of war's side-products upon it.

But these are issues to argue about, not to dismiss. Although the book's length and complexity, personalized jargon, and innumerable typologies will not make it an easy classroom tool, especially for undergraduates, Mann packs so much historical and comparative synthesis into his book and is so widely knowledgeable about social and state development in Europe and America that no student of comparative or historical sociology or state building will want to ignore it. At least, I hope to be around when Michael Mann gets around to giving us his final two volumes.

Cornell University

SIDNEY TARROW

Politics in the Portuguese Empire: The State, Industry, and Cotton, 1926–1974. By M. Anne Pitcher. New York: Oxford University Press, 1993. 322p. \$62.00.

Substantive treatments of Portugal's empire since its demise in 1975 are rare occurrences. While there is an extensive literature on Portuguese Africa during the 1960s and the first half of the 1970s, at the high point of the conflict between guerrilla movements pushing for independence and a recalcitrant authoritarian government in Lisbon refusing to cede any territory, studies of Portuguese colonialism since have been infrequent or, when published, partial and incomplete. Given this context, Anne Pitcher has advanced our knowledge of the dynamics of the Portuguese empire and its interrelationship with Salazar's and Caetano's New State regime significantly.

In taking the case of the Portuguese cotton industry and cotton production in the colonies, Pitcher has selected an arena that provides excellent insight into the dynamics between the state and groups and films in society engaged in economic activities. As a consequence, she clarifies a number of crucial dimensions relevant to understanding not only how Portuguese colonialism operated but also the policy instruments of the New State and its impact on politics, society, and the economy. First of all, she establishes the differences in these interrelationships according to whether one is looking at an authoritarian regime in the making (1926-1936), at a consolidated authoritarian regime (1936-1946), at an authoritarian regime undergoing significant transformation (1946-1958), or at an authoritarian regime in decline (1958-1974). Second, she shows how the regime was shaped by the need to build a broad coalition pulling together a disparate collection of antidemocratic forces in order to consolidate its power, how it was corporate in nature really only during the fascist era in Continental European politics, and how corporatism once again became incidental to regime dynamics as political leaders tried to transform an economic and political order in decline. In contrast to those who look at Caetano's rule as an era of reform and change, she characterizes the *caetanato* as one full of irreconcilable tensions and contradictions that ultimately led to the collapse of Portuguese colonialism and authoritarian rule. Third, the interplay between broad-scale regime dynamics and the operations of the cotton industry and trade introduces a degree of specificity into our understanding of the political economy of authoritarian Portugal that has been absent hitherto in Portuguese studies.

But this book is more than just an isolated case study of Portuguese authoritarianism or colonialism. It contributes to our understanding of how a strong state operating under authoritarian precepts shaped an economy and society according to the policy preferences of its leader. It examines the policy legacies of that state in the economic stagnation later of firms seeking to modernize and the costs entailed to the nation in generating a divided industry and the unresolved dilemmas which subsequently would have to be faced by a democratic regime seeking to integrate its citizens into a wider concept of European community.

The book is carefully researched and well-documented. Its particular strength is its clarification of how, despite the monolithic pretensions of the New State, authoritarian rule in Portugal was comprised of a complex set of competing forces and interests and how the state Salazar created sought to mediate and control that conflict by retaining the upper hand. Through engaging in precise analysis of cotton interests. Pitcher brings into focus the dynamics of authoritarian Portugal. With these lenses, she demonstrates why this regime was far from static and how it was continually undergoing change and evolution through Salazar's determination to retain control over the mainland and the regime's overseas extensions.

My only criticism concerns what she characterizes as the final stage of Portuguese authoritarianism (1958-1974). Although Pitcher captures regime formation and consolidation exceedingly well, I am not convinced by her argument that it is correct to characterize this regime as one in decline for its last 16 years. What is certain however, from the data and material she amasses, is that the cotton industry and trade were in decline during these years. It is here that this case study reaches its limitations, in attempting to generalize about a whole regime and era from a single set of actors and circumstances. For, without a broader perspective on the state and the economy during the 1960s and early 1970s, what is missing is a balanced assessment of the attempt to transform state and society by reformers and the crucial role played by the country's leaders once Salazar was incapacitated. Since Spain underwent significant transformation from within during the 1960s and early 1970s, what needs clarification is why Portugal failed to accomplish similar change. My own view is that the explanation is to be found more in individual leaders, in who emerged as the successors to Salazar, and their policy choices, rather than in systemic arguments as to the inevitability of the Portuguese collapse as a consequence of structural conditions and factors.

University of Texas, Austin

LAWRENCE S. GRAHAM

Vietnam: The Politics of Bureaucratic Socialism. By Gareth Porter. Ithaca: Cornell University Press, 1993. 227p. \$29.95.

This is essentially a historically oriented study of Hanoi's post-Vietnam War institutions and its state and Party socioeconomic policies, with a brief concluding chapter on foreign policy. In the later years of the Vietnam War, the author, a long-time Vietnam specialist and academic activist, was codirector of the Indochina Resource Center in Washington, which at the time amounted to the country's chief antiwar lobby. The book is divided into seven chapters. There is an introduction by George Kahin, Cornell's guru of Southeast Asian studies, under whom Porter studied as a graduate student.

Today is not the most propitious moment to be publishing full-length studies of contemporary Vietnam, so kaleidoscopic is the scene since the momentous decisions of the Vietnamese Communist party's Seventh Congress in mid–1991. After a decade-and-a-half of stagnation, Vietnam suddenly finds itself thrown into future shock. What were mere clouds on the horizon at the time this manuscript was closed have now become torrential downpours.

The first of this mandated change is the massive Party effort to restore its lustrous image of the wartime years. Indeed, in those days, the Party (i.e., chiefly its cadre structure, perhaps 10% of the total membership) was an impressive political institution, holding a reputation among rank-and-file North Vietnamese as being almost superhuman. Cadres had satisfactory answers for every question; they were more competent than anyone else on the scene; they were incorruptible; they were an ideal role model. But battlefield attrition and then time itself began to take their toll. The war ended, and cadre morale began to sag; performance declined; corruption crept in, along with rising "mandarinism" (i.e., selfish, imperious misuse of office).

Leadership rectification efforts (endless self-criticism sessions and semipurges) were too cautiously applied out of deference to past loyalties and service. Recently, however, came a more ruthless weeding out of the incompetent, not vindictively but simply to get them out of the system and into harmless sinecures. At the same time, an intensive nationwide recruitment drive has been launched to earmark the young for future top leadership assignments. One Hanoi official described this as the Party "scouring the boondocks for the best and brightest young." The concepts behind Party renovation are (1) that the Party is not as discredited as some of the communist parties in East Europe; (2) that the Party is, and must be, committed (better, resigned) to eventual political pluralism in Vietnam (in the form of the now much detested "multiparty system"); and (3) that if it renovates itself well and coopts the most promising of the country's future leaders, it can as a political party take on other political challengers (by fragmentation and playing one faction off another) so as to win political control at the polls, openly and honestly. This is a bold but risky course, but the Party just might be able to pull it off in, say, about five years.

The second "dated" quality about this work has to do with Vietnam's economic sector. This is not a thesis book as such. Rather, it is descriptive of the events that have buffeted "socialism Vietnam-style." Unlike many writers, the author does not sharply separate Marx from

Lenin—that is, Marxian economic theory, historical interpretation, and philosophy from Leninist instruments of governance to organize, mobilize, and motivate a society. This is just as well; for the question of just how Marxist Ho Chi Minh and his fellow rulers were (clearly, they were true Leninists) grows more uncertain with increased transparency of the history of Hanoi's internal political dynamics in those wartime years. It is to be noted that Vietnamese media and spokesmen today employ more variety of "official" terms for the economic system being put in place (market mechanism, market orientation, logic of the situation, etc.), but never socialism. There is great semantic ambiguity here, and it is deliberate.

Vietnam's contemporary economy is hardly addressed by the author. There is a brief and generally satisfactory account of the early years leading up to 1991, treating the problem of poverty, systemic inability to save and invest, and a decade of extraordinarily poor Party-state economic guidance. Yet even here the author fails to note the basic debate between industrialoriented export strategy and closed-door protectionist strategy. Unfortunately, the writing cuts off the moment when Vietnam actually began moving away from a command economy and towards a market economy. The heart of politics of today's bureaucratic socialism in Vietnam is found here, between the Politburo-level factions who genuinely mean to allow the logic of the situation (as it is delicately put) to fix economic policy and those who wish only to tinker with the machinery of socialism, seeing no need to abandon it outright.

The third major development in Vietnam, too late for inclusion in this work, is the fascinating political science experiment now going on within the SRV National Assembly, converting it from a motivating and mobilizational tool of the Fatherland Front mass organization into a law-making, law-implementing body.

From the beginning, in the Democratic Republic of Vietnam, governance was by the Party. Policy instruction went downward from the center to the lower-level Party jurisdictions and to state enterprises. This instruction (Politburo orders, Central Committee directives, Party Congress resolutions, etc.) was written in ideological language. It worked fairly well during wartime, but with the complexities of economic decision making in the postwar years, it broke down. At the implementing level, orders were subjected to great and honest variations in interpretation. As with the Holy Bible, they could easily mean whatever one wanted them to mean. Advisors (not least those from the international funding agencies) counseled the Vietnamese leadership that one cannot direct a semimodern society in this manner. The Seventh Congress decided to hold a political science experiment. Government by Party fiat would be replaced by government by law. Policy would be transmitted by the Party leadership to the National Assembly, still in ideological language; and the Assembly would translate the instructions into laws to be sent down the line for implementation. This is the process now underway. Most significant of the 30-some pieces of legislation approved to date is the Law of the Land, which, while fuzzy in defining ownership (yours "for a period of time" to do with as you wish) drives a stake through the heart of the most cherished concept of Marx-collective ownership of the means of production.

Finally, while not wishing to make this an ad hominem review, I feel obliged to set forth briefly the author's

record. Gareth Porter's reputation in the field has long been as a point man for the antiestablishment. He was the scourge of academics who supported the Vietnam War. Typical of his effort is "How Scholars Lie" (Worldview, December 1973). With George Hildebrandt, he defended the Pol Pot regime in Cambodia Cambodia: Starvation and Revolution, 1976). He argued at a congressional hearing that there was no bloodbath going on there (long after French intellectuals had done a mea culpa on the Khmer Rouge). At academic conferences he was often vindictive, occasionally downright mean (throwing the charge "war criminal" at those present who disagreed with him). Such was the passion of the times, and it was, if not excusable, at least understandable.

However—and this is the point—one finds none of this side of Gary Porter in the work here, not even vestiges. On balance, I would pronounce this an estimable book and commend it to all interested in examining Vietnam in depth. Porter has put to use his years of work on the subject and, in doing so, appears to have outgrown his earlier ideological orientation. We are all the better for it.

University of California, Berkeley

DOUGLAS PIKE

The Politics of Economic Stagnation in the Soviet Union: The Role of Local Party Organs in Economic Management. By Peter Rutland. New York: Cambridge University Press, 1992. 297p. \$64.95.

Peter Rutland asks how the Soviet political system worked between 1965 and 1990 and "why it was so resistant to reform" (p. 3). He approaches this through a detailed study of Soviet administrative behavior, focusing on the role of regional, local, and enterprise (PPO) party organizations in economic management. In updating earlier studies of party-management relations, Rutland makes some revisions but largely confirms the findings of Berliner, Azrael, and Hough.

The author introduces his study with a defense of the totalitarian model, then extends Friedrich and Brzezinski's "totalitarian syndrome" to describe the "post-totalitarian regime." Rutland emphasizes its ideological foundationsuniversal agreement by those who were politically active in the official system to preserve (1) elite rule, (2) the separation of public and private spheres, (3) monopolistic control over information flows, (4) the command economy, and (5) the Soviet Union's status as a superpower. (In addition, Rutland writes, they shared a view of politics in which the major differences within the elite were defined not by ethnic or regional interests but by generations.) In the conclusion, Rutland argues that a 'crucial intermediate causal factor" in the collapse of the Soviet Union was the disintegration of this value consensus.

In the empirical chapters, Rutland examines Communist party intervention in the management of both industry and agriculture. He devotes special attention to the Party's role in regional economic coordination through its "prefects" (obkom secretaries), its role as "fireman" mobilizing resources for high-priority projects, and its control of personnel through its cadres policy. The evidence is drawn from a careful reading of Soviet-era publications, with particular emphasis on Ekonomicheskaia gazeta. He uses his empirical cases to

substantiate the claim that Western analysts (using the "pyramid metaphor," p. 19) had overestimated the degree of central control over the lower reaches of administration; that Party and economic bureaucrats were guided by discrete political and economic logics; and that under Brezhnev, "party interventions in the economy were increasingly clumsy and ineffective" (p. 23). Rutland brings this empirical analysis up to the end of the Soviet system with a very brief discussion of administrative behavior after 1985.

In focusing his empirical analysis on the lower reaches of the Soviet bureaucratic hierarchy, Rutland may make it impossible to answer the most important questions he poses in his study. The central Party leadership could make heads roll, redirect investment funds on a massive scale, and thoroughly redesign bureaucratic structures. Instead, as Rutland's case studies show, the Brezhnev leadership usually chose to leave personnel in place, change investment only incrementally, and let the system grind away in its established routines. Why did the top leadership not choose differently? This question gets at the ultimate sources of economic stagnation and failure, but this study begs the question.

Because Rutland excludes the upper reaches of the Soviet system from his analysis, he omits the context that might give very different meaning to the administrative behaviors he observes. For example, the central leadership may have deliberately fostered the tension that Rutland notes between local Party and managerial officials. Rather than evidence of its ineffectiveness, this tension may have been a mechanism of central Party control. Again, the ministerial hierarchies may themselves have been mechanisms of central Party control. Thus, the victory of ministerial hierarchies over local planning and coordinating may have represented the victory of center over periphery and a victory of the Party. Again, the Soviet press's spotlight on deviations from the expectations of the central Party leadership (Rutland's primary evidence) may be not a sign of failings of Party control but actually a means of control. This study does not give us a picture of the routine workings of the economy and the enormous power of "anticipated reactions" to contain the behavior of the economic managers. In short, without the big picture, it is difficult to impute meaning to the smaller details in the

Following on his detailed study of administrative behavior, Rutland's conclusions are somewhat baffling. First, he concludes that "the economic stagnation which crept over the USSR during the Brezhnev era was in no small part due to the close monitoring of economic life exercised by the CPSU" (p. 218). Yet the author seems to make the diametrically opposite case throughout the preceding chapters—that in the Brezhnev era, stagnation resulted from the growing inability (or unwillingness) of the CPSU to move economic administrators from their established routines. Second, to find the primary cause for the collapse of the Soviet Union, Rutland reaches completely outside the bounds of his study. The crucial factor, in his estimation, was "the multinational character of the state" (p. 220). (Once again, the research design seems inappropriate to the questions with which the study begins.) Third, his 'intermediate" cause, which is closer to the subject of the monograph, seems to shift his focus from the micro behaviors on the shop floor to the macro phenomena of the Soviet political system—the breakdown of shared values in the political elite. In short, while Rutland has written a fine study of administrative behavior *under* the *ancien regime*, it is not entirely clear that he has explained how the regime worked, why it stagnated, or why it collapsed.

Despite the apparent mismatch between its larger conceptual framework and its empirical analysis, Sovietologists and post-Sovietologists should find much of value in this monograph. Rutland has probably written one of the final Sovietological studies of party-management relations, synthesizing the major findings from the existing literature. Through close reading of the Soviet press, he advances this analysis up to 1985. Rutland has established a solid base on which future historians and social scientists can build with new information culled from interviews and archives.

University of California, San Diego PHILIP G. ROEDER

The Rise of Meso Government in Europe. Edited by L.J. Sharpe. Newbury Park: Sage, 1993. 327p. \$65.00.

This book, originally conceived at a European Consortium of Political Research conference held in 1986, is the product of a series of meetings held in a number of desirable European locations. It is concerned with the phenomenon of the meso (taken from the Greek word mesos, meaning middle). The central thesis is that the unitary state in western Europe has experienced developments over the last 20 years or so which have fundamentally changed its character. Following a general survey by the editor which both helps to link the individual contributions as well as providing a useful overview of the literature, the volume comprises eight country studies Belgium, France, Italy, the Netherlands Norway, Poland, Spain, the United Kingdomtogether with a brief note on Denmark and Sweden. In addition, there is a chapter on the place of regions within the European Union. The style varies. Some of the contributions—such as that on Poland—offer an account of governmental structures with little analysis of the concept of the meso. Others, such as Toonen on the Netherlands and Keating on regions and the EU, help the reader to reflect on the general concept and its implications for the political processes.

What then is the meso? Trying to provide an answer to this question on the basis of the papers in the book is reminiscent of the "blind man and the elephant" phenomenon in the sense that definition depends on context. The editor states on the first page of the introduction that the meso should not be equated with the region. Rather, he argues, it is not so much a level of government as a "decision-space" within the governmental structures where certain kinds of decision are taken. Nevertheless, even he lapses into the usage "level of government." The contributors appear to use the term to indicate various phenomena, most often the region or some structure approximating to a level of authority between central and local government. When this is combined with the predictable variations characteristic of national experiences together with uncertainty as to whether the meso exists at all in some cases (Belgium, discussed in Delmartino's chapter, for example) some inevitable uncertainty enters the reader's mind.

The causes of the rise of the meso are set out in a clear fashion in the editor's survey and encompass the kinds

of development which one might expect. Thus regional ethnic nationalism, functional adaptation to changing responsibilities, particularly those associated with the growth of the welfare state, and regional planning accompany ideology, political interests and "me-too-ism" among the explanatory factors. The situation, of course, varies from country to country. In the case of France, for example, Mezey argues that the key factor has been the response to the needs of modernization.

A perspective which is relatively underplayed is that of domestic responses to the forces of globalisation. Keating's contribution on regions in the context of the EU is helpful here, as is that of Toonen on the Netherlands. Keating argues that we are witnessing the emergence of a new political dynamic focusing on a triangular relationship between Europe, the nation state and the region which gives rise to complex patterns of interaction. Toonen extends this outside the European context by pointing to the links that bind regions and cities (such as Rotterdam) to the wider international environment.

This takes us back to definitions: the *middle* of what? The problem is that the term *meso*, despite Sharpe's initial disclaimer, implies a position within a hierarchy of political activity from the subnational to national to international. And yet much of what is described here is part of a broader and even more complex redefinition of political processes wherein actors are frequently constrained to operate outside their allotted arenas. In this context, it is not at all surprising that one of the recurring themes in the book is the relationship between noncentral authorities and the EU. Clearly in the Spanish situation as described by Cuchillo, access to Brussels has been a bone of contention between the regions and Madrid and has yet to be resolved.

But as Keating suggests, the position of regions on this issue is complicated by two sometimes conflicting needs: a degree of local autonomy to pursue interests on the one hand and access to centers of decision-making which affect those interests on the other. Increasingly, those centers lie outside the domestic arena. The consequence is ever more complex patterns of linkage, not simply between center and region/locality but between a variety of actors, governmental and non-governmental, both within and without national boundaries. In other words, Sharpe's "decision space" (p. 1) is becoming internationalised. Internal interdependencies between levels of government, a well-recognized result of adaptation of the polity to the needs of policy management, are being joined to international interdependencies reinforced by the globalization of capital but also by a range of transnational linkages which focus on the region as much as, or more, than its national setting. Consequently, the meso is continually shifting-or, as Toonen describes in the Dutch case, there are several mesos, sometimes in conflict with each other.

As both Sharpe and some of his contributors make clear, the pressures with which this book is concerned do not presage the demise of the nation-state. Rather, we are witnessing an evolutionary process whereby policy functions are discharged at a variety of levels. National governments may come to perform fewer functions in some contexts, more in others. Whether or not we are witnessing the construction of a "Europe of the regions" (if this implies the disappearance of the nation-state it seems unlikely), the policy arena will become more complex and the *meso* will have a place within it.

But increasingly the question will be: what is it the *middle* of?

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BRIAN HOCKING

The Rising Tide of Cultural Pluralism: The Nation-State at Bay? Edited by Crawford Young. Madison: University of Wisconsin Press, 1993. 305p. \$49.50 cloth, \$17.95 paper.

Until recently, political scientists have been loath to treat the dynamics of ethnic politics seriously. They assumed that modernization required a strong, secular state and that ethnic attachments represented annoying impediments in the way of cultural assimilation. The more rapidly these hindrances to progress could be swept aside, they concluded, the sooner state elites would be in a position to move their societies toward nation-building and modernization. This was a case of misunderstanding and misperception writ large. Because the ethnic community, whether real or mythical, has played an important role as a demand-bearing group of a psychological nurturer, it has survived and developed, for some purposes at least, as an alternative to the state itself. It is therefore time for political scientists to revise their frames of analysis and to take account of the complex and fluid interplay of state-societal and interethnic relations now unfolding rapidly.

How are we to interpret and understand the radically changing process of identity politics in post-Cold War times? Crawford Young's newly edited book, The Rising Tide of Cultural Pluralism, provides an initial boost to analyzing the multi-faceted forces at work. In his introductory essay, Young develops a broad theoretical framework for studying cultural pluralism, setting forth the three formative conceptions of instrumentalism, primordialism and constructivism. Whereas instrumentalists emphasize the role of communal broker in mobilizing his or her ethnic constituents to extract material resources from the state, the primordialists perceive group identities largely in terms of their powerful emotional and psychological holds on their memberships. The constructivist label is applied to those scholars who view the ethnic group as a creation of political and social elites and who take advantage of a momentary historical opportunity to establish a new (albeit artificial) grouping. Young's modes of conceptualization are not new. Yet he does advance the current dialogue by stressing the overlaps and points of convergence between the instrumentalist and primordialist views. "Primordialism," he writes, "usefully completes instrumentalism by explaining the force of the 'affective tie' through which interest is instrumentally pursued" (p. 23). Primordialism and instrumentalism, then, are dimensions of a larger ethnic phenomenon. If social scientists accept this

synthesis, we will be well on the way to going beyond definitions to the analysis of process and policy.

A number (but by no means all) of the contributors to this volume apply Young's conceptualization in the chapters that follow. Growing out of a summer seminar for college teachers, the result is an uneven coverage of conflict themes and situations—with the former Soviet Union, the United States, and Ethiopia probed most extensively. In an interesting essay on language policy in the United States, Ronald J. Schmidt interprets language politics within an instrumentalist framework, contending that "competitive ethnic relations will devolve into a competition over who will bear the greatest 'costs' and which group will garner the most 'benefits' in the mutual adjustment to multi-lingualism in the political territory which the groups share" (p. 82). Douglas Spitz, focusing upon cultural pluralism in contemporary India, writes convincingly about the Rashtriya Swayamsevak Sangh's use of religious and cultural symbols to promote feelings of communal kinship and resistance to multi-nationalism, separatism, and secularism. Finally a number of authors-Nader Entessar ("Azeri identity in the Azerbaijan Republic and Iran"), Tekle M. Woldemikael ("Eritrean social pluralism"), James Quirin ("The Beta Israel of Ethiopia"), and Alan LeBaron ("Maya ethnonationalism in Guatemala")-adopt the constructivist perspective and describe their respective identity groups as "imagined communities."

The commonalities and differences among these perspectives are not carefully probed, leaving the task of comparative explication largely to the reader. What measures are we to employ to determine whether such identity groups as Bosnian Muslims or Serbs, Tamils, or Hutu are formed to further instrumentalist, primordial, or constructivist purposes? When does the imagined community cross the threshold to a genuine sociopolitical community? If peoples mobilize along communal lines and are prepared to sacrifice their lives to advance identity group objectives, does the constructivist framework adopted by many of these contributors help in interpreting the dynamics of competition and conflict?

With their large brush strokes, Young and his colleagues point us toward the need for further comparative analysis. Once this is done, we will be freed up to concentrate on other problems—most notably, systematic attention to the interactional process at work, the relationship of economic and political liberalization to cultural pluralism, the impact of the international variable on many tense ethnic conflict situations, and the effective management of identity group conflicts by state elites. Young is right that "[t]he postwar flow of political history has . . . radically transformed the politics of cultural pluralism" (p. 21). This leaves us with much to do.

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DONALD ROTHCHILD

INTERNATIONAL RELATIONS

The Morning After: Sexual Politics at the End of the Cold War. By Cynthia Enloe. Berkeley: University of California Press, 1993. 326p. \$38.00 cloth, \$15.00 paper.

Cynthia Enloe, one of feminism's liveliest minds, is at it again. In Bananas, Beaches, and Bases: Making Feminist Sense of International Relations, she dared to discuss the Chiquita Bananas of international political economy, the Pocahontases of diplomatic practice, the female companions of realist warriors on military bases, and the chambermaids western tourists failed to notice when going international for an "exotic" vacation. All of these people, she contended, were in international relations and should be accounted for by the field of IR. Indeed, it took considerable power to create and then exile these people and activities as irrelevancies next to the global management of interstate relations. In The Morning After, Enloe extends the themes of her earlier work to the supposedly new era we have entered at the end of the Cold War. Her overarching point is that various forms of masculinity and femininity were required to create the Cold War, and that various sorts of transformations in the relationships between women and men are needed to ensure that the ending processes, which are multiple and ongoing, proceed.

Much of her discussion revolves around the concept of militarization, a word she defines minimally as any part of a society that becomes controlled by or dependent on the military or military values. There is much around us that can be militarized, such as toys, marriage, scientific research, university curricula, motherhood, fatherhood, AIDS, immigration, or comic strips. The Cold War, she argues, depended on multilocated and thoroughly permeating militarizations of identity and security. Men had to be instilled with a type of masculine identity that made it possible to commit thousands of mostly male bodies to places like Korea and Vietnam in the name of securing a distant national interest. Women had to play their parts as domesticallybased yet vulnerable patriots, armed with a sense of what real, what proper, what brave men did for their countries (armed later with yellow ribbons of support for troops abroad). When the "little" wars of the Cold War finally wound down, thousands of men had to be deprogrammed so that masculinity could take on peacetime dimensions. Thus, from the ashes of war fathers were born; of course, the burdens of militarization merely shifted to men holding the line for us in foreign armies, men trained in our ways of Cold Warring. Some women had to relinquish elements of independence and autonomy gained while their men were away: they mothered, soothed, or turned from pristine patriots into Fatal Attractions. Others were encouraged to service the outposts of freedom by offering love for money.

Enloe argues that the process of militarizing masculinity and femininity, however, was never easy and straightforward. Just as it took power to create Chiquita Banana as a marketing logo for the Banana Republics that political economy had also created, it took extraordinary power to create and sustain militarized gender during the Cold War. Says Enloe, "if masculinity in the raw" were sufficient, there would be little need for the

sweat, blisters, and humiliations of basic training" (p. 55). If femininity were a cup of sweet natural nectar to be preserved from alien ideologies, why was militarization in Central America and the Caribbean accompanied by efforts to bring women into the low-paid, underprotected, decidedly unsweet sweatshops of foreign companies?

Enloe's gaze takes us on the type of global journey that rarely figures into mainstream IR. We consider Nicaraguan voters electing a woman to office, Soviet women engaged in anti-war protests, Filipina prostitutes, Canadian dissenters from NAFTA, American servicewomen raped by their fellow soldiers in the Persian Gulf War, Kuwaiti feminists, NATO politics, and UN peacekeepers. In each case, what makes the story noteworthy is that it illustrates some aspect of the militarizing reach of the Cold War or some fragment of a post-Cold War world that continues to be defined by Cold War values (when will Americans turn out for a parade to mark the return of UN peacekeepers? Why do some of the strongest manifestations of militarized Zulu identity coincide with the post-Cold War leap toward postapartheid South Africa?).

This book, sturdily buttressed by Enloe's previous work on feminist international relations, is exemplary in the refusal to refuse the lives of everyday people in order to do IR. It is exemplary in concerning itself with everydayness everywhere and not just with "the public" in the great powers we normally study in this field. It lays down important ground when it suggests that IR, the home of power politics, actually underestimates power by failing to consider how much power it takes to militarize gender and how much of our world politics depend on this militarization. The point: relations of sex and gender are not incidental to world politics. They are not topics some of us can simply choose to research because we are feminists. Rather, these relations illuminate IR's core concerns with war, peace, and security and should become part and parcel of the field.

There are ways, however, in which this book is somewhat marred. Enloe does not make a fully persuasive case that many of the militarization strategies she discusses are especially indicative of Cold War fighting as opposed to other militarized conflicts, other wars. She does not problematize or periodize the Cold War, nor question the gender identities of the men and women she interviews and writes about. "The Cold War" might be a feature of all patriarchal peaces rather than the mark of a period we have been trained to recognize in a certain way. "Men and women" may not see themselves as men and women acting thus and so during "the Cold War." Sometimes, they may see themselves enmeshed in a plethora of relationships (e.g., as soldiers, mothers, workers, party members) that are not easily summarized as militarized gender relations, in which case, whose morning after is Enloe studying? When I used the book in an advanced undergraduate course on women, power, and politics, moreover, I also found that the students were not certain they understood what militarization was and whether it was something different than patriarchy. A little more time with definitional issues might have strengthened an already powerful set of arguments.

Finally, there is the issue of theory. Enloe has been accused in the past of being atheoretical, in that she does not present the reader with a discipline-recognizable framework for her arguments, nor endeavor to refer her findings back to IR theory in a way that could compel the field to review its inadequacies. She prefers action stories to theory. She prefers writing for the lay person and the feminist and the bloke with common sense to writing for the imprimatured experts of IR. Arguably, her approach simultaneously challenges IR's preoccupations and scuttles away from the inevitable confrontation it invites with the field.

Yet to quibble with her way of showing how everyday people and the dailiness of their lives comprise aspects of international relations is to privilege the usual way we in the field understand theory. To ask for more definitions and tracings of how the many examples she offers illustrate militarization is to take from the reader the distinct pleasure and challenge of reading IR differently, more evocatively. To ask for a querying of men and women as people we may not be able to identify and label easily is to risk making postmodernist sour grapes in the face of a champagne achievement of standpoint feminist IR. This book effervesces. It will tingle stodgy IR palates and be consumed in one bubbly gulp by the many merry-makers about whose tastes run to spicy stews.

Australian National University Christine Sylvester

Double-Edged Diplomacy: International Bargaining and Domestic Politics. Edited by Peter B. Evans, Harold K. Jacobson, and Robert D. Putnam. Berkeley: University of California Press, 1993. 490p. \$50.00 cloth, \$15.00 paper.

This book is an important landmark in what has become a virtual flood tide of recent works deemphasizing the international system as an explanatory focus. In contrast to others that situate themselves theoretically smack in the middle of the domestic arena, this treatment is located precisely at the boundary of the international system and domestic politics. As such, it recognizes both systemic roots of foreign policy decision making as well as domestic influences on the calculus of such decision making. However, the nomenclature is not the familiar one of "balance" or "disequilibrium," but a new one introduced largely by Robert Putnam's well-known article entitled "Diplomacy and Domestic Politics" (International Organization, 1988). Indeed, the entire book emerged from several conferences held at the Center for Advanced Study in the Behavioral Sciences, dedicated to exploring Putnam's formulation in a series of case studies using the method of focused comparison. Concepts such as "win-set"-the set of international agreements that would gain domestic approval-or "reverberation" wherein international pressures "reverberate" within domestic politics, constitute the new lexicon.

The book is divided into three principal sections, viz. security issues, economic disputes, and North-South tensions. As one might expect, this formulation works least well in matters concerning national security, where international threats can temporarily mute the contentiousness of domestic interests. Jack Snyder clearly shows how domestic changes that initially appear to be

sui generis actually are consequences of international changes that yield learning by decision makers of the objective constraints and incentives of the system. Understanding bargaining behavior of East and West over Germany entails not only some of the diplomacy occurring at the boundary between system and state suggested by Putnam, but also as Snyder suggests, a knowledge of the separate processes in both system and domestic society that occur independently and then can have separate impacts on the negotiation process.

At times, the system is slighted in favor of domestic processes as in Janice Stein's treatment of the Camp David accords. Although the Egyptian economy and Israeli domestic politics clearly affected the Camp David process, nevertheless, it was the 1973 Yom Kippur War and especially Henry Kissinger's encouragement of a military disengagement, that introduced elements of a power balance missing from the Middle East since the 1967 war. This new balance facilitated the making of the later accords. Although mentioned in her analysis, this process is treated as a "constant" instead of the variable initially manipulated by Kissinger to lay the groundwork for a later peace, and of course, increase United States influence in that region.

In his introduction to the volume, Andrew Moravcsik also tends to avoid systemic explanations in favor of domestic ones. Brezhnev is accused of inconsistency in his policies of accommodation in Europe and belligerency in the Third World; such inconsistency is explained with reference to the need to placate domestic hardliners. However one need not adduce ad hoc explanations. In my own work, for example, the system itself after the Hungarian, Berlin, and Czechoslovakian crises gave evidence of the extreme danger of further confrontations in Europe, while displacement of superpower conflict to the Third World actually helped foster system stability. The demands of parsimony and simple structure of explanation still must be met, even as we open new theoretical venues.

Two of the most interesting and informative chapters are those that deal with violated expectations as to probable outcomes. Given the realist paradigm, the more powerful target of influence should be better able to resist the blandishments of a great power. Yet, in two instances, quite the opposite occurred. The first is by John Odell, who describes the United States failure to prevent Brazil from developing its national computer industry and in doing so displacing U.S. and other foreign firms, and contrasts it with U.S. success in removing new barriers to U.S. feed grain exports to Spain and Portugal as new members of the European Community. Success in the EC case but failure in Brazil occurred despite sizable power differences between the EC and Brazil, and the far greater economic dependency of Brazil on the U.S. The answer turns on new theoretical contributions by Odell that emphasize the role of domestic interests in making threats credible or undermining their credibility, as the case may be.

The second and perhaps the most enlightening of these contributions is by Lisa Martin and Kathryn Sikkink, who make human rights issues not only morally compelling but theoretically important. Why did the rather large and important South American power, Argentina, succumb to U.S. pressures concerning human rights abuses while the much smaller and relatively insignificant Guatemala was able to resist such pressures? Again, the answer lies in the domestic arena in which the absence

of domestic human rights organizations in Guatemala and in particular, the view of the rural countryside as the preserve of the military that would countenance no peasant opposition, doomed the Carter administration's efforts to be seen as interventionist and hence to be rejected. In contrast, the presence of such organizations in Argentina ultimately facilitated the U.S. effort. There are other excellent treatments in this book, but space limitations prohibit an extended discussion.

Overall, the most theoretically significant contribution is the concluding chapter by the Berkeley sociologist, Peter Evans. He is able to generalize across the several case study comparisons and transcend their immediate theoretical and empirical domains. Evans relates the probability of agreement in negotiation to the extent to which costs are concentrated and benefits are diffuse. The extent of enfranchisement appears to bear no relationship to the probability of agreement. This latter conclusion, drawn in part from the Guatemalan instance, appears to defy conventional wisdom on the likelihood of agreement being dependent on the number of voters and voting groups in society. Here, we find one of the strong contributions of this book. The overall effort is not one that simply confirms the obvious in stressing the importance of domestic interests in order to reach international agreement, but one that yields some counterintuitive findings that can be built upon in the future.

Theoretically, where does this book fit in the overall scheme of things? Clearly, at this stage it is no more than a theoretical framework for analyzing the success and failure of international negotiations. This is not to diminish its importance, for international negotiations can be exceedingly important, but simply to suggest that it is not pitched at the level of grand theory. With the publication of this book, Putnam's initial contribution is now more than a metaphor of analysis, as he himself suggested it might be, and far more than a loose framework for the incorporation of a chronology of events. This is the potential degenerate instance of an application of Putnam's framework that this volume happily avoids.

Yet in one respect, this book constitutes a retreat from the level of sophistication of Putnam's initial formulation. In that article (included as an appendix), there are at least the beginnings of formalisms that could have been built upon. None of these are in evidence in this edited collection that rests at the level of verbal theory. Inherently, there is nothing wrong with this, but it does leave the reader less satisfied than if at least some of the contributions had dealt with the formal aspect. Also missing, as Evans indicates in his concluding chapter, is a sense of the historical. These studies are almost entirely cross-sectional in design. A larger sense of change over longer time periods would be a valuable addition to this now successful research program.

What is now to be done? Increased formalization and an incorporation of the longitudinal are clear possibilities. Equally important might be a large N quantitative analysis of the validity of several of the propositions gleaned from these case studies. What is clear is that this essentially brush-clearing operation has been successful and future researchers are now in a position to build on these efforts.

Rutgers University

Manus I. Midlarsky

The Best Defense: Policy Alternatives for U.S. Nuclear Security from the 1950s to the 1990s. By David Goldfischer. Ithaca: Cornell University Press, 1993. 283p. \$42.95.

For much of the nuclear age, Americans and Europeans have learned to live with the paradoxical notion that security rested on a balance of mutual annihilation. Most of the time, however, most people disliked this situation. It was either morally unsettling or seemed to require unwarranted trust of our adversaries, or both. These circumstances appeared to be caused by a combination of technological necessity (given the massive destructive power of nuclear weapons) and political rivalry. Neither element easily yielded to change.

The end of the Cold War removed much of the political compulsion to maintain the course of security by mutually assured destruction (MAD). David Goldfischer suggests, furthermore, that MAD cannot be adequately understood as a function of the technological imperative. Goldfischer argues that a middle ground for nuclear security, between the pursuit of war-fighting and submission to assured destruction, has existed since the beginning of the Cold War. Mutual defense emphasis (MDE) arms control was promoted, sometimes within the U.S. government, in the early 1950s, the early 1960s, and again during the Reagan years. On each occasion, the combination of mutual offensive restraints and strategic defenses lost the bureaucratic battles in favor of exploiting U.S. strategic offensive advantages. The resulting MAD world of offense-based deterrence and arms control that prohibited strategic defenses was wholly predictable from the politics of the process.

The process is well revealed in Goldfischer's chapter on early proposals to combine restraints on hydrogen bomb tests with pursuit of continental defenses. J. Robert Oppenheimer and others who pressed for this policy direction came under suspicion of disloyalty, and the proposals were tarred by the same brush. In addition, Goldfischer characterizes the strategic inclinations among the warring parties in the Defense Department (for whom much more was at stake than for the armscontrol outsiders) as an emerging "cult of the offensive" that narrowed the range of acceptable security policies.

Mutual defense emphasis must be paired with offensive arms control to be a viable nuclear security policy. Neither unrestrained defense nor mutual offensive reduction alone is prudent, according to Goldfischer. The first aggravates insecurity, insofar as unilateral pursuit of defenses may create ambiguity about the defender's intentions, especially when some defensive technologies and deployments, such as strategic defense initiative, are capable of offensive applications. Deep mutual reductions, on the other hand, create unnecessary vulnerability, because offensive deterrence becomes unstable at very low numbers of weapons.

Goldfischer does not argue for anything like the Star Wars program of the 1980s, despite his defense of defenses. In fact, he offers a very sensible set of criteria for MDE arms control that explicitly excludes space-based deployment. The first two elements in this list, for example, are "a pledge not to deploy defenses unilaterally beyond the confines of the 1972 ABM Treaty" and "a proposed ban on space-based weapons of all types, including restrictions on development enforced by means of a ban on testing in space" (p. 248). These prescriptions, along with continuing the offensive re-

ductions undertaken in the INF, CFE, and START treaties, place Goldfischer's argument squarely within the bounds of mainstream arms control. It is his case for undertaking mutual defenses in conjunction with offensive restraint that challenges the conventional wisdom.

This otherwise fine book suffers, however, from a tone and language that reveal it as a victim of its times, most obviously in its first and last chapters. Goldfischer acknowledges the end of the Cold War and the demise of the Soviet Union; but this is fundamentally a book about the U.S.—Soviet rivalry, and he seems unsure in talking about the post-Soviet set of relationships. Admittedly, 1992 (the last year in which the manuscript was likely to be changed or updated) was also the first year of the post-Soviet order, and foretelling what the future held was particularly difficult. Still, simply discussing the "nuclear superpowers" as if nothing had changed except the leadership of one side is not entirely satisfactory.

Russia and many of the former Soviet satellites seeking to join NATO or the more general Partnership for Peace and Washington and Moscow's retargeting their strategic forces to the North Pole, rather than at each other, raise several questions. Is the pursuit of mutual defense emphasis necessary in this new world? Arguably, so long as some states possess long-range nuclear weapons systems, a defense against accidental or very modest launches may be prudent. But it must be evaluated in its relevant political context and in light of alternatives. Ongoing debates about tactical ballistic missile defenses are related to these problems. Similarly, MDE may be a key to expanding the regime of offensive reductions from the United States and Russia to include Britain, France, and China. But at least three-and perhaps four-of these players do not have a problem with nuclear threats against each other, so the nature of the threat is less clear. Finally, is MDE poltically possible? In an era when defense spending is increasingly yielding to domestic priorities, the necessity of a new, extensive strategic defense effort will be a tough sell.

Harvard University

WILLIAM B. VOGELE

Ideas and Foreign Policy: Beliefs, Institutions, and Political Change. Edited by Judith Goldstein and Robert O. Keohane. Ithaca: Cornell University Press, 1993. 308p. \$37.50 cloth, \$14.95 paper.

Idealism has been sneaking up on international relations theory for a couple of decades now. Though few students of world politics today call themselves idealists, many are focusing their research on the ideational and social causes of state action, rather than the material ones favored by realists: political psychologists on belief systems and operational codes, liberals on international institutions and democratic values, critical theorists on ideology and discourse, and even "cultural realists" on nationalism and religion. Despite the crowded field, Judith Goldstein and Robert Keohane have assembled an impressive set of new essays that goes a long way toward giving ideas analytical respectability as causes of state action, even if they also miss some opportunities to clarify and push the thesis further.

Ideas and Foreign Policy opens with a strong theory chapter by Goldstein and Keohane, in which they propose a common analytical framework hammered out in

three meetings with the other authors. Positioning the project against rationalists for favoring interests over beliefs, critical theorists for failing to develop a research programme, and cognitivists for focusing on belief accuracy rather than content, they argue we need to show not only that ideas matter but how. On this basis Goldstein and Keohane define as the null hypothesis for the volume that variation in foreign policy can be explained by factors other than ideas, principally power and interest (an analogous null governs much of regime theory). They then identify three types of beliefs (worldviews, normative, and causal), and three "causal pathways" by which they may affect foreign policy: road maps to reduce uncertainty, focal points to solve coordination problems, and institutionalization. The theory section is rounded out with a superb chapter by John Hall on the materialist-idealist debate in classical sociology.

There are eight more empirical essays organized around the causal pathways. Exemplifying road maps, John Ikenberry shows how Keynesian economic ideas shaped American and British national interests in postwar trade negotiations; Nina Halpern analyzes the role of Stalinism in helping states do socialism; Robert Jackson focuses on the role of normative ideas in decolonization, as does Kathyrn Sikkink regarding human rights policies. Geoffrey Garrett and Barry Weingast integrate ideas as focal points into a rationalist model of the Single European Act, and John Ferejohn examines the effects of competing ideas of monarchy in 17th century England. Stephen Krasner makes a materialist argument that ideas did not matter in the Peace of Westphalia, and Peter Katzenstein details the effects of institutionalized norms in Japanese and German postwar internal security policy. These chapters are methodologically sophisticated, substantively interesting and, Krasner's excepted, provide compelling evidence for the independent causal force of ideas.

The volume differs from other idealisms (or "ideaisms") in two important ways. The first is its broadly rationalist cast, which separates it from the otherwise similar (yet strangely neglected) literature on belief systems. Although Goldstein and Keohane criticize rationalism for focusing only on preferences, as I understand it the core of rationalist explanation is that desire or preference plus belief or expectation causes action. Game theorists, after all, now tell us that "common knowledge" helps solve games with multiple equilibria. As such, most of the chapters do not challenge rationalism so much as rejuvenate its ideational component. A second contribution is the attention to micro-foundations. The editors do not emphasize this, but the ideas discussed are those held by individuals, not corporate actors or social systems. This enables the authors to provide detailed descriptions of the mechanisms by which ideas cause foreign policies, which are often lacking in more holistic or institutional idealisms, including regime theory.

The volume also misses two important opportunities. One stems from an ambiguity in the dependent variable. Goldstein and Keohane and several other contributors accept the point—long pressed by critical theorists—that ideas may shape interests as well as behavior; one effect operates on a property of actors themselves, the other merely on what they do. Yet, there seems to be little recognition of how different these effects really are, and of its significance for larger theoretical debates. If ideas only shape behavior, materialists can say they are at

most "relatively autonomous" of material forces that still determine interest "in the last instance," and a dualistic model of man in which preferences and expectations are rigidly separated is not threatened. If ideas sometimes shape interests, however, as Ikenberry and Jackson show, then both the impact of power and the nature of interest will depend on beliefs. Unfortunately the volume's research design is not set up to distinguish and compare the two effects. Indeed, despite the concern to show how ideas matter, the book's null hypothesis in fact permits us to infer only that they do. To answer how, a second null that ideas only affect behavior might have been set up, and then analysis directed to whether interests really are a function of ideas.

The other issue concerns the relationship between the ideas held by individuals and the institutional structures in which they are embedded, notably the state and international system, which are themselves constituted by ideas. Several chapters invoke this relationship, but none addresses the question of whether institutional structures are reducible to individuals' ideas ("common knowledge"), or have a facticity independent or constitutive of individuals (what is today sometimes called "supervenience"). The rationalist sub-text of the book suggests that most of the authors would favor the individualist, common knowledge view over the more holistic, supervenience approach. If institutions are reducible to common knowledge then we should study ideas much as these authors have, starting with individuals and aggregating to capture social and institutional effects; if not, there will be an important aspect of world politics that can be grasped only by examining collective (as opposed to shared or common—the distinction is Durkheim's) representations like discourses and ideologies. The latter are the primary focus of more explicitly critical or "structural" idealisms, as well as of Jackson's paper, to this reviewer's taste the best of the lot. The book leaves us with a significant challenge of balancing the micro-foundations of individual beliefs with a conceptualization of corporate agents and international institutions as emergent collective knowledge structures.

Ideas and Foreign Policy is full of interesting lines of argument that bear further development, and even in its problems raises important issues that will advance the idealist research programme. It is a must-read for idealists and their critics alike.

Yale University

ALEXANDER WENDT

Foreign Relations and Federal States. Edited by Brian Hocking. New York: St. Martin's, 1993. 294p. \$57.00.

This volume examines an important, though very understudied, phenomenon in the field of international relations. The lack of attention to federalism as a source of foreign relations is unfortunate because politics in a substantial number of countries worldwide involves tensions between (and among) territorially based actors and central authorities. This is evident in the breakup of the former states of the Soviet Union, Yugoslavia, and Czechoslovakia as well as in ongoing political pressures and change in Canada, India, Germany, South Africa, and so on. Less obviously, federalism is an important dimension of routine domestic politics in a large number of Western states (e.g., Germany, Australia, the United States) and the numerous ethnically divided African and

Asian states. It is, then, curious that these pervasive, fragmenting pressures on foreign relations have not received greater attention in the field of international relations, especially given its preoccupation with the territorially defined nation-state.

This thought provoking, well-organized collection of essays makes an important, though not comprehensive, step in that direction. The majority of the volume's 17 chapters are case studies examining closely a particular issue and/or country's system and foreign affairs. The countries included in the volume are the larger, major advanced industrial democracies with federal systems: Australia, Canada, Germany, and the United States. Case study chapters vary in theoretically meaningfully ways with respect to the scope and type of actors considered. Some are single (or dual) country studies, several look at federal systems in interaction (e.g., Australian states, Canadian provinces, and the Chinese central government), and others focus mainly on noncentral types of actors, e.g., subnational actors (Ontario and Quebec) or supranational bodies (the European Union). In the most basic sense, the value of these case studies lies in their providing substantive, or descriptive, insights into the kinds of international activities created by federal arrangements and their distinctiveness from the traditional foreign policy behaviors of central governments.

But this is not to say that the book is atheoretical. The book's four opening chapters take different theoretical or conceptual cuts at approaching federalism as its linkage to international affairs. At one level, they focus on basic conceptual tasks required to capture the apparent crossnational complexity of, first, the variety of constitutional arrangements linking states and provinces to the international arenas and, second, the classification of different types of "non-central foreign economic behavior" and activities disputed by "central" and "non-central" activities. At the other level, the essays consider what factors drive the increasing involvement of states and provinces in international affairs. Particularly useful in this regard is John Kincaid's original essay in which he argues that growing international economic interdependence has created a tension between "citizenship" and "consumership" involving a greater demand for the relatively local, or non-central, regulation and coordination of international ties. These kinds of analytic insights are not limited to the first four theoretical chapters. Almost all case study authors have interesting theoretical and conceptual points about the form and sources of federal pressures in their particular situation. Though often made briefly, these deserve careful attention, often best by comparing related chapters. For example, the chapters on Quebec and Ontario by Louis Balthazar and David Dyment, respectively, are useful in conceptualizing the different situations of the two provinces and showing that much of dissenting Quebec's international activity falls short of dramatic separatism while the more federally content Ontario engages in considerable autonomy in its international contacts. The essays by Michael Smith and Christopher Hill are important steps toward understanding the European community as a single international actor. Taken together, then, the volume has a strong theoretical strand running through it. If there is one complaint, it is that there is no concluding chapter to tie together the various strands running through these chapters. Given their diversity and their originality, a closing synthesis (if yet possible) of different arguments and approaches—and their implications for further research—would have been valuable. Still, the book remains coherent given the solid introduction and logical flow of chapters.

Given its broad title, it is important to recognize that this book is not comprehensive and takes a particular theoretical approach. In fact, the book might have been better titled "transnational relations and federal states." Reflecting the work of Keohane and Nye (but with interesting non-U.S. perspectives), almost all of the book's chapters view federalism within the context of increasing "complex interdependence." In doing so they raise two intertwined considerations. One is that "noncentral" political entities, such as states and provinces, need to be considered in disaggregating the state to uncover the "multiple channels" that now link national societies and propel transnational ties. These entities are different from private "transnational" actors such as multinational corporations; instead, like "transgovernmental" players, these actors operate authoritatively but presumably far more independently than bureaucracies of the central government. This leads to the other theoretical theme: that the "coordination" of the authoritative activities of "non-central" and "central" actors are likely to be yet another governing problem affecting national leaders coping with growing interdependence.

Some topics are not covered, which is quite legitimate, but because of the book's broad title the prospective reader should be aware of these. First, of course, the sample of nations is quite narrow. Authoritarian states, past and present, are not included; nor are the many ethnically fragmented states throughout the non-Western world. In fact, even such smaller advanced industrial democracies like Switzerland and Belgium are excluded. Second, the focus is clearly on the management of less controversial kinds of activities. Political crises surrounding ethnic conflicts and separatism are not considered; how central foreign policy makers deal with these severe "two level games" is not addressed. Third, the implications of federalism are not fully addressed with respect to the decision making within the central government. Little is said about the representation of regional interests in the legislatures and executives of central governments. The emphasis is solely on "non-central" actors as a somewhat autonomous channel in a transnational world. What is missing, and this is my sole criticism of the book, is how "non-central" influence is wielded within national institutions and how this, in turn, affects national foreign policy. The representation of sectoral interests in national legislatures and coalitions is an important second dimension of federal foreign relations, even among the stable advanced industrial democracies considered in this book. This, though, could be considered in a subsequent volume; the current volume has accomplished a great deal despite these gaps.

West Virginia University

JOE D. HAGAN

Localizing Foreign Policy: Non-Central Governments and Multilayered Diplomacy. By Brian Hocking. New York: St. Martin's, 1993. 249p. \$59.95.

The activities, influence and concerns of local governmental units-states, provinces, regions-in the formulation and implementation of the foreign policy of their

respective states appears to be a matter of increasing interest. Local governmental concerns with issues such as environmental pollution, economic development and international trade, and taxation of multinational enterprises that may be operating in the state cannot be ignored as matters of "low politics" by the central foreign policy establishment with its traditional focus on matters of "high politics." Brian Hocking, as author of the work under review and as editor of the recently published series of essays in Foreign Relations and Federal States (1993), is making a useful and timely contribution to the discussion of this important issue.

Although the author introduces the study with a chapter focusing on the localization of foreign policy, the emphasis of the book under review is better served by reference to the subtitle, Non-central Governments and Multilayered Diplomacy, rather than to the title. That local governmental units have some role to play in the formulation and implementation of national foreign policy, especially in federal states is not a new idea. That there may be tensions between the non-central governmental units and the foreign policy elite or establishment that operates as the central level is also not a new point. Hocking's argument goes well beyond these basic points. The traditional literature, he argues, tends to see the non-central governmental units operating as distinct and independent units which have incompatible interests with the central governments and which attempt to stake out their own sphere of influence. This argument, Hocking asserts, is misleading and fails to appreciate the complexity of the relationships between central and non-central governmental units. Hocking's argument is that the non-central units should be seen as participants along with the central governments in a process of multilayered diplomacy. It is not the case that the two units proceed separately in their diplomatic negotiations; rather the units interact with each other at the same time they may be interacting with the external governmental unit. The interactions between the central and non-central governmental units are complex, influenced by constitutional constraints and opportunities, occasionally confrontational, and occasionally supportive, and tend to be issue specific.

In making his argument, the author focuses on the role of non-central governments in three federal states: Australia, Canada, and the United States. Hocking offers three studies of diplomatic negotiations to support his basic argument: the United States-Canada free trade negotiations, the United States-United Kingdom dispute regarding the unitary taxation policies of several of the American states, especially California, and the United States-Canada negotiations over acid rain. These three diplomatic disputes offer insights into the role of noncentral governments in three agenda areas that are important to both the central governments and the non-central units: trade, taxation, and environmental protection. Of the three studies, the discussion of the United States-Canada free trade negotiations is the most fully developed in detail and in analysis.

In his general overview and conceptual introduction as well as his general review of managing multilayered diplomacy, which provide very useful insights, the author discusses briefly other issue areas and draws upon examples from Australia and New Zealand as well. Unfortunately the author's discussion and analysis in these sections is uneven and occasionally frustrating. Specific examples drawn from one state to illustrate a

particular relationship between central and non-central units are discussed but comparable questions that might be asked or illustrations that might be provided about the other states in his research are not pursued. Thus the reader is left wondering whether the relationship is state specific or whether it is a more generalizable one. In this sense one wishes that Hocking had been deliberately comparative in his approach.

Notwithstanding instances of occasional frustration, the reader will discover that Hocking sets forth a number of insightful and provocative prospositions about the nature of the relationships between central and noncentral governments dealing with foreign policy matters and how those relationships are managed. These propositions offer fruitful opportunities for future research for students of comparative foreign policy as well as federalism.

University of Maryland

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DON C. PIPER

Morality, Prudence, and Nuclear Weapons. By Steven P. Lee. New York: Cambridge University Press, 1993. 418p. \$64.95.

As strategically sophisticated as Robert Jervis, as morally subtle as Michael Walzer, as tautly logical as Thomas Schelling, as relentlessly tough-minded as Colin Gray, as imaginatively far-sighted as Jonathan Schell, and as calmly sensible as Lawrence Freedman, Steven P. Lee has produced a spectacular synthesis of the military and ethical considerations relevant to decisions about nuclear weapons in the wake of the Cold War. Strictly as an analysis of the purely "realist" considerations affecting deterrence and stability, or strictly as an analysis of the purely normative principles constraining strategic options, Lee's unfailingly lucid and scrupulously fair dissection of every major option would be invaluable. Yet, far from being either exclusively strategic or exclusively ethical, this remarkable study has genuinely interlocked morality and security (what the title somewhat archaically calls "prudence"). Constantly, moralists are confronted with the implications for stability and the likelihood of war of implementing their favored principles, and strategists are forced to unmask the moral reality of their favored options for preventing, limiting, and ending wars. This is, within one cover, the two best books I have read about nuclear weapons, the best one on the strategic logic and the best one on the ethical logic. The author has transformed both his subjects by restoring

The central argument has the deceptively simple thesis that morality and prudence—what is right and what is safe—must both be respected, not through some simple-minded compromise between the two, but each independently. Yet if morality requires policies that would likely be suicidal, while safety requires policies that are barbaric, we face an intolerable dilemma. Lee captures the dual necessity that morality does constrain self-interest significantly but does not mandate its surrender with his "principle of tolerable divergence" (p. 21). The basic question about nuclear weapons becomes, then, whether they have created an intolerable divergence between prudence and morality that can be remedied only by radical revision of either deterrent postures or ethical standards.

One of Lee's unusual contributions is to insist that the

question cannot be answered by thinking vaguely about counterforce targeting, countercity targeting, and nuclear disarmament, as almost all other normative theorists have done until now. His subtle exploration of the rich and critically different varieties within each general option defies non-caricaturing summary, but one pinnacle of Lee's argument is a brilliant demonstration (p. 286-91) of how the stability/instability "paradox," used valuably by Glenn Snyder and others, inadvertently masks the risk of accidental nuclear war. Lee's theoretical innovation in response to the inadequacy he shows in the stability/instability dilemma is the suggestion that we think instead in terms of three, not two, kinds of instability, treating risks of accidental war separately from both intentional conventional attack and intentional nuclear attack. This turns out to be especially apt in light of the simultaneous publication with Lee's more conceptual work of Bruce G. Blair's The Logic of Accidental Nuclear War (1993) and Scott D. Sagan's The Limits of Safety (1993). Lee uses this original conception of a triangle of instabilities to formulate a perspicuous prudential trilemma among minimum deterrence (of nuclear attack only), extended deterrence (of conventional as well as nuclear attack), and counterforce deterrence (modest and ambitious), showing that each of the three fundamental strategic options respectively solves a different one of the three stability problems but fails to deal with either one of the other two. And Lee argues powerfully that this is necessarily, not contingently, the case: just as the stability/instability "paradox" was said to be insoluble, the stability trilemma truly is insoluble. No form of nuclear deterrence can possibly "work," on an adequate non-dichotomous understanding of what it means for deterrence to "work."

Does this imply, then, that nuclear disarmament, unilateral or multilateral, is called for? In a fascinating final argument about policy, which employs Jonathan Schell's insight that nuclear disarmament is unavoidably still a species of minimum deterrence with the number of warheads set at zero, Lee argues that immediate nuclear disarmament would create the same instabilities that all forms of minimum deterrence create, unless nuclear weapons were first delegitimated to such an extent that they lost their prudential value in solving the other two stability problems that extended and counterforce deterrence now solve better than minimum deterrence does. The moral imperative, then, is first to delegitimate and only then to disarm. To delegitimate we must practice the opposite of the foolish forgetfulness Henry Kissinger advised and always remember with crystal-ball clarity the horror that nuclear weapons will otherwise always hold in wait, if not for now, then for later. Meanwhile, the best policy is minimum deterrence, and, as Lee shows through insightful pair-wise comparisons, neither the extended deterrence favored by leading political scientists nor the nuclear disarmament favored by leading philosophers. It will be prudent to abandon only nuclear weapons that have first been delegitimated, but morality now requires we must move urgently to avoid continuing to rely upon them. We must actively contribute to changing the norms of international relations, and what nations can expect from each other, in order to be able to change our own dangerous practice of deterrence safely.

The one serious lack in this magnificent piece of interdisciplinary scholarship is the absence of even a select bibliography, which Cambridge could easily remedy in the yet-to-be-issued paperback.

Cornell University

HENRY SHUE

Beyond Charity: International Cooperation and the Global Refugee Crisis. By Gil Loescher. New York: Oxford University Press, 1993. 260p. \$35.00.

Very few persons know more about refugee affairs than Gil Loescher of Notre Dame University. Loescher has now put in one volume most of the basics, and some new ideas, about this perplexing and important subject. Writing for the Twentieth Century Fund, he has provided ample background to such contemporary developments as the exodus of over two million persons from Rwanda during 1994.

This is an essential book for scholars, students, and libraries. Perhaps its only major detraction is that specialists will have to wade through some well known points in order to get to the new ideas. Loescher acknowledges (pp. ix–x) that the book is meant to appeal both to the experts and others. But if the specialists can skim the basics, the rest will prove rewarding. Theorists will find that Loescher is not interested in speculating about what refugee affairs means for "regime theory" or other debates about various forms of "realism" or "liberalism" as schools of thought for world politics. Nevertheless, ample material is here that could be applied to those debates concerning, inter alia, whether states dominate world politics or whether international organizations can act independently.

The fundamentals are stated in admirably clear prose and based on carefully researched facts. The number of conventional refugees (those protected under international treaty as having a well founded fear of individual persecution), war refugees, and internally displaced persons has mushroomed. The Office of the United Nations High Commissioner for Refugees (UNHCR) has not been able to keep pace with the demands on it for protection and assistance. The OECD countries (24 industrialized democracies form the Organization for Economic Cooperation and Development) are increasingly shutting their doors in the face of increased migration of asylum seekers, even though the numbers they face (a 700% increase from the early 1980s to 1991) are much smaller than the number of refugees hosted by the developing countries. "In Malawi . . . where the GNP per capita is only \$170, one in every ten persons is a refugee from Mozambique. This is the equivalent of the United States . . . suddenly admitting over 25 million Central Americans—the entire population of the region'

Given this situation, Loescher argues that those dealing with refugees (and with persons in a refugee-like situation) must approach the problem from a political, and not strictly humanitarian, standpoint. He argues that rather than building barriers to newcomers, OECD states should integrate refugee policy with other policies pertaining to human rights and also development (see p. 127). This means that OECD states should deal with the human rights problems that create refugees, and should link trade and other policies designed to produce economic growth to projected refugee solutions.

The situation is complex. Loescher notes that development policies may actually stimulate migration during certain stages of growth for certain regions, concluding that "any pro-development policy is unlikely to discourage mass departures from the South and the East in the near future" (p. 169). He continues to argue, however, for a long term development policy designed to keep citizens at home. He also takes a firm stand in favor of conditionality—the tying of development aid to human rights reforms in pursuit of pluralism and humane governance.

In his final chapter Loescher lays out an action plan for the future. While commendable in terms of morality and reason, much of this vision is no doubt beyond the reach of most states in the near future. Given the reluctance of OECD states to exercise leadership and bear the costs of paying serious attention to human rights in places like Bosnia or Rwanda, it is unlikely that they will, for example, amend the United Nations Charter to permit binding decisions by the Security Council for human rights rather than for security affairs (see p. 197). In fact, so far this has proven unnecessary, since the Council reinterprets human rights issues to mean security issues when the political will is there to take a binding decision. The primary problem is not lack of legal authority but lack of political will. Moreover, the U.S. in particular has numerous reasons of self-interest for avoiding Charter revision, fearing, inter alia, the results of many Southern votes in any such process.

Other ideas are stimulating, even correct, but ahead of the times politically. It is true that the UN Secretary-General "needs to have at hand a permanent standby military capacity" (p. 187). But there is a slight problem in that the Pentagon has rejected this idea. We should indeed move away from a situation in which the Conference on Security and Cooperation in Europe "lacks teeth to implement its decisions" (p. 191). But neither the 54 states of the CSCE nor the 16 states of NATO have found a sure and systematic way to do this in Bosnia and elsewhere. It would certainly be well for the exercise of state sovereignty to be predicated on responsible and humane behavior (p. 183). The international community may be stumbling toward recognition of this principle. But we are not there yet, as authoritarian rulers know from China to Iran.

The visionary aspects of Loescher's final chapter not withstanding, this is a fine book, at once informative and creative. If combined with other recent material, such as the relatively new annual report on refugees by the UNHCR, or an earlier volume edited by Loescher and Laila Monahan, *Refugees and International Relations* (Oxford 1989), a reader can be well informed about this major and growing issue of world politics. The primary impediment to solution of the myriad problems is just one small matter: adequate political will by the OECD states that provide most of the material resources, and most of the potential places of resettlement, for refugees.

University of Nebraska, Lincoln

DAVID P. FORSYTHE

Decision and Interaction in Crisis: A Model of International Crisis Behavior. By Ben D. Mor. Westport: Praeger, 1993. 179p. \$49.95.

Contemporary scholarship in political science on the causes of international crises and war is deeply divided by alternative approaches to both theory building and empirical analysis. Ben Mor attempts to build bridges across the typical divides in the field by developing a formal game-theoretic model of crisis initiation and outcomes which allows for the influence of psychological variables. Furthermore, he then empirically tests the formal model in a case study of decisions by Israeli and Egyptian leaders in the crisis which escalated into the Six Day War of 1967. The book has much to offer to both scholars with a general interest in theories of crisis initiation and escalation as well as those with a more specific interest in the historical debate about the causes of the 1967 War.

Mor's premise is that powerful theories of crisis behavior require the integration of insights from both rational choice game-theoretic approaches as well as the cognitive psychological literature which focuses on the limits to rationality. In particular, Mor argues that one fruitful approach to theoretical synthesis is to incorporate into game-theoretic models psychological variables which allow for the preferences of decision makers to change within a crisis. As a result, Mor argues that more dynamic game-theoretic models of crisis behavior can be developed.

The strength of the book lies in the lucid and very accessible presentation of the game-theoretic model (chapters two and three). Mor clearly presents a typology of different crisis player types and then discusses the logical patterns of strategic behavior produced by alternative combinations of player types interacting with one another. From this discussion a range of intuitive as well as more nuanced hypotheses about the role that misperception, resolve, military strength, and deception play in crisis initiation and escalation is presented. The case study (chapters four and five) is also well done with the empirical analysis driven by the testable propositions derived from the model. Mor uses the model to make a very interesting argument that much of President Nasser's behavior can be explained in rational choice terms in which Nasser was trying to use deception to achieve the best possible outcome short of war for his country (himself). In the end, Mor argues that Nasser's strategy of deception was too successful since when Nasser stopped escalatory actions and attempted to resolve the crisis short of war, the Israeli leadership was firmly convinced of his resolve to use force and to not compromise and therefore launched what they believed was a necessary preemptive strike.

The major shortcoming of the book is Mor's failure to actually incorporate in a generalizable and systematic fashion psychological variables into his game-theoretic model. This is without question a formidable task but Mor posits this as one of the principal contributions of his book and thus it is imperative that he be judged on those grounds. The weaknesses are interrelated and twofold. First, Mor contends that psychological variables are incorporated into his mode! when he discusses the different components of a crisis player's preferences. It is not at all clear, however, in his discussion (chapter two) how psychological theories or insights provide the foundation for the formation of different attitudes or beliefs such as "peace with victory" vs. "peace without victory" or "make the opponent pay" vs. "cut costs." Similarly, Mor does not develop a link between psychological variables and conditions under which we might expect decision makers to change their attitudes or beliefs.

Second, because there is no strong theoretical basis for

predicting the preferences of players in the crisis model, it is difficult to explain and generalize about when and why preference changes occur within a crisis. Earlier in the book (chapter one), Mor criticized existing gametheoretic models for their inability to accommodate change in the basic preferences of players but ultimately Mor makes limited progress on this issue. This weakness is apparent in the case study of the Six Day War when Mor is unable to explain why Nasser seemed to have changed his attitude towards loss during the crisis. When such a change in attitude, however, is assumed to have taken place, then Mor is able to draw out the logical consequences of such a change in Nasser's preferences for expected crisis behavior and outcomes. The danger is that ad hoc explanations could often be introduced into the model to explain away apparent mispredictions by arguing that player's preferences must have changed which then produced a different set of expected behav-

Mor is sensitive to such problems and avoids such pitfalls in his own empirical analysis. Indeed, Mor is refreshingly straightforward in discussing the strengths and weaknesses of his own theoretical and empirical work. In sum, Mor has tackled a large and complex topic in the field of international politics. While he has not fully succeeded in resolving the theoretical problems that he had identified in the existing literature, he deserves credit for pinpointing the right problems, outlining in general form the types of answers we need to develop, and attempting to constructively build on the work of scholars from alternative theoretical approaches.

University of Michigan

Paul K. Huth

Regime Theory and International Relations. Edited by Volker Rittberger. New York: Oxford University Press, 1993. 470p. \$57.00.

The concept of international regimes, far from being the passing fad Susan Strange predicted in 1983, has led to vigorous research agendas in the United States and Germany. The purpose of this volume is to explore these research initiatives, comparing the work across continents and among theoretical traditions. Presented at a conference, critiqued, and revised, the papers examine different facets of regime theory. How is cooperation possible between states claiming sovereignty? The answer is found in the relationship between the idealist's neoliberal institutional emphasis on law and norms and the realist's power and interests.

Rittberger's introductory essay traces the differences between the American and German approach to the problem in terms of issue areas and preferred approaches to outcomes. Keohane's introduction provides an overview of the essential questions and findings in this transnational research agenda. Each offers an excellent synthetic overview, which both expands and updates Haggard and Simmon's literature review (1987).

The essays are arranged in three major sections. The first group places regime theory in broad theoretical traditions or examines a "piece" of the theoretical puzzle. In the former category, Hurrell sees regimes within the framework of international society, community, and cooperation, as distinct from international law; Kratochwil places regimes within the logic of contracting

by identifying five distinct forms of contracts; and Kydd and Snidal plead for theoretical diversity in regime study by returning to the contributions of game theory as a way of providing analytical rigor and coherence. In the latter area, Haufler examines nongovernment organizations as parts of regimes often understudied in the literature. One important thread connecting these theoetical essays is recognition that domestic structures and processes are too often overlooked.

In the second group of essays, regime formation and transformation is the focus. These articles explore the dynamics within regimes, across regimes, and at different stages of the regime process. For example, Krasner's contribution focuses on the interplay between the sovereignty metaregime and specific human rights issues, while P. Haas's essay traces regime dynamics within the issue of the environment, with special emphasis on knowledge of the epistemic community. Two of the essays present findings from major ongoing projects, both asking similar questions: Young and Osherenko summarize their findings on regime formation, based on power-based, interest-based, and knowledge-based hypotheses, while Efinger, Mayer, and Schwarzer do the same for an elaborate set of studies in the Tübingen Project. In both articles, the specification of alternative hypotheses provide excellent material for students attempting empirical regime research using systematic comparative case studies. Equally useful are the generation of multicausal frameworks so necessary for explaining social phenomena.

Formation and change in international regimes (as many theorists in the first section assert) is intimately linked to domestic structures and processes. Jönsson makes the link by suggesting that researchers explore the cognitive world of decision makers to enrich the regime framework. And Zürn, as his title suggests, brings the second image (back) by identifying both foreign policy types and actor characteristics (interest profiles) that are regime-conducive. This excellent article offers some suggestive foreign policy types that can be linked to regime-conducive behavior and specific complex hypotheses, and utilizes the case studies of the Tübingen Project to produce innovative findings. Zürn and his colleagues have clearly provided a useful approach to linking domestic and foreign policy behavior with international regimes.

The third group of essays examine regime consequences: Do regimes matter? Previous researchers who have dealt with regime outcomes have puzzled over the methodological dilemma: How can you prove that X causes Y, that the presense of a regime makes any difference? Müller, like other scholars, focuses on regime compliance as one requirement of regime effectiveness. Regime norms are internalized within the domestic process.

More broadly, Biersteker wrestles with the methodological dilemma by proposing use of historical counterfactuals. He offers students of regimes a systematic approach to the problem: If there had been no global debt regime during the 1980s, how would have the outcomes been different? Breitmeier and Wolf utilize evaluative criteria—a standard of stable peace—to assess regime consequences in the area of the environment. Does the regime contribute to the advancement of civilization and to democratization domestically? Each author is cognizant of both the methodological problems presented and of the necessity to examine both domes-

tic-level and system-level factors in assessing regime consequences.

Regimes analysis has become a part of the core of international relations. In the last outstanding chapter, Mayer, Rittberger, and Zürn suggest how knowledge about regimes has been advanced by the contributions in the volume. Most importantly, they place regime analysis within a broader literature: How can there be goverance without government? How can there be cooperation without anarchy? Both macro and micro approaches utilized in the volume have propelled this research ahead.

In short, the book is one of the first systematic overviews of regime theory and its empirical referents since the seminal volume of International Organization in 1982. The work shows how a primarily American research agenda has been developed and amplified by European scholars, offering some findings heretofore available only in German-language publications. Several of the articles will "stand alone" as pieces of integrative scholarship, assessing the current state-of-the-art in regime research for future generations of scholars (Rittberger-Keohane-Mayer, Rittberger, Zürn). Others provide conceptual breakthroughs to parts of the regime puzzle (Zürn, Haufler). Still others will stand as summaries of research work elsewhere (Kratochwil, Haas, Young-Osherenko). And several provide key and innovative methodological parts of the puzzle (Biersteker, Efinger-Mayer-Schwarzer). Regime theory is significantly advanced by the publication of this volume.

University of Kentucky

KAREN A. MINGST

Grasping the Democratic Peace: Principles for a Post-Cold War World. By Bruce M. Russett. Princeton: Princeton University Press, 1993. 173p. \$19.95.

Bruce Russett's laudable book summarizes, dissects, and expands our understanding of the disinclination shown by democracies to fight each other, a finding that has spawned a minor cottage industry of analytic studies. Russett's principal contributions include the very broad empirical domain to which he applies his research, as well as his close scrutiny of the respective role played, on the one hand, by the institutional constraints that limit a democratic government's freedom of action, and, on the other hand, by the normative and cultural consensus that underlies political democracy. At a more general level, the book provides a powerful challenge to core assumptions of political realism and neorealism.

Starting from the observation that democracies hardly ever fight each other (although they show no disinclination to fight nondemocracies or to skirmish with each other short of warfare), Russett dismisses a number of spurious explanations for this regularity. For example, it is true that democracies tend to share common international institutions, and this in itself may discourage fighting; but, as he points out, peaceful relations generally precede the creation of shared institutions. In Russett's view, two properties of democracy deserve particularly close scrutiny: its normative consensus and its institutional structure.

Democratic norms and culture cannot make these countries inherently more peaceful—else, why are democracies so willing to fight autocracies?—but they may well operate to decrease bellicosity among democracies.

The normative understanding that promotes peaceful resolution of societal conflicts within democracy may be generalized to relations between nations espousing similar democratic beliefs. Accordingly, it may not be considered legitimate or necessary to resort to violence against other democracies, nor is aggressiveness from them feared (making preemptive attacks unnecessary). The same reasoning does not apply to relations between democracies and autocracies. At the same time, structural and institutional constraints stand to play a role, since democracies are constrained from going to war by the need to ensure the support of public and a consensus between branches of government (although this may make them somewhat more vulnerable to aggression by autocratic states).

Using a continuous (though rather complex) measure designed to assess the degree of a country's adherence to democracy and controlling for a variety of possible confounding factors (e.g., wealth), Russett examines the respective impact of measures of institutional constraints and normative restraints on war-proneness of pairs of nations during the past half-century. He concludes, as did previous studies that used a binary (democracy—nondemocracy) measure of political system, that while democracies are not less belligerent in general, they are certainly more peaceful in their relations with each other. In addition, his analysis suggests that democratic norms provide a more powerful inhibition to warfare among democracies than do institutional constraints (which, nevertheless, remain influential).

One of the book's original contributions resides in the empirical domains within which the relationship between democracy and peace is tested. A variant of the test is applied to the system of Greek city-states of the late fifth century B.C., though the results prove somewhat ambiguous. On the one hand, city-states that could, by some definition, be characterized as democracies did fight each other less often than those who probably or certainly were not democracies. On the other hand, those that were most unambiguously democracies did fight more often than predicted. Still, this finding might be distorted by the fact that all of the wars between clear democracies were part of the Athenian campaign against Syracuse, not completely independent events. (Also, the democracies were disproportionately naval powers, with an ability to project power and, hence, to fight.)

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A variant of the test is also applied to nonindustrial societies, that is, populations that contiguously inhabit a territory, speak a common language and contain political units of various levels of aggregation (e.g., bands, villages, districts). Controlling for size of political unit and the effect of insularity, Russett finds that political units characterized by a high degree of political participation (a surrogate measure for democracy in a preindustrial society) are unlikely to fight each other. Accordingly, the relation between democracy and peace seems robust: it holds when a number of confounding factors are controlled for, and it applies to a wide variety of empirical domains.

One could quibble about certain technical details of analysis. For example, it may sometimes be hazardous to infer the power of political norms from a country's political stability and its lack of political violence (the Soviet Union displayed both qualities over numerous decades). Nevertheless, the book combines rigor and relevance, maturity and originality; it takes the democracy-peace link further than it has yet been taken and draws thoughtful policy implications from empirical research. Perhaps most importantly, it suggests that the political realist's Hobbesian conception of world politics may be decreasingly relevant in a world of democracies; and it urges us to take more seriously than political realism ever could the proposition that "the policies of states in international relations, and their peoples' support for those policies, derive in large part from their fundamental values and images."

University of California, Davis

MIROSLAV NINCIC

The Limits of Safety: Organizations, Accidents, and Nuclear Weapons. By Scott D. Sagan. Princeton: Princeton University Press, 1993. 286p. \$29.95.

This book is a significant contribution to three fields: international security studies, organization theory, and risk analysis.

Grounded in original research in U.S. national security archives, Limits reveals a disturbing history of nearcatastrophes in the handling of nuclear missiles and bombers. During a period of high tension when pilots had been told there would be no tests, for example, a bear climbing a military-base fence in Minnesota touched off a "nuclear war has begun" alarm in Wisconsin, and only an officer in a jeep flashing his lights on the runway stopped nuclear-armed aircraft from taking off toward the Soviet Union. During the Cuban missile crisis, radar stations mistakenly showed practice tapes of a Soviet missile attack just at the time a satellite came over the horizon in the expected trajectory, leading to a report that Tampa had been destroyed. Alcohol- and drug-abusing nuclear weapons guards engaged in suicidal and homicidal behavior. Accident, curiosity, and bravado led American pilots to arm nuclear bombs and otherwise violate standard procedures. And virtually all the problems were either covered up or failed to serve as a source of organizational learning. The book moved me to reappraise the relative proportions of luck and strategy that went into the Cold War's absence of accidental nuclear detonations; I predict most readers will join me in that reevaluation.

The historical incidents are interpreted through lenses provided by organization theory. Limits formalizes, extends, and tests the comparative usefulness of two theoretical traditions: (1) Charles Perrow's normal accidents theory, holding that system failure eventually is inevitable in tightly coupled and complexly interactive organizations, such as the U.S. missile command; and (2) "High reliability theory," Sagan's own interpretation of research by La Porte/Rocklin/Roberts, Wildavsky, and Morone/Woodhouse, who seek in disparate ways to understand how organizations protect against catastrophic outcomes in the face of high uncertainty.

Sagan argues that normal accidents theory is the more illuminating theoretical tradition for his cases, since—time after time—redundancies and other built-in protections are overridden by organizational actors pursuing goals other than minimizing American citizens' risks from accidental nuclear discharges. SAC has a stake in keeping bombers flying, so it uses a bomber as a redundant warning system despite the possibility it can crash on a radar; a pilot wants to protect his career, so he covers up a potentially catastrophic error; a commander

wants to avoid criticism, so he blames problems on subordinates rather than on shortcomings in policy.

Does the argument stand up to scrutiny? For the most part, yes; one comes away from *Limits* with a profound appreciation of the roles of organizational routines and actors' interests in shaping and misshaping the implementation of nuclear weapons safeguards. Extrapolation to other hazardous technologies is but a small, and justified, step—as evidenced by the *Exxon Valdez* and Bhopal accidents, among others correctly attributable to organizational dysfunction, and not merely to individual human error.

If the book has a shortcoming, it is that its theoretical treatment could be framed more even handedly. *Limits* is marvelous in depicting organizational forces leading to the breakdown of safety procedures, and the picture thereby painted is chilling. But it is perhaps equally important to ask, "Given tens of thousands of nuclear warheads, controlled by flawed humans who were supposed to respond almost instantly to ambiguous signals of Soviet threat, how did SAC and the other parts of the nuclear missile command manage to get through the Cold War without an accidental or erroneous detonation?"

Part of the answer is revealed by La Porte et al.'s studies of emergency flight deck operations on aircraft carriers, management of multi-state electricity grids, and other high-reliability organizations. These scholars have identified social-organizational requisites for near-perfect performance, including a thoroughgoing commitment to safety by top executives in the organization, intense training and socialization, and redundant backup systems. Neo-incrementalists Morone and Woodhouse find that complex, risky endeavors succeed in direct proportion to the extent there is diversity of participation in decision making so that fewer considerations are overlooked, flexibility in policy trials to enable error correction, redundancy and other means of mitigating the consequences of the inevitable errors, and vigilant monitoring of policy trials. Most of these decision strategies are to be found in the U.S. missile command, and arguably go far toward explaining the extent of its success in preventing accidental detonations.

Still, Sagan is right to underline the limitations of such high-reliability strategies. *Limits* causes us to focus more attention on conflicts of interest in organizations, even in those managing nuclear weapons and other hazardous technologies. Of redundancy, Sagan argues that at times it actually makes matters worse, inadvertently causing the very problems it was designed to prevent: SAC's redundant B-52 *did* crash next to the radar station in Thule, Greenland, for example, almost providing simultaneous blinding of two information sources and thereby increasing Air Force monitors' confidence that a Soviet attack was in progress.

Of flexibility, Sagan points out that partisans within one organization will not necessarily find it rewarding to pursue strategies that might be sensible from some Olympian viewpoint. On socialization, Sagan argues that strong organizational control can encourage excessive loyalty and coverup to protect the reputation of the organization. These and other arguments move the normal-accidents approach to organizational reliability a good bit beyond Perrow's largely structural views, offering a richer organizational politics interpretation.

I feel confident that my fellow "high reliability theo-

I feel confident that my fellow "high reliability theorists" would join Sagan in expecting organizational politics (and the egregious shortcomings of contempo-

rary democratic politics) often to interfere markedly with learning from experience and other facets of organizational reliability. In the case of nuclear weapons, moreover, everyone can agree that nearly perfect is not good enough. How, then, to do better?

Normal accidents theory recommends making an organization less tightly coupled and less complexly interactive, so that breakdowns in one part of an organization or activity do not ramify into larger and more catastrophic problems. One of Sagan's illustrations of this is to move warheads away from missiles in the post-Cold War era, allowing more time to evaluate whether an alarm is real, and less opportunity for catastrophic accidents. (Bombs now have been removed from bombers during peacetime, following a fire in a nuclear-armed B-52).

Altogether, Limits is perhaps the outstanding contribution to date in a developing genre that is rejuvenating the organization-theoretic approach to national security studies. After a long hiatus when Halperin, Steinbruner, and Allison moved into administrative positions and when neorealists and rational actor theorists were at their apex, Sagan reinvigorates the use of intra-organizational loyalties, routines, and power relations as means of understanding inter-organizational behavior in national security realms. (Also see Sagan's organization-theoretic analysis of nuclear proliferation in International Security.)

Sagan's conceptual analysis advances as well our theoretical ideas about organizational reliability under conditions of high uncertainty and potentially catastrophic risk. The book is sure to stimulate scholars from several traditions to sharpen their arguments to take account of Sagan's criticisms and insights. *Limits* also will encourage risk analysis scholars to take organizational politics into better account.

Finally, for organization theory more generally, Sagan joins Perrow, La Porte et al., and a handful of others in forging a normative wing of the field. These scholars are concerned with developing theories about how organizations do work in order to understand how organizations and their networks can work more reliably, more fairly, and otherwise "better." As a stimulus to further development of relevant organization theory, Sagan's contribution is most welcome.

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E.J. WOODHOUSE

The Limits of State Autonomy: Societal Groups and Foreign Policy Formation. Edited by David Skidmore and Valerie M. Hudson. Boulder: Westview, 1993. 328p. \$42.50 paper.

This book is the first product of an occasional series of edited volumes sponsored by the Foreign Policy Analysis Section of the International Studies Association. Despite a growing number of publications in recent years, the role of domestic factors in explaining foreign policy remains a relatively underdeveloped field, and this is even more true for the role of societal groups. As the editors argue convincingly (p. 4), and I agree whole-heartedly, theories of international relations which focus at the systemic level of analysis may complement but cannot substitute for models which specify the domestic determinants of foreign policy. Our knowledge in this area, however, leaves much to be desired. Any effort to

fill this gap should therefore merit applause and our attention.

The papers in this book were selected from among those received in reply to an invitation sent out in 1990. The editors have certainly been successful in selecting a number of papers which show, as they put it the "diverse theoretical and empirical approaches while assuring that the volume as a whole addressed the role of societal groups in varying national contexts" (p. vii). At the same time, this volume makes no exception to the general rule that the shortcomings of this kind of volumes are often all too obvious, i.e., the absence of a general framework and a convincing rationale for the ways and theoretical reasons why the respective papers were chosen, as well as the absence of any effort to draw general conclusions and in particular, to provide an answer to the question: "where do we move from here?"

It is true, indeed, that in an introductory chapter the editors do try to bring some (theoretical) order in the research agenda and the diversity of materials in this volume. They do so largely by distinguishing between what they refer to as statist, societal and transnational approaches or types of state-society relationships. This typology is then used to provide the categories by which this volume is, somewhat artificially, organized. These distinctions, however, do not return as organizing principle in the other theoretical chapters (including an interesting effort to provide a framework for comparative studies to which I return below)—however interesting and ingenious they are in themselves—and neither are these theoretical chapters related to or informed by the empirical chapters. To the extent that the latter have theoretical ambitions they point in rather different directions. Apart from the introductory, theoretical chapter, the book contains two other efforts at theorizing. One, by Douglas van Belle, looks at the options and strategies which rational leaders and governments have in dealing with forms of domestic opposition to foreign policy. In order to stay in power they will have to defer in some ways to these domestic pressures. However elegant and intriguing the elaboration of this model of reaction is, it does treat "domestic response" to government actions as a general category, and therefore does not really help us to understand the conditions under which societal groups in particular have a specific impact on foreign policy.

In the most ambitious (and longest) chapter, by Valerie M. Hudson, Susan M. Sims and John C. Thomas, a detailed effort (including coding rules) is made to develop a framework of analysis for comparative studies of domestic factors in foreign policy making. It is argued, correctly, that one needs to bring together country experts' knowledge with a generalized, systematic treatment (p. 55). This, however, remains a daunting task. This chapter, by primarily listing and defining the variables concerned can only be considered a first step in the right direction. Theorizing on the assumed relationships between the variables needs to be carried much further. For this, we keep needing heuristic case studies.

The book contains a number of such studies, which, on the whole, make for stimulating reading, and which raise many intriguing questions and hypotheses, which, however, in most cases are only answered and confirmed in an ad hoc fashion. Moreover, some of the case studies deal with issues which have continued to evolve after the respective papers were finished. Due to the long production time of the book, this was often a

considerable time ago. In the cases of South Africa, the Israel-Palestine conflict and the end of the Cold War, the reader gets the feeling of being left in mid air when the narrative stops.

Nevertheless, the case studies retain their use to the extent that they suggest theoretical implications. In the study of the struggle over sanctions against South Africa in the United States, by Elizabeth S. Rogers, it is the concept of domestic costs and benefits and the impact of American blacks on foreign policy. David Skidmore, in his study of the domestic struggle over arms control, in particular the SALT II treaty, explores why proponents of arms control failed to produce a counterweight to the massive campaign of Cold War hardliners, and suggests how his results could be generalized. In his case study of efforts to combat the international drug trade at the beginning of this century H. Richard Friman shows the usefulness of "historical" studies as well as emphasizes the ways in which governments resort to duplicity and deception in order to cope with the crosspressures of domestic groups and international counterparts and avoid unpalatable compromises. Tamar Hermann contributed an interesting explanation of the gradual emergence of an independent peace movement in Israel in spite of the prevailing "siege syndrome" characterizing the public mind in this country. Finally, the book contains two contributions on the role and relevance of transnational movements and forces, one, by Ellen Dorsey, which is more general and philosophical-theoretical (and not entirely convincing), the other, by David S. Meyer, which is more empirical and focuses on the peace and human rights movements in Europe and the United States, and their role in stopping the arms race and bringing about an end to the Cold War. Both fail, however, to deal convincingly with the many conceptual and methodological issues raised by the analysis of the impact of transnational forces on international relations and individual foreign policies. Among the latter, the problem of separating the influence of societal groups from other potential causal factors remains the most pressing one, even after reading this book.

The most informative (and, for students probably most useful) chapter is the one by Harald Muller and Thomas Risse-Kappen, "From the Outside In and from the Inside Out," which surveys the voluminous literature (including a number of European writings which tend to be less known among Americans) and argues the need for empirical research which takes both the international/systemic level and the domestic level into account in foreign policy analysis.

Leyden University, Netherlands

PHILIP P. EVERTS

Feminist Theory and International Relations in a Postmodern Era. By Christine Sylvester. New York: Cambridge University Press, 1994. 265p. \$54.95 cloth, \$16.95 paper.

Where might feminist theory find ontological, epistemological, and political homes in the field of international relations during a postmodern era? This is a central question raised by Christine Sylvester's latest contribution to feminist theory debates. Posing this question enables Sylvester to explore how various feminist positions (feminist empiricism, feminist standpoint, feminist postmodernism and postmodern feminist

nism) are applied to international relations. After a theoretical consideration of these feminist approaches (chapter 1), she rereads the three discipline-defining debates in international relations theory (idealism vs. realism, traditionalism vs. scientific behavioralism, and positivism vs. post-positivism) through them (chapters 2-4). In so doing, Sylvester highlights how international relations theory has excluded and continues to exclude feminist contributions while she effects a feminist repositioning of international relations theory. Read through Sylvester's compendium of feminist interventions, international relations theory is not simply supplemented by feminisms. It is transformed by them, as she illustrates with her analyses of how women's activities at the Greenham Common and in women's cooperatives in Harare, Zimbabwe, reinscribe international relations concepts of security and cooperation (chapter 5). These are but two illustrations of what Sylvester calls a "repainting of the canvases of IR" (chapter 6).

This repainting is attempted through a questioning of subjectivity, linked to epistemology/methodology and concern with political action. Sylvester stresses the necessity "of exploring the possibilities for identity hyphenations, metamorphoses, and mobilities" so that we may "consider various ways of knowing that help us to slip and slide around identity-constituting subjectivities in theory-useful ways" (p. 10). It is on these issues that Feminist Theory and International Relations in a Postmodern Era is the most theoretically ambitious. Making ontological/epistemological/political connections leads Sylvester to (1) propose two concepts—what she terms homesteading and empathetic cooperation—as the methodological basis of a postmodern feminist approach to IR; and (2) argue in favor of postmodern feminism and against feminist postmodernism as a guide to political action in a postmodern era.

For Sylvester, homesteading refers "to processes that reconfigure 'known' subject statuses . . . in ways that open up rather than fence in terrains of meaning, identity, and place" (p. 2). It is marked by the "double images of emancipation and exclusion" (p. 2), for as Sylvester employs it, homesteading becomes a site of potential emancipation while drawing attention to exclusions of gender, feminisms, (unproblematized) women, and (socially constructed) "women." Empathetic cooperation is configured as a method which does "not simply rehearse old inclusions and exclusions or reinforce old readings. Empathy leads to listening to the excluded, listening to their sense of the good, knowing that they will present a fractured and heavily contested discourse because they have been simultaneously inside and outside a master narrative. Cooperation comes in rescripting agendas to reflect the subjectivities that have been etched into the identities of empathetic listeners"

Homesteading and empathetic cooperation are best expressed by what Sylvester (following Nancy Hirschmann, Rethinking Obligation [1992]) labels postmodern feminism, not to be confused with feminist postmodernism. "The subtle point of difference between a feminist postmodern and postmodern feminist position would be that the latter would tolerate the politics of self-assertion on the way to a politics of empathetic cooperation" (p. 53). In other words, Sylvester argues that feminist postmodernism risks abandoning subjectivity—a necessary home for political action. She critiques some postmod-ernists by arguing that, "To cut ties to socially constituted sex at this time is to risk leaving certain subjects out of the future by virtue of ignoring any of their stories that revolve around gender experiences" (p. 182). She embraces postmodern feminism because she argues, "Only when ontology is affirmed can the politicized Self relax into many hyphenated identities" (p. 53).

Sylvester's theoretical discussion of the differences

between feminist postmodernism and postmodern feminism in some respects occludes rather than sheds light on how these two feminisms theorize ontology. For example, Sylvester hints that for feminist postmodernists, "women" have no basis in material reality when she writes, "If gender is a home for some 'women,' then it makes no sense to refuse them that identity because, technically, it is not real" (p. 53). But feminist postmodernists do not deny the material reality of women, nor do they refuse gendered implications of women. Instead, they are attentive to how the representation of real women in language obscures or overwrites women's realities. In this sense, feminist postmodernists widen the scope of politics by insisting on the materiality of language. Political action for feminist postmodernists includes exposing and disabling discursive techniques of exclusion, domination, and erasure. While this involves an analysis of the textual construction of ontology, it hardly amounts to its denial.

The differing approaches of feminist postmodernism and postmodern feminism to questions of ontology is not, as Sylvester suggests, a matter of the first approach denying ontology while the second embraces it. Instead, their differences concerning ontology hinge upon their relationships to boundaries. Feminist postmodernists problematize boundaries; postmodern feminists, like standpoint feminists, affirm them. For postmodern feminists, what is within these boundaries may be multiple, hyphenated, and mobile, but the boundaries themselves are accorded a degree of acceptance. The effect of postmodern feminism is to construct a "home" for a stable and self-identical subjectivity, however plural that identity may be.

In its search for homes, Sylvester's account of postmodern feminism shares a nostalgia for a politics of place (William Connolly, "Democracy and Territoriality," [1991]) that Sylvester finds so contemptible about most of international relations theory. While postmodern feminism's nostalgia is rooted in a desire for feminist political action in a postmodern era, it paradoxically betrays a fear concerning "the replacement of truth [stable subjectivities] with politics [calling the boundaries which fix identity into question]" (my brackets; Wendy Brown, "Feminist Hesitations, Postmodern Exposures," differences 3, 1:63-84, 1991). What eludes Sylvester's attention is that feminist political desires can be satisfied from a postmodern feminist unbounded or nonplace, just as it can be from feminist empiricist, feminist standpoint, and postmodern feminist positions. One need not meld standpoint epistemology with postmodern sensitivities in order to enable feminist politics in a postmodern era. Indeed, this move returns political action to a radical form, as Sylvester herself concludes when she argues that political action for a postmodern feminist means a return to socialist politics (p. 63). Rather than affirm how feminist politics is carried out from various feminist positions and nonpositions, Feminist Theory and International Relations in a Postmodern Era ultimately reinscribes an arbitrary boundary between

what is political and what is apolitical by (to use Sylvester's term) evacuating postmodernism ontology, epistemology, and politics out of postmodern feminism. For all of its important contributions to the study of international relations and feminist theory, this aspect of Sylvester's textual canvas repaints international relations theory in strokes that are all too familiar.

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CYNTHIA WEBER

International Migration and Security. Edited by Myron Weiner. Boulder: Westview, 1993. 333p. \$55.00.

With the end of the Cold War, the concept of what constitutes national security has changed dramatically and acquired a new and complex significance. Considerations of national security have expanded from traditional military—political issues at the interstate level (e.g., protecting a country's borders or continued existence from external threats) to many new concerns, including refugee and migration flows.

Refugees and displaced people are high on the list of international concerns today not only because of their humanitarian importance but also because of their impact on peace and security. It is no longer sufficient to discuss the subject of forced migration within a narrow national context or as a strictly humanitarian problem requiring humanitarian solutions. Despite this recognition of the underlying political nature of refugee movements and of the strategic effects of forced migration, surprisingly little systematic research has been carried out to explain the interrelationships among foreign policy factors and international security concerns and population movements. A conference, "The Impact of International Migration on the Security and Stability of States," held at MIT's Center for International Studies in Cambridge in December 1991 resulted in what is the first edited book on the subject, International Migration and

The 11 essays in International Migration and Security attempt to develop an analytical focus in order to link these previously disparate fields of international security and population movements. Myron Weiner's introductory essay provides an excellent overview of the variety of ways in which migrants and refugees are perceived as potential threats to the security of states. Part 1 analyzes the migration flows that make societies ethnically either more homogeneous or more diverse, and examine the consequences of these movements for the security and political stability of states. Rogers Brubaker details the changing relationship between the ethnic minorities in the successor states of the Soviet Union and their own newly independent homelands and argues convincingly that a key determinant of future population movements will be how the debate works out over whether people of migrant origin enjoy the same rights as indigenes. William Zimmerman looks at how the war among the successor Yugoslav republics are aimed at moving borders and peoples so as to create ethnically more homogeneous societies. Rosemarie Rogers describes how and why migration has come to be regarded by many Europeans as a threat to societal stability and argues that the fundamental issue for most West European countries remains one of how to integrate migrant populations initially admitted with the status of temporary residents. Gary Freeman examines the foreign policy and trade considerations behind Australia's decision to open migration to non-Europeans and then to opt for an explicit multicultural policy.

Part 2 explores the security implications of international migration for the Third World, where the majority of population movements occur. Papers by Myron Weiner on South Asia, Astri Suhrke on Southeast Asia, Karen Jacobsen and Steven Wilkinson on sub-Saharan Africa, and William Stanley on Central America examine the sociopolitical conditions that generate population movements, the consequences for sending and receiving states, and the policy responses. Weiner writes that until recently, throughout the Third World, borders either did not exist or were not clearly demarcated, and people moved freely without regard for national boundaries or legal conceptions of citizenship. Part 2 argues that with the mass exodus of peoples from many developing countries and the high costs these migrations impose on receiving countries, many Third World states, particularly those with precarious ethnic balances or those caught up in regional conflicts, are closing their borders and are more clearly defining their residents on the basis of citizenship.

Part 3 analyzes the political role of ethnic diasporas, the millions of people who now live in a country other than the one in which they were born yet retain loyalty to their places of birth. Gabriel Sheffer and Yossi Shain examine how these new transnationals are mobilized, what their specific roles are for sending and receiving countries, and what the implications are for the conduct of interstate relations.

An important point made by several of the contributors to this volume is that contrary to popular belief, population movements are often welcomed and can, in fact, strengthen both sending and receiving countries. Australia purposively expanded migration from Asia in order to attract educated and entrepreneurial migrants with ties to businesses in Asia and to increase trade from that region. In Central America, sending countries like El Salvador, Guatemala, and Nicaragua have benefited from significant remittance flows from their migrants in the United States, and these foreign-exchange earnings helped stabilize the economies of all three of these war-torn countries. In Africa, refugees have in some cases served to enhance the host country's security either through the incorporation of refugees into their armies or by the in-flow of international refugee assistance and aid in the form of foreign exchange or infrastructural benefits. Most past research on international migration and security has focused on the negative impact of migration on interstate relations. Analyses of the benefits of population movements and their contribution to security should be given more attention in future studies.

Although the regional breadth of International Migration and Security is impressive, some important regions are not covered. For example, the Middle East, North Africa, and East Asia (regions in which migration and societal stability are closely intertwined and becoming increasingly important) are not examined in this book. The Middle East, in particular, is a region where there have been recent mass expulsions of national minorities and migrant workers (e.g., to Yemen and to Jordan from Iraq, Kuwait, and Saudi Arabia) and where migrants have played a crucial role in the economies and, to a certain extent, the politics of the oil-producing states. Similarly, it would have been useful to have concluded

the book with an integrative chapter examining the implications of the links between population movements and security for the operations and present capacities of international and regional security and humanitarian institutions and mechanisms.

This collection of papers, along with the contributions of a few other scholars, have begun to map out an important and previously neglected area of international relations research. What is required in the future is not only more comparative work examining the interrelationships between migration and societal security but also more imaginative inquiry into what sorts of policies, institutions, and coalitions are needed both to maximize the positive contributions that migrants make to national, regional, and international stability and to create international order in a world in which there will continue to be large-scale population movements in the years ahead.

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GIL LOESCHER

Justice and the Genesis of War. By David A. Welch. New York: Cambridge University Press, 1993. 335p. \$49.95.

The central claim of this book is that states want justice as well as power. Welch explores the role of what he terms the justice motive, defined as "the drive to correct a perceived discrepancy between entitlements and benefits" (p. 19), in the genesis of five wars over the last century-and-a-half. The author's aim is not to develop or test a general theory of war causation. Rather, it is to show that the justice motive has been an important-in some cases, decisive-influence leading to interstate war and, in so doing, to bolster the case that the justice motive plays an important role in international affairs generally. This inference leads the author to speculate, in the concluding chapter, on how the justice motive might be harnessed to the cause of constructing a peaceful world order.

The book's primary target is political realism, which, in the author's view, provides an incomplete, if not distorted, conception of human nature. Realism assumes that all states are motivated by self-preservation and self-aggrandizement, which can be fully captured by a singular cost-benefit calculus. Against this view, the author argues that policymakers respond not only to "aversion to loss" and "appetite for gain" but to a desire to see entitlements fulfilled or respected. The motive for justice is of a different character than the motive for self-aggrandizement, in the author's view, not least because "the mode of reasoning involved in the defense of one's entitlements differs fundamentally from the mode of reasoning involved in the pursuit of other goods: it tends to be categorical and deontological rather than utilitarian" (p. 21). Realism, in the author's view, fails to recognize the distinctive character of the justice motive (if it recognizes this motive at all) and thus offers an inadequate basis for understanding either the genesis of war or the possibilities for peace.

Welch assesses the contribution of the justice motive to the outbreak of five conflicts: the Crimean War, the Franco-Prussian War, World War I, World War II, and the Falklands/Malvinas War. Because these are Great Power wars, the author argues, realism would suggest

that these conflicts are motivated primarily (if not solely) by power and material interest. As such, they constitute "crucial cases": if the justice motive is found to be influential in these episodes, realism is thrown into general doubt. In each case study, the author is concerned to characterize both the motive's intensity (relative weight vis-à-vis other motivations) and agency (role in triggering the conflict). The author carefully explains his methodology and forthrightly addresses a number of difficult questions regarding research design. He concludes that the justice motive played an important role in all but one of the five conflicts (the Franco-Prussian War). In the last of the wars surveyed—that pitting Argentina against Great Britain in 1982—the author finds that the justice motive alone conditioned and

triggered the conflict.

Welch's characterization of realism as a theory of human motivation is unusual. Readers who prefer to see in realism a theory of international politics (rather than foreign policy) and an explanation of recurrent political patterns (rather than singular events) may be unmoved by the author's assault on the assumptions about human nature underlying such theory. This is particularly likely where such assumptions are defended in terms of their explanatory utility, rather than their truth, and where parsimony is given pride of place in the assessment of competing theories; for Welch's account of motivations necessarily implies a messier, more detailed, more historically sensitive theory of war than does the realist account he aims to correct. But such objections to Welch's work would be misplaced; for what is most valuable in his book is not his theory of war causation (which, in any case, is, as he points out, only partially developed) but his insistence that a more adequate understanding of human motivation is necessary if we are to understand fully the challenges of building a just and peaceful world order. In the last chapter, "Justice and Injustice in a Global Context," the author argues against universalistic formulations of international justice and in favor of a regime-based "patchwork" conception of justice capable of dealing with the actual claims made by states in concrete circumstances. These normative arguments are based on the empirical analyses of motivation developed in the first six chapters. The interplay of these chapters is meant by the author to break down the barriers to communication across the "empirical-normative gap" in political science: "Explanation and prescription need not remain forever in their present artificial isolation" (p. 218).

The book is clearly written, soundly organized, and judiciously argued. The case studies are balanced and concise. While one might disagree with the author's characterization of realism as a theory of motivation, the book's contribution to our understanding of state motivations is nonetheless valuable. Most important may very well be the author's move to bridge the gap between normative theory and empirical analysis in the study of war. Certainly, the question of what institutions might best promote a just world order requires more than the single chapter the author here can devote to its discussion. But too many empirical studies of war have ignored this critical issue. Toward its clarification, Welch's book is an important first step.

College of William and Mary

DAVID DESSLER

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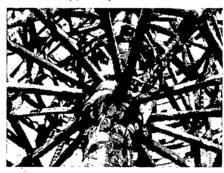
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